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ERRORS IN THE MEMORIZATION OF NUMBERS

By MILDRED B. MITCHELL, George School, Pa.

The purpose of this experiment was to determine the various types of errors that occur in the memorization of lists of three-place numbers, and the frequency of their occurrence.[1]

Few studies of this type have been made. Hamilton's study is a notable exception.[2] He used the multiple choice method with both human and infra-human subjects, and found it possible to classify their errors into 5 types. While there was a great deal of overlapping and individual variation, the frequency of the different types of errors tended to vary with the animal's position in the phyletic scale. Experimenters working in memory have made various provisions in scoring for different types of errors. For example, Robinson,[3] using four-place numbers in a memory experiment with human subjects, took into consideration the source of the response when incorrect. If the four-place number given was the next number in the list, it counted as one error only, while if it was more than one place removed from its correct position it counted as four errors.

While conducting an experiment on retroactive inhibition under hypnosis, the present writer observed that frequently in giving a three-digit response, the subject would give one digit correctly, and two digits from the next number.[4] While these were not always in their correct position within the three-place numbers, they were observed to be so more frequently than it seemed possible to attribute to chance alone. Position and the next number were observed, therefore, to be important factors in the response. As a result of these observations, and the suggestion of

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* Accepted for publication October 15, 1931.

[1] From the Psychological Laboratory of Yale University. This study was conducted under the direction of Professor Edward S. Robinson.

[2] G. V. Hamilton, A study of perseverance reactions in primates and rodents, *Behav. Monog.*, 3, 1916, (no. 13), 1-65; A study of trial and error reactions in mammals, *J. Animal Behav.*, 1, 1911, 33-66.

[3] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, (no. 6), 18.

[4] M. B. Mitchell, Retroactive inhibition and hypnosis, *J. Gen. Psychol.*, 7, 1932, 343-359.

types of errors in learning obtained from Hamilton, as well as the importance of position obtained from Robinson, a record of the responses was made during an experiment on retroactive inhibition using waking suggestion.[5]

In the previous experiments[6] merely plus or minus records of the errors were recorded, but in the present experiment, the entire response of the subject was taken if it was incorrect in any respect. The errors were then classified according to the number of digits given in the response and analyzed with respect to number of correct digits, position of these digits, and relation to the next number in the list.

APPARATUS, MATERIALS, AND SUBJECTS

Apparatus. The memory drum used in this experiment was designed by McGeogh.

Material. Lists of 10 three-place numbers were used as material in this experiment.

Subjects. The *S*s were all Yale students. Five were undergraduates and six were graduate students, all from departments other than psychology.

METHOD AND PROCEDURE

The anticipation method of learning was used. *S* was seated in a comfortable armchair in front of the apparatus. He was told that the experiment consisted of learning lists of 10 three-place numbers. He was then given a typewritten sheet with a verbal request to read the following instructions for learning the numbers.

> During the *first* trial you will study each number as it appears. In the succeeding trials, try to give the number aloud before it appears in the slit. Give each digit separately; *i.e.* give 123 as one, two, three, not as one twenty-three. There will be no time between successive trials. The last number will become your cue for the first number of the list. Try to anticipate as many as you can each trial. You will be through when you have anticipated the entire list correctly. Do you understand? No questions will be answered once the experiment is started.

The instructions were then expanded verbally until *S* was sure he knew what was expected of him.

E sat at a table dorsal-laterally to *S*, out of his field of vision but in view of the exposed numbers.

Every *S* was given a practice period of from 8 to 11 days depending upon his speed of memorization. The usual procedure was to have *S* learn 2 lists each day with a short rest period between. All the experimenting was done between October 27, 1930 and December 17, 1930. Every *S* was given the series at the same time of day, five days a week.

The *S*s learned a list of 10 three-place numbers; after a 40-sec. interval they learned a second list of numbers, and then after another 40-sec. interval they relearned the first list. Preceding each list, *S* was given suggestions regarding the efficacy of retroactive inhibition. In Condition A, he was given the direct suggestion that he *would not* be able to remember the first list after learning the second list. In

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[5] M. B. Mitchell, Retroactive inhibition and waking suggestion, *J. Abn. Psychol. & Soc. Psychol.*, 27, 1932, 336-341.

[6] Mitchell, *op. cit.*, *J. Gen. Psychol.*, 7, 1932, 343-359.

Condition B, he was given the direct suggestion that he *would* be able to remember the first list after learning the second list. A cycle was arbitrarily considered as 8 sessions with the 2 conditions given in the order ABBAABBA.

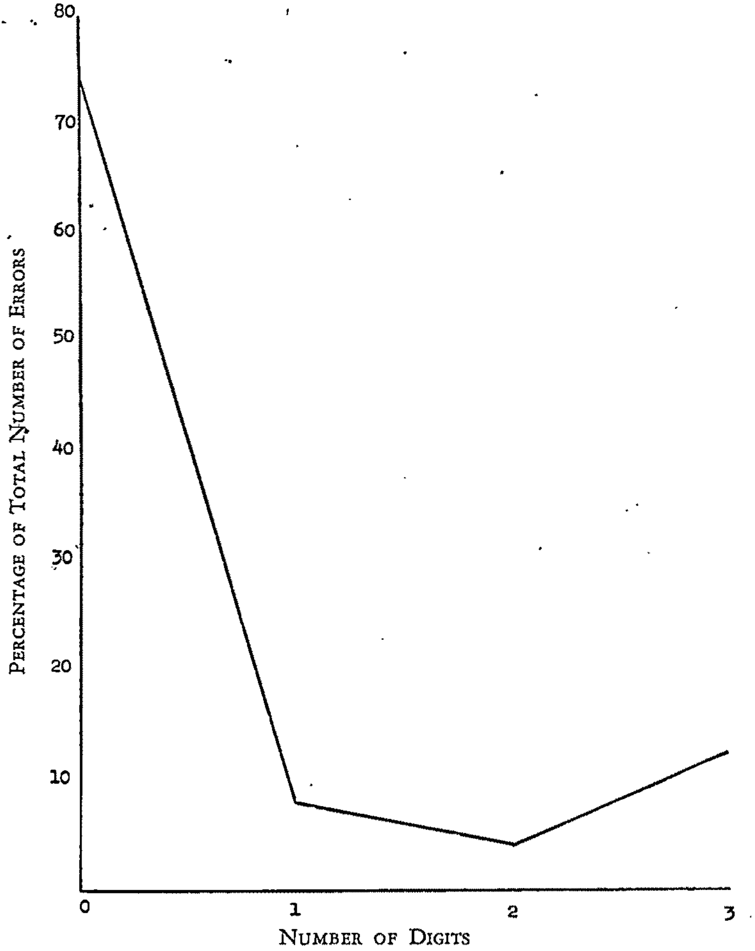

FIG. 1. GENERAL TYPE OF ERRORS FOR ALL *S*s, ALL CYCLES, AND ALL LISTS (30,092 cases)

RESULTS

Classification of the errors. In anticipating a 3-place number *S* may make no verbal response, or may give one, two, or all the digits. Unless he gave all the digits in their correct order during the 2-sec. interval preceding their exposure, the trial was recorded as an error.

The percentage of each general type of error, classified according to the number of digits in the response, is shown in Fig. 1. These results are based on 30,092 errors. As Fig. 1 shows, there is a preponderance of 0-digit responses with the incorrect 3-digit responses ranking second in frequency. The 1-digit responses rank third and the 2-digit responses are least frequent of all.

Table I gives a summary of the two conditions for all *S*s in all lists, and all cycles. The gross number of errors for Condition B is 99.5% of those for Condition A, or 15,011 as compared with 15,081. The summary for each type of response is also practically identical as is shown most clearly by comparing the percentage of each type of error found under the two conditions. The greatest difference, 0.9% is found in the 1-digit responses. The percent of imperfect 3-digit responses is identical, 12.8%, for the two conditions. The reliability of the method is consequently assured—the statistical validity of the complete data is established by the equality of the two halves of the data. Henceforth, it will not be necessary to consider the two conditions separately.

TABLE I

SHOWING FOR EACH CONDITION THE NUMBER AND PERCENTAGE OF ERRORS MADE BY ALL THE *S*S, IN ALL THE LISTS, DURING ALL THE CYCLES

Condition	Errors	No. Digits				Total no. errors
		0	1	2	3	
A	No.	11,262	1202	680	1931	15,081
	%	74.7	8.0	4.5	12.8	
B	No.	11,092	1341	652	1926	15,011
	%	73.9	8.9	4.3	12.8	

Our next concern was with the errors made during the learning of the various lists, *i.e.* during the original learning of List 1, the learning of List 2, and the relearning of List 1. As Table II shows, the errors for the learning of List 2 and the relearning of List 1 are practically identical. The original learning of List 1 shows a few less 0-digit responses with a correspondingly greater number of imperfect 1-, 2-, and 3-digit responses. The total number of errors for List 2 is slightly greater than for the original learning of List 1, and the percentage of 0-digit responses is nearly 4% greater. On the whole, however, there is very little difference in either the number or type of errors found in the different lists,[7] so that the results from all lists may be combined in making comparisons between cycles and *S*s.

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[7] A more detailed account is soon to appear in the *J. Exper. Psychol.* (Feb. 1933).

Our results thus far indicate that the relative percentages of the various types of errors is practically constant whether comparisons are made between conditions or lists. The next question was concerned with the effects of practice. This can most readily be examined by examining the results by cycles. A complete record was kept for the practice periods of 5 of the *S*s. Each of these *S*s learned from 16 to 20 lists during the practice period, and 24 lists (including the relearning) during the first cycle. The top part of Table III is based on the 12,215 errors in the practice period and the 9,615 errors made in Cycle 1. With increase in practice there is a relative increase in the number of 1- and 2-digit responses, especially 1-digit responses, with a corresponding decrease in 0-digit responses. The same thing holds true for the three cycles run by the other 6 *S*s (lower part Table III). Cycle 1 for the 5 *S*s shows a higher percentage of 1-digit than 3-digit responses. For none of the three cycles is that true in the case

TABLE II

SHOWING FOR EVERY LIST THE NUMBER AND PERCENTAGE OF ERRORS MADE BY ALL THE SS DURING ALL THE CYCLES

List	Errors	No. Digits				Total no. errors
		0	1	2	3	
1	No.	8,099	1069	561	1549	11,278
(original)	%	71.8	9.5	5.0	13.7	
2	No.	8.970	949	507	1438	11,864
	%	75.6	8.0	4.3	12.1	
1	No.	5,285	525	269	871	6,950
(relearning)	%	76.0	7.6	3.9	12.5	

TABLE III

SHOWING FOR EACH CYCLE THE NUMBER AND PERCENTAGE OF ERRORS MADE BY THE SS IN ALL THE LISTS

Cycle	No. Ss	Errors	No. of Digits				Total no. errors
			0	1	2	3	
Practice	5	No.	9807	597	421	1390	12,215
		%	80.3	4.9	3.4	11.4	
1	5	No.	6692	1289	456	1178	9,615
		%	69.6	13.4	4.7	12.3	
1	6	No.	7103	280	299	1127	8,809
		%	80.6	3.2	3.4	12.8	
2	6	No.	4566	492	306	872	6,236
		%	73.2	7.9	4.9	14.0	
3	6	No.	3993	482	276	681	5,432
		%	73.5	8.9	5.1	12.5	

of the other 6 *S*s. On the whole, these 5 *S*s were slower in learning, *i.e.* made more errors, than the other 6. The further significance of this fact will be seen when the *S*s are grouped according to speed of learning or number of errors.

The increase in the number of 1- and 2-digit responses with practice for the group of 6 *S*s is more apparent than real. It is only the slower *S*s who are making these changes while the faster *S*s are keeping their relative frequencies practically constant, as may be seen in Table IV. With practice the slow *S*s, instead of becoming more like the faster, tend to learn still more by *parts,* thus accentuating the differences between the two groups, and causing an apparent increase with practice in the total number of 1- and 2-digit responses for all *S*s.

TABLE IV

Showing for each Cycle the Number and Percentage of Errors Made in all Lists by *Bri*, the Fastest *S*, and by *Cob*, the Slowest *S* in the Group of 6 *S*s

S	Cycle	Errors	No. of Digits				Total no. errors
			0	1	2	3	
Bri	1	No.	826	17	15	186	1044
		%	79.1	1.6	1.4	17.8	
	2	No.	673	14	7	150	844
		%	79.7	1.7	.8	17.8	
	3	No.	663	12	9	103	757
		%	83.6	1.6	1.2	13.6	
Cob	1	No.	1440	104	84	171	1799
		%	80.0	5.8	4.7	9.5	
	2	No.	802	187	98	145	1232
		%	65.1	15.2	8.0	11.8	
	3	No.	642	219	97	114	1072
		%	59.9	20.4	9.0	10.6	

Bri held first place for speed of learning and lowest number of errors in all three cycles. For the 6 *S*s run for three cycles, *Cob* was just as consistently the slowest, and made the most errors. The relative number of 1- and 2-digit responses does not change with practice for *Bri* but stays practically identical throughout (Table IV). The relative number of 1- and 2-digit responses steadily increases with practice for *Cob* while the 0-digit responses steadily decrease (Table IV). In the second and third cycles the number of 1-digit responses exceeds the number of 3-digit responses. This is the same phenomenon already found with practice in the relatively slow group of 5 *S*s (Table III).

There are large individual differences in the total number of errors made and in the relative frequency of each type of error. For instance, the three slowest *Ss* each have more than twice as many errors as the fastest *S. Bri* has very few 1- and 2-digit responses (Table V) while the slowest *S, Hic,* has almost as many 1-digit as 3-digit responses.[8] When the 5 *Ss* below the median speed are compared with the 5 above, the same holds true (Table V). There are relatively more 1- and 2-digit and less 0- and 3-digit responses for the slow group than for the fast. Apparently the slower *Ss* are learning the three-place numbers by *parts* so that they give more 1- and 2-digit responses than the more successful memorizers; the latter tend to give 3-digit or 0-digit responses almost exclusively. They seem to be learning the numbers as *wholes.* With the fastest *S, Bri,* this remained true even after much practice. A slower *S, Cob,* on the contrary, with practice tended to give relatively fewer 0-digit responses, to speak forth the one and two digits he had learned, (Table IV). Hence practice tends to accentuate the earlier individual differences in method of learning numbers.

TABLE V

SHOWING THE DIFFERENCES BETWEEN THE SLOW AND FAST *Ss* IN THE NUMBER AND PERCENTAGE OF ERRORS MADE IN ALL LISTS DURING CYCLE 1

Ss	Errors	No. of Digits				Total no. errors
		0	1	2	3	
Bri (fastest)	No.	826	17	15	186	1,044
	%	79.1	1.6	1.4	17.8	
Hic (slowest)	No.	1704	252	110	298	2,364
	%	72.1	10.7	4.7	12.6	
Fast Group	No.	5277	160	201	1052	6,690
	%	78.9	2.4	3.0	15.7	
Slow Group	No.	7498	1051	480	1153	10,182
	%	73.6	10.3	4.7	11.3	

In general, the most frequent response by far of all the imperfect responses is the 0-digit response with the 3-digit response holding a distant second place. On the whole, the 2-digit response is least frequent of all. In *Ss* who learn very rapidly and apparently by the whole method, the 1- and 2-digit responses are, however, almost the same in frequency. On the contrary, with *Ss* who learn more slowly, the 1-digit responses approach the 3-digit responses in frequency and this difference is accentuated with practice. Of course these differences are for small highly selected groups and might not hold for the population in general.

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[8] The *Ss* with the fewest errors learned the most rapidly, and vice versa. There was a correlation of 0.92 between the total number of trials to learn the lists and the total number of errors in the first cycle for the 10 *Ss* used in comparing slow with fast *Ss*. There was practically no difference in overlearning scores; the slow group had a mean score of 79.1 while the fast group had a mean score of 79.3. There was only the slight positive correlation of 0.28 between the overlearning scores and number of errors.

Analysis of 1-digit responses. In giving a 1-digit response there are always 10 possibilities. By pure chance or guess work any one of the 10 digits may be given. Since there are three digits—all different—in each number, the probability of getting one of these, that is, of making a correct 1-digit response is 3 out of 10 or 30%. If for convenience of illustration the excluded numbers 456 and 789 be considered the correct and next number respectively, then the probability of an *S* giving 4, 5, or 6 is 30%. The chance that the digit given is not only correct, but also in the correct position, that is, the first digit in the number (as 4 in the example), is one out of 10 or 10%. In other words, the probability that the correct 1-digit response will be in the wrong position (*i.e.* will be 5 or 6), is 2 out of 3 or 66.7%. There were a total of 2543 1-digit responses made by the 11 *S*s during the experiment. Out of these 1939 or 76.2% were correct, while as stated above, only 30% would be correct by chance alone. Furthermore, only 258 or 10.1% of these were in a wrong position while 66.7% might have been had chance alone been operating. This is 56.6% less than chance would give. Thus in about half of the cases, *S* actually knew the correct response is so far as he went and did not get it correct by chance alone. He had learned a *part* of the correct response.

Of the incorrect responses, the probability of being one of the three digits in the next number (as 7, 8, or 9) is again 3 in 10 or 30%. Out of the 604 incorrect responses, 420 or 69.5% were from the next number (Table VI). This is more than twice the probability from pure chance. Instead of 66.7% of these being in the wrong position only 25.2% were other than the first digit in the next number (as 7 in the example given above).

In all such analyses there is some factor other than chance operating that gives responses identical with those in the next number. Of course, the three correct digits are ruled out, for if they are given, the response would not be among those considered here. A further complicating factor is that the preceding number is exposed at the time the response is given. Its digits might tend to act as a positive suggestion to give one or more of them, or on the other hand, they might act as a negative suggestion to avoid giving them. The determination of these factors is a problem in itself that cannot be considered in detail here, but might be determined from the data of this experiment.[9] Apparently, these factors are more or less balancing each other or at least are not all important factors for the numbers are not only frequently identical with those in the next number, but are also in the correct position within the next number.

A comparison of the 5 *S*s who learned most slowly shows about 10% more correct responses for the slow group than for the fast and about 6% less position errors. The influence of the next number was the same in both cases, about 69%. These same tendencies are seen in an exaggerated form when the fastest and slowest *S*s (for all the three cycles) are compared. Here the slowest has about 20% more correct responses and 10% less position errors. In addition, the source of the incorrect responses in the next number is found in 23% more cases in the slowest *S* than in the fastest. Evidently, the slower *S*s actually are learning the numbers a digit at a time and so are more successful in their 1-digit responses. The faster *S*s may either be guessing or starting to give a longer response and stopping as they realize they have made an error.

TABLE VI

AN ANALYSIS OF 1-DIGIT RESPONSES FOR ALL *S*s IN ALL LISTS DURING ALL CYCLES
(2543 Cases)

	Correct	W.P.*	Incor.	Next no.	W.P.*
No.	1939	258	604	420	106
%	76.2	10.1	23.8	16.5	4.2
% W.P.*		13.3			25.2
% n†				69.5	

* Wrong position, means that the digit is correct but in the wrong position within the number and when heading a column indicates the number of position errors among the responses in the preceding column.

† The % of the digits given incorrectly that are found in the next number.

It may be concluded that there is some evidence for digits being learned separately especially by the slower subjects since 1-digit responses are more frequently correct than chance would lead one to expect. Their position is learned at the same time because the number of position errors is much less than chance alone would allow. Furthermore, the next number is an important source from which incorrect 1-digit responses are obtained.

Analysis of 2-digit responses. In making a 2-digit response there are three gross possibilities regarding its degree of correctness: both digits may be correct, one digit may be correct, or both may be incorrect. Any 2-digit response containing 4, 5, or 6 as one of its digits would be of the type of 1-digit correct, while all others would be of the 0-digit correct type. Taking all *S*s for the entire experiment, 45% of the 2-digit responses were correct so far as digits alone were concerned. (See Table VII). Thus if only two digits were given, the chances were about even that at least one of these digits would be incorrect. As a matter of fact, of all the 2-digit responses one of the digits was correct nearly as frequently as two of them, while only a relatively small percentage had both digits incorrect.

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[9] An actual analysis of all the responses with respect to the preceding number (the one exposed) showed approximately a chance appearance of these digits; hence they were not avoided.

When the 5 slow *S*s are compared with the 5 fast *S*s for the first cycle, the rank order for the number of correct digits is the same as for all *S*s taken together in Cycle 1. The faster *S*s give slightly fewer 2-digit correct and more 1-digit correct responses.[10] This is in accordance with the hypothesis set forth by the writer in the previous sections; namely, that the slower *S*s are learning the numbers by parts and hence have more of their partial responses correct.

TABLE VII

An Analysis of 2-Digit Responses for all *S*s in all Lists During all Cycles
(1337 Cases)

	2 cor.	W.P.	1 cor.	W.P.	1 in next no.	W.P.	2 incor.	2 in next no.	W.P.	1 in next no.	W.P.
No.	601	249	568	119	298	152	168	125	20	31	12
%	45		42.5				12.6				
% W.P.*		41.4		21		51			16		38.7
% n †					52.5			74.4		18.5	

* Wrong position and when heading a column indicates the number of position errors among the responses in the preceding column.

† Responses from the next number.

The permutations of ten digits taken two at a time is 90; therefore, there are 90 possible 2-digit responses.[11] The permutations of three digits taken two at a time is 6; therefore, there are 6 correct 2-digit responses possible as illustrated above. In other words, the probability of getting a 2-digit response correct by mere chance is 6 in 90 or 6.7%. Actually, however, out of the 1337 2-digit responses (Table VII) 601 or 45.0% were correct. Of the 90 permutations only one can be correct with respect to position as well as digits. Hence of the 6 correct responses only 1 in 6 (45 in the example where the correct number is 456) would have the correct position by chance; in other words, 83.3% could be expected to have a wrong position. Only 41.4% or less than one-half that number were in the wrong position.

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[10] Here as throughout this paper comparisons are in terms of percentages not in actual numbers.

[11] Not counting responses in which one digit is repeated. These practically never occurred probably partially due to the fact that the numbers to be learned never contained a repetition of digits.

The case of one digit being correct here is the same as when it is the only digit given. Instead of obtaining 30.0% correct, there were 42.5%; hence, there was slightly more than a chance success. In order to avoid a position error the first digit would have to be given as the first one of the pair, or the second one as the second digit of the pair. For instance, in the example 456, 42 would have the first digit correct and in the correct position, while 15 would have the second digit correct and in the correct position. Instead of 66.7% position errors only 21.0% were obtained.

If chance should be credited with giving only 6.7% 2-digits correct responses and 30.0% 1-digit correct responses, it would leave 63.3% with 0-digits correct. In contrast with this, only 12.6% fall within this classification.

With 1-digit correct, the probability is 30.0% of the other digit being one of the three digits from the next number (7, 8, or 9 in the example), but 52.5% were from the next number. 51.% of these were in the wrong position out of a probable 66.7%. Here again the differences are slightly greater than chance.

The largest variations from chance come when two of the incorrect digits are from the next number in the list. The probability is only 6.7%, but 74.4% of these incorrect responses are both correct for the next number. A large variation is also found with respect to the position of the digits. Thus while the probability of getting them in the wrong position is 83.3%, only 16.0% were incorrectly placed.

The 2-next-number responses were so far from chance that the expected 30% probability of getting 1-digit from the next number was made impossible, but 18.5% of the cases did appear with one digit from the next number with 38.7% instead of 66.7% of these digits in an incorrect position.

In general, when 2-digit responses are given by the *S* at least half of them contain an error. Most of these contain only one incorrect digit half of which come from the next number. Of the 2-digit incorrect (0-digit correct) responses about three-fourths have both digits from the next number and most of them are in the correct position.

Analysis of 3-digit responses. In a 3-digit response, there are 4 possibilities with respect to the number of digits correct. All of the digits may be incorrect, one, two or three of the digits may be correct. The 3-digits correct are composed of those responses with the digits in the wrong position and those responses given correctly but too slowly. The relative frequency of these types of errors for all *S*s, all lists, all cycles, may be seen in Table VIII. Nearly half, 44.5%, of all the 3-digit responses (not including the perfect responses) contain two correct digits; 3-digits correct ranks second with a frequency of about half as many. The 0- and 1-digit correct responses are both relatively few in number and about equal with a slight difference in favor of the 1-digit correct responses.

TABLE VIII

AN ANALYSIS OF 3-DIGIT RESPONSES FOR ALL *S*S IN ALL LISTS DURING ALL CYCLES
(3858 Cases)

	Too slow	W.P. in list	n. no. in list	Digits W.P.	0 cor.	1 next no.	W.P.	2 next no.	W.P.	3 next no.
No.	615	800	477	440	504	27	16	52	25	411
%	15.9	20.7	12.4	11.4	13.1	.7	.4	1.3	.6	10.7
% W.P.				41.7			59.3		48.1	
% n			62.4			5.4		10.3		81.5

	W.P.	1 cor.	W.P.	1 next no.	W.P.	2 next no.	W.P.	2 cor.	W.P.	1 next no.	W.P.
No.	16	581	194	211	123	189	65	1718	481	825	425
%	.4	15.1	5.0	5.5	3.2	4.9	1.7	44.5	12.5	21.4	11.0
% W.P.	3.9		33.4		58.3		34.4		28.0		51.5
% n				36.3		32.5				48.0	

Just as formerly we have found significant differences when we have compared the slower with the faster *S*s, so here there is also a difference. While the 0- and 1-digit correct responses remain relatively low for both groups, they are lower for the slow group. There is a decided increase for 2-digits correct for both groups, but it also is lower for the slower group. The most marked difference comes in the 3-digit correct responses. For the fast group, there is a huge drop, down to below the number of 1-digit correct responses. In contrast to this, the slow group have even more 3-digit correct responses than two or any other. The ranking of the various types is almost completely disrupted, although the 0- and 1-digits correct remain comparatively low and the 2-digits high in both cases. These same differences are merely exaggerated when the slowest and fastest subjects are contrasted.

In the majority of cases when 0-digits are correct, the number is merely displaced within the list (W.P. in list, Table VIII). There were 800 such misplacements.[12] Since there were nine numbers each of these might have been, the probability by mere chance of their being the next number in the list was only 11.1%, but 62.4% were found to belong there. This same influence will be seen again when the total number of 3-digit responses with no correct digits is analyzed.

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[12] These are further analyzed under the title of "Anticipatory place-skipping tendencies in the memorization of numbers" which is soon to appear in this JOURNAL.

If no digits are correct, one, two, or three of them may come from the next number. The mere mathematical probability of obtaining digits from the next number would lead one to expect more with one from the next number than two, and more two than three since the probabilities are 30%, 9% and 0.1% respectively. However, the reverse is found to be true with only 5.4% of these responses with one, 10.3% with two, and 81.5% with three digits from the next number. The position errors were also reversed from the mathematical probability; they decrease rather than increase with the number of digits involved. For one digit in the next number 59.3% were in the wrong position which is a little less than 66.7%, the chance allotment. For two digits in the next number there were only 48.1% instead of 83.3% in the wrong position. For three digits in the next number only 3.9% out of a probable 99.2% were in the wrong position.

As stated in the introduction, while doing some experimenting on hypnosis using retroactive inhibition with lists of numbers, the writer noticed the *S*s giving one digit correct with two digits from the next number, or two digits correct with one digit from the next number. The digits were not always in their correct position, but were observed to be so more frequently than could be attributed to chance. The next few paragraphs give a summary of the quantitative findings which confirm these observations.

In the cases in which one digit out of the three given was correct, there were only half as many position errors as could be expected, 33.4% out of a probable 66.7%. Chance could be held responsible for the 36.3% of single digits found in the next number. The actual number of 3-digit responses with two digits from the next number was nearly the same, 32.5%, but in this case the probable percentage was only 9; therefore, something more than chance is needed to explain the finding. Similarly, it takes more than chance to explain that only 34.4% of these digits were in the wrong position since chance would allow 83.3%. On the other hand, when only one digit came from the next number, it was mere chance if it were in the right position. There was, therefore, a tendency to obtain responses of one digit correct with two digits from the next number.

In the cases where two digits were correct, only 28.0% out of a probable 83.3% were in the wrong position. Practically half of the time, (48.0%) the remaining digit was from the next number although only

30% could be attributed to chance. Half of the time (51.5% wrong position) it was also in the correct position although chance would only account for it one-third of the time. There was, therefore, a tendency to obtain responses of two digits correct, with respect to position as well as digits, with one digit in the correct position from the next number.

With a retroactive inhibition set-up such as that used in this experiment, it would seem reasonable to expect some of the responses in the learning of List 2 and the relearning of List 1 to be numbers from the list just learned. In learning List 2, 7.6% of the incorrect 3-digit responses were numbers from List 1. In the relearning of List 1, 13.5% of the incorrect 3-digit responses were numbers from List 2. The difficulty in evaluating the influence of a previously learned list is the fact that with only 10 digits there is so much repetition within two lists that no adequate account can be taken of any responses not given completely. For instance, any two digits may have occurred several times within the same number in either list; it would, therefore, be impossible to say that the response was due to the number in the previous list.

It is of some interest in view of the similarity theory of retroactive inhibition as expounded by Robinson[13] and Skaggs[14] to note the number of digits correct in the responses taken over bodily, as it were, from the preceding list. Of the 227 responses of this kind, 119 or 52.4% of them contained two correct digits (Table IX) while only 8 or 3.5% had no correct digits. This is in decided contrast with the 13.2% of 0-digits correct found among all the 3-digit responses for these two lists. There was almost twice as large a percentage of 1-digit responses correct for preceding-list errors as for all such responses for these lists. The apparent

TABLE IX

Showing the Number and Percentage of Errors for Each Number of Digits Correct in all the 3-Digit Responses and Those from the Preceding List During all Cycles for List 2 and Relearning of List 1

3-digit responses	Errors	No. Digits Correct				Total no. errors
		0	1	2	3	
From Preceding List	No.	8	72	119	28	227
	%	3.5	31.7	52.4	12.3	
Total	No.	305	376	1021	607	2309
	%	13.2	16.3	44.2	26.3	

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[13] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.* 28, 1920, (no. 128), 1-57; The similarity factor in retroaction, this Journal, 39, 1927, 297-312.

[14] E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.*, 34, 1925, (no. 161), 1-60.

differences in the 3-digits correct is due to the fact that the "too slow" responses are included in the 607 3-digit correct responses. When these are excluded the percentage is reduced to 10. There was no possibility of a large frequency of the 3-digits correct from the preceding list because it involved the relatively rare phenomenon of having numbers in two succeeding lists containing the same three digits. From these tabulations, it can be stated that when a response identical with a number in the preceding list is given, that number is almost always partially identical with the correct number and in half of the cases it has two out of three digits identical.

SUMMARY AND CONCLUSIONS

The results as indicated in the preceding sections may be summarized as follows.

(1) The most frequent type of error in learning lists of 10 three-place numbers is to give no verbal response, that is, a 0-digit response. The 3-digit response holds a distant second place with the 1-digit response ranking third and the 2-digit response least frequent of all. This is true even after having just learned another list and for the relearning of a list. Practice tends to increase the relative frequency of 1-digit responses and correspondingly decrease the number of 0-digit responses. For slow *S*s this tendency is so marked that the 1-digit responses sometimes become more frequent than the 3-digit responses, but this is not true for the fast *S*s.

(2) The numbers are sometimes learned by digits especially by the slower *S*s since more correct 1-digit responses are given and given in the correct position than can be attributed to chance.

(3) Approximately half of the 2-digit responses contain one incorrect digit. This incorrect digit comes from the next number about half of the time. The relatively few cases with both digits incorrect usually have the next number as their source.

(4) Considering all the *S*s, 3-digit responses have 2 of the digits correct more frequently than 0, 1, or 3. When the fast *S*s are considered separately this is true in an exaggerated form but when the slow *S*s are considered alone, the 3-digits correct are the most frequent. There is a tendency for the 1-digit correct responses to be completed with two digits from the next number, and for 2-digit correct responses to be completed with one digit from the next number. A number from a list just previously learned is practically never given unless it is partially identical with the correct number, and most frequently has two digits in common.

The fact that the *S*s who learned most rapidly tended to give zero or three digits rather than one or two digits suggests the possibility of improving speed of memorization in the slower *S*s by giving explicit directions about learning the number as a whole rather than learning and reciting it by parts. This might be determined experimentally by varying the instructions for memorization.

All *S*s tend to give the digits in their proper sequence even when they are not correct but belong in the next number. The tremendous influence of the next number found throughout all the verbal responses may be described as a form of retroactive inhibition within the list. The *S*'s chances of remembering a particular three-place number exposed to him for two seconds at a time seems to be lowered when he is shown a similar three-place number immediately following the exposure. It may also be described as a forward association within the list or a place-skipping association.

THE FACILITATIVE AND INHIBITORY EFFECTS OF MUSCULAR TENSION UPON PERFORMANCE

By G. L. **Freeman**, Northwestern University

To critical students, it is now quite evident that the relationship between mental activity and muscular tension is far from simple. One of the most perplexing problems of contemporary research concerns the universal validity of muscular reënforcements. Data obtained under widely different conditions indicate that increments in tension may (1) increase efficiency of performance, (2) have no effect on it, or (3) be actually inhibitory. Such apparently contradictory results point to the necessity of further work, both with more refined techniques and with more rigid experimental controls.

Our general problem has thus been formulated as follows: When may muscular tension facilitate performance and when is it likely to be inhibitory? This paper describes several new approaches to the question and attempts to clarify the interpretation of data obtained from other sources. Since the terms facilitation and inhibition have both a general and a technical meaning, it is advisable to mention at the outset which concept is to be used. According to the general view all factors, environmental or organic, which influence the efficiency of performance favorably or unfavorably are termed facilitators and inhibitors respectively. While this non-technical view is implied in the treatment of our problem, its relation to physiological concepts will be attempted in a final section.

The effect of muscular tension upon mental processes has been approached experimentally from two directions. The first, which has occupied the majority of researchers, is to vary some phase of behavior and observe its influence upon the tonus accompaniments of the work. The second attack is to alter the tension of the subject experimentally and observe its effect upon performance.

The validity of investigations of the first type depend primarily upon the adequacy of the technique used to record the changes in tonus. These techniques I

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have critically examined in a previous article.[1] In general, it may be said that the mechanical systems seem the more open to exploitation than the electrical systems at the present time; and that in such systems (mechanical) the problem of measuring tonus changes resolves itself into one of optimal magnification with a minimum of distortion. Since most existent work is open to technical criticism, it is all the more significant to find a certain general agreement. Even with such a crude indicator as the amplitude of knee-jerk, Lombard,[2] Tuttle,[3] and many others[4] have reported an increase in tension during mental activity. But whether such tension is only an incidental consequence of cerebration or an actual reënforcing agent is not apparent from such data as these. In order to answer this delicate question, more careful experimental controls were devised. Golla was among the first to utilize more refined means of recording and to compare extent of tonus increment with work output.[5] In his second article (Golla and Antonovitch)[6] he noted that the maximal tension usually preceded the actual mental work. Although the work was said to become progressively more difficult, the period of heightened tension was not prolonged. On the basis of such results, Golla argued that the tonus accompaniments of mental effort are merely "atavistic phenomena representing the preparation of the organism for continued bodily activity, the necessity for which no longer exists." Opposed to this interpretation are several extensive researches, utilizing the same mode of attack. Bills and Brown reported that tension in the arm muscles was greatest and work-output highest, when the subject was set to do a limited amount of work.[7] Freeman showed that where quadriceps tension increased under distraction, work output tended to be kept to normal.[8] The work of Freeman and Lindley indicated that tonus increases as the subject becomes relatively fatigued and that the greater the tonus increments during protracted work, the less the average decrement in work output.[9] Bills found that the tension of arm and jaw muscles increased when the subject was running the more difficult parts of the finger maze.[10] Duffy, however, reports that children with highest tension scores usually give the poorest performance.[11]

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[1] G. L. Freeman, The measurement of tonus by deformation of the tendon, this JOURNAL, 42, 1930, 581-592.

[2] W. P. Lombard, The variations of the normal knee jerk and their relation to the activity of the central nervous system, *ibid.*, 1, 1887, 2-71.

[3] W. W. Tuttle, The distribution of tone in skeletal muscle, *J. Exper. Psychol.*, 8, 1925, 319-322.

[4] A fairly complete list is given by A. G. Bills, The influence of muscular tension on the efficiency of mental work, this JOURNAL, 38, 1927, 227-251.

[5] F. L. Golla, The objective study of neuroses, *Lancet*, 201, 1921, 118.

[6] F. L. Golla and S. Antonovitch, The relation of muscular tension and the patellar reflex to mental work, *J. Ment. Sci.*, 75, 1929, 234-241.

[7] A. G. Bills and C. Brown, The quantitative set, *J. Exper. Psychol.*, 12, 1929, 301-324.

[8] Freeman, Changes in tonus during completed and interrupted mental work, *J. Gen. Psychol.*, 4, 1930, 309-334.

[9] G. L. Freeman and S. B. Lindley, Two neuro-muscular indices of mental fatigue, *J. Exper. Psychol.*, 14, 1931, 567-605.

[10] A. G. Bills, Tension in learning and association, *Proc. 9th Int. Cong. Psychol.*, 1929, 75-76.

[11] E. Duffy, Tensions and emotional factors in reactions, *Genet. Psychol. Monog.*, 7, 1930, 1.

It is difficult to compare any of the above work. Golla's reports of his experiments are very sketchy and show little familiarity with accepted concepts of psychological control. He does not state how long his subject worked, what the instructions were, and whether or not a period of pre-work rest was given. With regard to his recording technique, he admits that "the delicacy of the method also showed slight postural adjustment;" but it is not clear what means, if any, were used to check or cancel the effects of these accidental movements on the records. While the work of Freeman and Lindley utilized an even more delicate system of recording tension, some check was obtained upon the effect of mechanical leg-displacements on the records. In common with other researches, however, very little was known of the absolute level of muscular tension from which increment occurred. Such an important matter was apparently not considered in the work of Bills, who used pressure downward on the pencil point as an indicator of the tension in the arm muscles, or of Duffy, who used pressure on a rubber ball held in the hand.

It is evident that aside from the question of the relative merit of recording techniques, none of these experiments provides crucial data upon the question of tonic facilitation. One difficulty is that there is no norm with which to compare the alleged facilitation.

The second type of attack owes its present popularity to the ambiguity of results obtained by the first. Here the general plan is to set up a state of heightened muscular tension experimentally and compare the efficiency of mental work during this period with a control series of observations. Various means of inducing tension have been used, including formal instructions to be relaxed or tense, and situational instructions, such as the gripping of dynamometers. The general outcome of this type of research has been in favor of muscular facilitation. Mental processes may be "relaxed away." Under muscular relaxation the knee-jerk is abolished (Jacobson).[12] Relaxation is accompanied by a reduction in the extent and speed of reaction to induction shock, as well as a raising of the sensory threshold (Miller).[13] Conversely, the rate of learning various tasks is facilitated by experimentally induced tension (Bills).[14]

The logic of this type of experimentation is based on the assumption that the *S* is more relaxed or tense in the test than in the control series of observations. But this is not a matter which can be settled arbitrarily. Some objective indicator of tension (such as is used in the first type of attack) is desirable as a check upon the actual neuro-muscular state of the organism in test and control series. Such a check has rarely been employed in the past.

There are many apparent difficulties which hinder the study of tonic muscular facilitation and inhibition at the higher levels of neural integration. In the first place we cannot deal with the neuro-muscular processes in a manner analogous to our treatment of external physical stimuli. If we wish to ascertain the facilitating effect of sound upon reaction to light, we define accurately the amount of the facilitating stimulus as well as its time of presentation. This is difficult when the facilitating stimulus is to be the tonic contraction of muscle. True, we may have the *S* grip a dynamometer with a certain force at a certain time and observe the effect which this has upon something else he is doing. But if gripping the dynamometer becomes the *S*'s major problem, to the relative neglect of the task first assigned, an inhibition may be worked by a shift in the behavioral pattern to a more dominate competing tendency. In the second place, precise time relations are difficult to study except on the reflex level. Here we might hope to accurately record the intervals of optimal facilitation and inhibition. The patterns of cortically dominated performance cannot be isolated in such a manner. They involve many levels of neural action. So if evidence was obtained of direct facilitation or inhibition of reflex response, it would be difficult to carry the analogy to the more complicated pattern, where the facilitative effects might be indirect. Clearly, we cannot set up a simple situation such as is possible in most work on facilitation and inhibition. Our approach must be more indirect. Only by correlating variations in behavior and tension under a wide variety of experimental conditions can we hope for adequate data. Even then our interpretation will be largely a matter of inference, the validity of which depends in large measure upon the experimental precautions taken in obtaining the results. These precautions may be summarized under the following headings:

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[12] E. Jacobson, *Progressive Relaxation,* 1929, 112, 164.

[13] M. Miller, Changes in the response to electric shock produced by varying muscular conditions, *J. Exper. Psychol.,* 9, 1926, 26-45.

[14] Bills, *op. cit.*

(*1*) *Measurement of tension.* A sensitive mechanical system is desirable in connection with all experiments. Whenever used it should be carefully checked to determine the effects of any artifacts (such as movement) in the records.

(*2*) *Pre-work baseline.* Since all records of tension are relative, care should be taken to establish a pre-work baseline, from which variations may be reckoned. Usually this will consist in having the *S* relax for a stated time before beginning the work. In certain experiments it may be more desirable to compare a normal control series with one of experimentally induced tension.

(*3*) *Practice effects.* Adaptation to the experimental situation has a notable effect on tension.[15] Thus it is frequently desirable to have several practice sessions before beginning the experimental series and to counterbalance any variations in procedure which may have a significant trend on the results.

(*4*) *Choice of performances to be studied.* Those performances which are chosen for study should be representative. It may well be that the type of performance under survey has a great deal to do with the question of whether a given amount of tension will facilitate or inhibit. A desirable feature of experimentation would be the inclusion of several distinctly different tasks.

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[15] This statement is made upon the basis of the report that tension is reduced by practice in learning nonsense syllables (G. L. Freeman, The spread of neuromuscular activity during mental work, *J. Gen. Psychol.*, 5, 1931, 479-494).

General Plan of Experimentation

The experimental program covered by the present report includes 6 separate researches, as follows: (I) Relationship between anticipation of task difficulty and tension in a remote muscle group. (II) Relationship between degree of motivation, work output, and tension. (III) Relationship between degree of muscular preparation and reaction-time. (IV) Effect of equivalent muscular reënforcement in several types of performance. (V) Effect of experimentally induced tension and relaxation upon reaction to two intensities of electric shock. (VI) Localization of tension producing optimal facilitation in a given task. Each of the above topics will be discussed separately. These discussions will be preceded by a brief description of the apparatus common to most of them and followed by an integrative résumé of the total program.

Apparatus and General Methods

Tension of the right quadriceps muscle was photographically recorded by the method of tendon deformation, previously described. This device was, in essential, a lever serving to depress the patellar tendon, connected with an optical system which magnified changes in tendon deformation (unless otherwise stated) approximately 500 times. The *S* sat in a comfortable morris chair (See Fig. 1), with a screen shielding the recording camera and attachments from his view. The lever serving to depress the patellar tendon was at first placed directly in contact with a point tattoed on the skin. In certain of the later experiments where comparable records were not at such a premium, the depressor rested over the pant-leg. In experiments where great care was taken to obtain equivalent magnification on successive days, the micrometer adjustment at the base of the chair proved invaluable. After the *S* was seated and the tendon depressor set in place, the chair, and *S*, could be moved forward or backward until the reflection of a mirror placed on the tendon apparatus coincided with a standard distance mark on the camera slit. The standard procedure in all experiments was to place the *S* in the apparatus for several sessions prior to actually beginning the experiment. This enabled the *S*s (who were with a few exceptions hired undergraduates) to become well adapted to the general experimental situation before beginning the regular series. The pre-work relaxation periods varied in individual experiments from 3–15 min. Occasional comments were taken from the *S* and sometimes questions were asked. The author (*F*) frequently served as an *S*, largely for the purpose of ascertaining the subjective side of the experiment. The results which are discussed are almost wholly objective and were obtained from the photographic records. In the first experiments only one *S* was used each day for fear of changing the setting of the tension recorder. Later it was found that the apparatus could be reset without loss of accuracy, thus enabling *E* to use several *S*s a day. The *S*s were chosen with some

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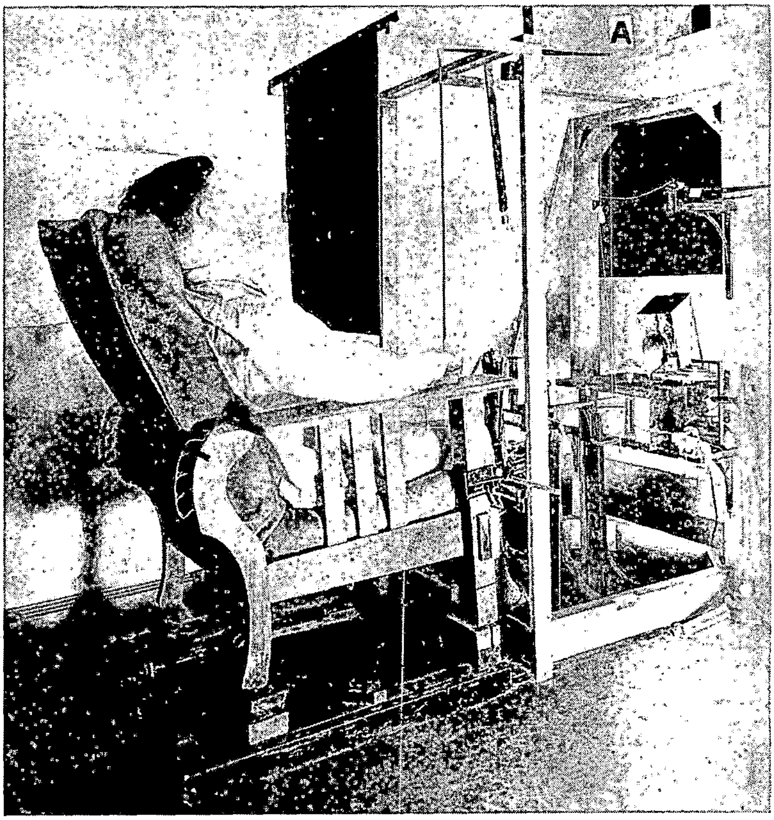

FIG. 1. APPARATUS AND EXPERIMENTAL SITUATION

care, both with respect to their reliability and the position of their patellar tendons.[16] While the method outlined does not lend itself to the rapid accumulation of data, its accuracy serves to counterbalance that disadvantage.

EXPERIMENT I

Problem. When we say that the effort of an *S* varies, we imply that some internal factor produces increments and decrements in his output. Mental effort is an ill-defined rubric covering variations in the *S*'s estimation of task difficulty, and in his motivation. Both factors are presumptively reflected in the tonus accompaniments of his mental work. To what extent does the anticipation of task difficulty influence the general tension of the organism? What effect has the degree of preparation on the efficiency of the performance? These questions were made the subject of the first investigation.

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[16] That is, position relative to the skin surface. If the tendon is imbedded and surrounded by a great deal of fatty tissue, changes in its position produced by quadriceps tension will not be transmitted to the tendon depressor. We favored, therefore, *S*s whose tendons lay freely exposed and close to the surface of the skin.

Procedure. Six *S*s were given 60 practice trials in reacting to the slow movement of a small dot in the visual field. The reaction took the form of a compensation, *S* pulling or pushing a stick in the direction opposite to the seen movement. The dot moved at three different rates of speed, making the reaction-problem of various degrees of difficulty.[17] All the *S*s worked under a reward incentive, calculated to produce maximal effort. Three specific variations in instruction were used, following the initial practice. There was a 15 min. rest period prior to each day's work and a 1 min. rest period between successive trials. Ten trials of each degree of task-difficulty (30 trials in all) were given in haphazard order in each of the variations. Practice effects occurring during the experimental series were equilibrated by counterbalancing the order of presenting the variations to the *S*s. The variations were:

(*a*) *Without knowledge of rate or direction.* Although the *S*s realized—as the result of the practice series—that the problems were of unequal difficulty, no intimation was given on which particular rate of movement would next occur. The instructions were:

> Your problem for today will be to react to the movement of one of the bright dots which you see before you. Your eyes will be closed before every trial. When the 'ready' signal is given open your eyes and fixate the dots. If the right hand dot moves upward, give the stick in your hand a slight downward pull. If the dot moves down, push up with the stick. Always make an effort to react as quickly as possible; but do not sacrifice accuracy since it will count against you. If you move in the wrong direction, correct your error at once.

(*b*) *With knowledge of rate.* Just prior to the ready signal, the *S*s were told what rate of movement would next be used. They were asked to try to increase their speed of reaction to the slower rates by being more adequately 'set.' The actual instructions were:

> Your records show that you react less quickly to the slower movements of the dots. In these tests the experimenter will help you to increase the speed of your reaction. Before every ready signal he will tell you the difficulty of the problem so that you may be adequately set. 'One' will signify the hardest problem of all, 'two' the next, and 'three' the easiest. The experimenter will not tell you in which direction the dot will move.

(*c*) *With knowledge of rate and direction.* The *S*s were informed prior to stimulation not only of the rate but also the direction of movement. This lessened the anticipated difficulty of all problems. The instructions were:

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[17] *S* actually saw two dots: one fixed, and one movable. The pair were shown to prevent reaction to the autokinetic shifting of a single dot. Task-difficulty was a function of the rate of movement of the variable dot. The most difficult reaction (1) was in response to the slowest movement, almost imperceptible. The next most difficult reaction (2) was to a slightly more rapid movement and the easiest reaction (3) was to a readily distinguishable movement.

It is possible to further increase the speed of your reaction. The experimenter will now state not only the rate but also the direction in which the dot will move: '1 up' will signify the slowest upward movement. Be on the alert for the ready signal as usual, but do not react until you actually see the dot move. Reactions made before the movement will be counted as false. Remember that accuracy counts as much in this as in previous tests.

In order to compare these results with the tonus accompaniments of tasks utilizing another sensory avenue, two *Ss* repeated variations *a* and *b* with reactions involving tactual discrimination. The problem given was to compensate for the felt movement of the stick (previously used in reacting to seen movement). Three rates of movement paralleling those in the visual problem were produced by releasing appropriate weights attached to lever A in the system pictured in Fig. 1.

Tonus changes were photographically recorded from the patellar tendon as already described. Since anatomical differences between *Ss* made it impossible to to make absolute comparisons of tonus change in terms of amount of tendon deformation (as expressed in mm. of deflection on the record), we sought a means of reducing these readings to a common denominator. The way was open through our discovery of the relationship between tendon deformation and muscular pull.[18] An apparatus was used to calibrate the photographic excursions of each *S*'s tendon record. The stirrup carrying the tendon depressor swung on an axis by finely machined cone bearings. After the stirrup and tendon depressor were satisfactorily adjusted, the lower limb hung limply from its support at the knee. The stirrup was then connected to another lever which could be raised by minimal contractions of the *S*'s quadriceps muscle. (The actual displacement of the foot was about ¼ in.) A mirror at the axis of this lever served to record the height to which the lever was lifted by the quadriceps. The excursions of the tendon depressor were photographed concomitantly with the changes in the height of the weight lever. We had already obtained by empirical means a record of the number of grams of weight which were necessary to lift the lever to various heights. By comparing a *S*'s calibration with this standard, we were able to express mm. of tendon deflection in terms of grm. tension. This system accurately measured increments in muscular tension of the magnitude of one grm. The calibration had to be made only once for each *S*, since care was taken in placing the tendon depressor exactly on successive trials.

Results. Before presenting the quantitative data, certain typical records may be profitably surveyed.

Fig. 2 reproduces the calibration record of one *S*. It will be recalled that as the *S* pushes the weight lever upwards, its excursions and the changes in quadriceps tension are simultaneously recorded. Having previously ascertained the points to which the weight lever rises under known pressures (W) it is easy to translate mm. of change in the tendon record (T) into grams tension. Thus an excursion of 10 mm. equals 1 grm. tension while an excursion of 35 mm. equals 10 grm.

Figs. 3 and 4 reproduce records taken respectively during the slow and fast rates of movement in the visual problem. The stimulus and reaction (shadow) lines are easily recognizable. The variable dark lines indicate two degrees of magnification of tendon deformation, and were produced by connecting the tendon

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[18] Freeman, *op. cit.*, this JOURNAL.

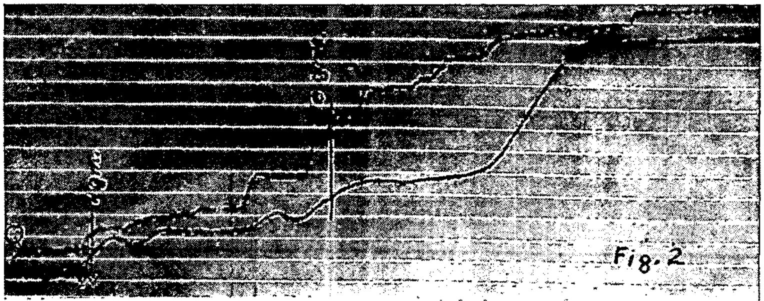


Fig. 2

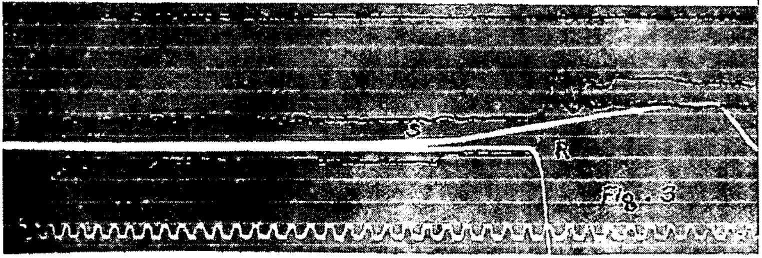


Fig. 3

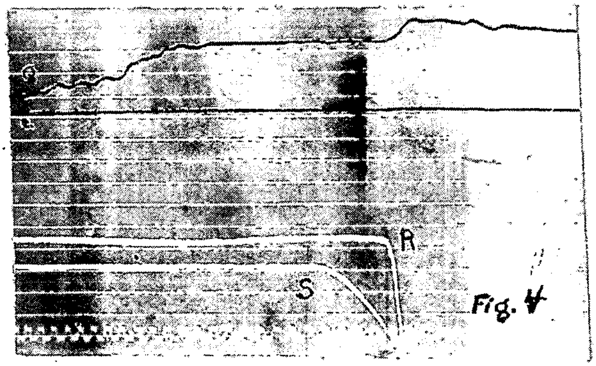


Fig. 4

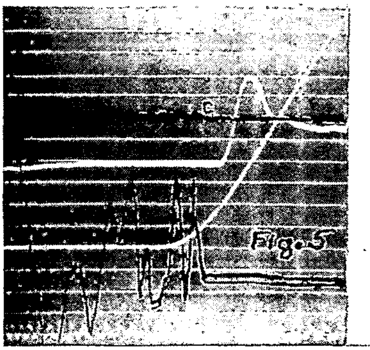
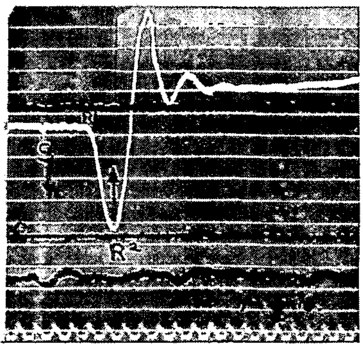


Fig. 5



Figs. 2–6. Sample Records Made in Experiment I

depressor to cylinders of different diameter. Since the ratio between the two cylinders is known, we may accurately estimate tonus changes according to the more sensitive recorder in case the reflection therefrom leaves the photographic paper. The straight dark line is a reflection from a mirror placed upon the carrying stirrup and indicates that no leg displacement occurred during these records.

Fig. 5 shows the artifacts produced in the tendon record (T) by postural shifts. Such records are eliminated from the quantitative computations.

Fig. 6 reproduces a record taking during tactual discrimination. The point in the shadow line marked *s* is where the weight was dropped. We see that the subject compensates in the wrong direction at R^1 and corrects his error at R^2.

Quantitative. Inspection of Tables I, II, and III show that the anticipation of task difficulty is reflected in the tonus accompaniments of the work. There are no consistent differences in the amount of tension which accompanies the solution of tasks of various degrees of difficulty, when the *S* is previously unaware of what that difficulty will be (Table I). There are, however, consistently greater tonus changes accompanying the hardest problem when the *S* is notified that it is coming (Table II). Knowledge

TABLE I

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Variation *a:* without prior knowledge of rate or direction)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
Pe	a, b, c	1	1.4	1.6
		2	1.2	1.7
		3	2.0	1.9
Th	a, b, c	1	2.4	.8
		2	5.3	1.8
		3	4.5	3.3
Su	b, c, a	1	.8	1.0
		2	1.6	.8
		3	1.4	.4
Li	b, c, a	1	.31	1.50
		2	.17	.78
		3	.33	.55
Se	c, a, b	1	1.6	.58
		2	1.5	1.00
		3	1.5	.97
Or	c, a, b	1	2.6	.33
		2	2.7	1.60
		3	1.9	2.75
Av.		1	1.52	.97
		2	2.08	1.28
		3	1.94	1.65

TABLE II

The Relationship between Tonus Fluctuations and Task-Difficulty
(Variation *b*: with prior knowledge of rate)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
Pe	a, b, c	1	5.45	4.00
		2	3.36	2.65
		3	1.05	2.40
Th	a, b, c	1	5.5	.33
		2	3.8	2.60
		3	4.0	2.80
Su	b, c, a	1	1.65	1.80
		2	.80	1.30
		3	.65	1.35
Li	b, c, a	1	1.1	.40
		2	.2	.10
		3	1.2	1.06
Se	c, a, b	1	4.3	.80
		2	.5	1.75
		3	.3	.80
Or	c, a, b	1	3.2	1.60
		2	1.9	.85
		3	2.3	.38
Av.		1	3.53	1.49
		2	1.76	1.54
		3	1.58	1.47

TABLE III

The Relationship between Tonus Fluctuations and Task-Difficulty
(Variation *c*: with prior knowledge of rate and direction)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
Pe	a, b, c	1	2.15	1.25
		2	1.9	.05
		3	1.1	1.26
Th	a, b, c	1	.8	2.4
		2	1.8	1.0
		3	1.6	.8
Su	b, c, a	1	.7	.2
		2	.2	.5
		3	.35	1.3
Li	b, c, a	1	.31	.1
		2	.5	.5
		3	.3	.1
Se	c, a, b	1	.85	.1
		2	.1	.8
		3	.4	.1
Or	c, a, b	1	.62	.1
		2	.4	.85
		3	.75	.2
Av.		1	.90	.69
		2	.81	.61
		3	.75	.62

of the entire nature of the problem prior to its solution is reflected by the lessened tonus changes accompanying preparation and solution (Table III). The observations made with the visual problems (Tables I, II, III) are substantiated with tactual problems (Tables IV and V). All tonus changes recorded are expressed in grams tension, over the pre-work baseline. The tonus changes recorded in all tables are the averages of 10 trials for each task-difficulty. The pre-reaction changes are reckoned between the ready signal and the time the stimulus moves (20 sec.). The post-reaction readings are taken between reaction and a point 20 sec. after this.

TABLE IV

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Tactual problems: without prior knowledge of difficulty)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
Li	a, b	1	1.2	0.3
		2	1.5	1.6
		3	0.7	1.1
Su	b, a	1	1.3	3.0
		2	3.3	1.1
		3	2.0	0.8
Av.		1	1.2	1.6
		2	2.4	1.3
		3	1.3	0.9

TABLE V

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Tactual problems: with prior knowledge of degree of difficulty)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
Li	a, b	1	5.0	0.8
		2	1.5	1.6
		3	0.9	0.6
Su	b, a	1	4.9	5.1
		2	2.7	1.0
		3	2.1	0.7
Av.		1	4.9	2.9
		2	2.1	1.3
		3	1.5	0.7

What effect has the degree of preparation upon the efficiency of the performance? This is an important question which will be considered in greater detail in connection with a later experiment. Certain indications are apparent from the present data. Let us consider the tonus changes occurring under a given instruction an index of this preparation. Table VI compares the reaction-times and tonus changes obtaining under variations (*a*) and (*b*). We see that the increased tonus changes of (*b*) are correlated with increases in efficiency (as measured by decreases in the reaction-time.) The records for variation (*c*) are excluded from this comparison, because the difficulty of all the reactions is experimentally reduced.

TABLE VI

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND REACTION-TIMES

			Variation (*a*)			Variation (*b*)		
S	Time order	Task Diff.	Av. R. in sec.	Tonus		Av. R. in sec.	Tonus	
				pre-r.	post-r.		pre-r.	post-r.
Pe	a, b, c	1	.63	1.4	1.6	.41	5.4	4.0
		2	.40	1.2	1.7	.50	3.3	2.6
		3	.40	2.0	1.9	.52	1.0	2.4
		Av.	.48	1.5	1.7	.48	3.3	3.0
Th	a, b, c	1	.78	2.4	0.8	.67	5.5	0.3
		2	.51	5.3	1.8	.58	3.8	2.6
		3	.49	4.5	3.3	.45	4.0	2.8
		Av.	.59	4.7	1.9	.57	4.4	1.9
Su	b, c, a	1	1.01	0.8	1.0	.65	1.6	1.8
		2	.71	1.6	0.8	.54	0.8	1.3
		3	.70	1.4	0.4	.64	0.6	1.3
		Av.	.81	1.3	0.7	.61	1.0	1.4
Li	b, c, a	1	.71	0.31	1.5	.59	1.1	0.4
		2	.49	0.17	0.7	.50	0.2	0.1
		3	.45	0.33	0.5	.43	1.2	1.0
		Av.	.55	0.27	0.9	.51	0.8	0.5
Se	c, a, b	1	.84	1.6	0.5	.46	4.3	0.8
		2	.55	1.5	1.0	.40	0.5	1.7
		3	.48	1.5	0.9	.43	0.3	0.8
		Av.	.62	1.53	0.8	.43	1.7	1.1
Or	c, a, b	1	.72	2.6	0.3	.48	3.2	1.6
		2	.69	2.7	1.6	.53	1.9	0.8
		3	.53	1.9	2.7	.50	2.3	0.3
		Av.	.65	2.4	1.5	.50	2.5	0.9
Av.		1	.78	1.5	0.9	.54	3.5	1.4
		2	.56	2.0	1.2	.51	1.6	1.5
		3	.51	1.9	1.5	.49	1.5	1.4
		Av.	.62	1.8	1.3	.51	2.3	1.5

Discussion. Knowledge of task difficulty influenced the degree of subjective 'effort.' Records of muscular tension reflected these variations. Experimental reduction of task difficulty decreased tonus changes. Increased tension due to anticipation of task difficulty and increases in efficiency of reaction are apparently positively related. It will be recalled that our results are in opposition to the report of Golla and Antonovitch that as the task became "progressively more difficult, the period of heightened tension was not prolonged." These authors, however, were more concerned with the form of his tonus curves than with quantitative values. Our chief criticism of their work rests upon the fact that they gave no indication of how it was determined that their problems actually became more difficult. This same criticism can be applied to a lesser degree in the work Bills, who reports that the tension of adults increased when they were running the more difficult parts of a maze. In neither of Golla and Antonovitch's nor Bills' research has task-difficulty been equated upon as objective a basis as our own.

EXPERIMENT II

Problem. We have seen that the work of Duffy with children indicates that high tension is sometimes detrimental to precise performance. The work of Bills on the other hand suggests that tension might be increased indefinitely without having an inhibitory effect. The problem of the following investigation was to seek evidence of both facilitative and inhibitory effects in the same task. Our plan was to elicit variations in the motivation of subjects and to observe the changes which such variations might produce in output and tension.

Procedure. Eight *Ss* were selected for competitive work in continuous addition. They were divided into two groups, each *S* working for a prize. Two experimental sessions were held for every *S*. On the first day, all added columns of figures for three 5-min. periods. On the second day they worked 5 min. under normal maximal incentive and 5 min. each under incentives calculated to produce 'super' and 'sub-maximal' effort. The instructions were:

> You will be given tests in addition for two consecutive days. After you are comfortably seated in the apparatus try to rest and relax. After a time I will give you the addition sheets and tell you to begin work. Enter upon these tests with maximal effort, as a prize of $5 will be given to the speediest and most accurate adder in your group. By the second day I will know something of what the others have done, and will so advise you. Thus you may know how you are measuring up with the rest. Sit quietly during the tests. Put down your totals only. Whenever I announce the end of a 5-min. interval place a check mark upon the column of figures you are adding.

The order of presenting the experimental variables was properly equilibrated. Sub-maximal effort was sought by telling the *S* there was no chance for him to win the prize, his yesterday score being found to be so far below those of his competitors. Super maximal effort was sought by informing the *S* that there was, after all, a chance of his winning the prize by extra effort; this was reënforced by standing over him for the next 5 min. and 'dogging' him on.

Results. Quadriceps tonus was recorded as in Experiment I, except that no attempt was made to convert the changes in mm. of deflection to grams tension. Thus the actual values tabulated are not rendered significant; that is, changes in the deflection values for the different *S*s do not represent equal changes in their quadriceps tonus.

TABLE VII

PERFORMANCE AND TONUS

(1st day: control series)

S	1st test period		2nd test period		3rd test period	
	Perf.*	Mean Tonus	Perf.*	Mean Tonus	Perf.*	Mean Tonus
Ci	20/2	13.0	32/2	63.0	21/5	32.0
Wi	20/2	28.0	20/4	25.0	20/2	21.0
Dr	15/2	32.5	30/4	72.7	22/4	67.5
Ma	15/2	9.5	18/2	13.0	17/1	11.0
Fe	14/3	22.0	15/5	17.5	14/2	18.5
Fo	10/2	34.0	12/2	69.0	10/3	42.0
Fa	10/0	7.1	12/0	28.4	12/2	42.0
La	15/2	50.5	14/5	19.0	15/2	8.1
Av.	15/1.9	26.1	19/3	38.4	16/2.6	30.3

* Performance scores include number of columns added in 5 min. over the number of columns wrong.

Discussion. The tables indicate that while tension is a factor normally contributing to the facilitation of mental work, it may become an inhibitor of precise performance. Tension was notably increased under the conditions of 'supermaximal' effort; and concomittently, the speed of addition

TABLE VIII

PERFORMANCE AND TONUS

(2nd day: under various incentives)

S	1st test period (max. incentive)		2nd test period (sub-max. incentive)		3rd test period (super-max. incentive)	
	Perf.	Mean Tonus	Perf.	Mean Tonus	Perf.	Mean Tonus
Ci	34/8	23.0	29/4	10.0	46/20	45.1
Wi	19/4	16.5	17/3	8.4	21/5	71.0
Dr	25/6	6.8	17/4	11.5	24/3	28.5
Ma	19/3	2.1	15/5	1.8	17/7	17.6
Fe	15/2	7.3	17/5*	12.5	15/4	11.7
Fo	15/2	44.0	9/4	50.0	16/8	71.0
Fa	18/4	51.7	18/5*	65.2	17/3	102.5
La	18/6	3.0	14/6	40.5	20/13	89.2
Av.	20/4.4	19.3	17/4.5	24.9	22/7.9	54.6

increased and accuracy decreased. The occasional instructions which sought to secure 'sub-maximal' effort were effective in all but two cases (*Fe* and *Fa*). These two *Ss* apparently took exception to the suggestion that they could not win and so redoubled their efforts. Table VIII shows that their mean tension increased and performance improved during this period rather than the reverse.

EXPERIMENT III

Problem. One of the more traditional approaches to the problem of facilitation and inhibition in performance has been through the voluntary reaction-time. The presentation of stimuli to two sense modalities may decrease or lengthen the voluntary reaction to one of them, depending upon time relations of the stimuli, (Jenkins).[19] The following experiment studied the effects of increased muscular tension upon reaction-time to sound. Since it is known that reaction-time is normally dependent upon the strength of the stimulus, the experiment took the form of comparing reaction to two intensities of sound under conditions (*a*) of greater and (*b*) of less tension. No precise means for defining the amount of tension or the time of its appearance were possible. By instruction, however, the *S* took the tension 'set' at every ready signal (2 sec. before the stimulus was sounded) and the photographic records of quadriceps tension gave some indication of the amount of neuro-muscular 'spread.'

Apparatus and technique. The stimuli used were clicks of the diaphragm of the *S*'s earphones. The first (which will hereafter be designated as 'weak') was slightly above the R.L. of the 4 male *Ss* used. The second ('loud') was well above the limen, but not too intense to cause discomforture when heard through the earphone. These clicks were produced by breaking an electric circuit through a relay, outside the soundproof room in which the experiments were held. By means of a double-pole, double-throw switch, the current could be made to pass through either of two inductoriums, with their secondary coils appropriately set for 'weak' and 'loud' clicks.

S sat in a morris chair. He was blindfolded and rubber 'ear-muffs' were placed over the earphones to eliminate secondary cues. Photographic records of reaction-time and right quadriceps tension were taken after the usual manner. A neon tube provided the time lines of the record. The latency of the Becker markers, used in the stimulus and reaction-lines, was found to be from 7 to 10 sigma. No correction has been made for such errors, however, in the reading of the records.

Procedure. All the *Ss* received 100 practice trials at the first experimental session. Thereafter they were divided into two groups. Group 1 worked for 4 consecutive days under instruction (*a*), and then for 4 days under instruction (*b*). Group 2 reversed the time-order of Group 1. The experimental variations were as follows:

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[19] T. Jenkins, Facilitation and inhibition, *Arch. Psychol.*, 14, 1926, 1-55.

(*a*) *Control series.* Fifty reactions each to 'weak' and 'loud' stimuli were taken at every experimental session. The stimuli were presented in haphazard order with the following instruction:

> Your problem is to react to these auditory stimuli as quickly as possible. When you hear the ready signal, press your finger on the reaction-key. Release the key immediately you hear the click in the earphone. It is important that you sit quietly during these tests. An interval will elapse between successive trials, during which you will count numbers to yourself. Try not to think of the next reaction until you hear the ready signal.

(*b*) *Tension series.* The same number of reactions (50) were taken as in (*a*). While 'weak' and 'loud' stimuli were still given haphazardly, *S* was informed at every 'ready' signal whether the following click would be weak or loud. His particular problem was to concentrate on reaction to the weak stimuli, attempting to shorten his time by being 'tense' and 'set.' The instructions were:

> Your problem is to react as quickly as possible to these two intensities of auditory stimulation. It generally takes longer to react to the 'weak' than to the 'loud' stimuli. By being properly set or ready, however, you can greatly increase the speed of your reaction to the 'weak' stimuli. At each ready signal I am going to state what the next stimulus will be. Do not relax your efforts to react quickly to the 'loud,' but exert an added effort when I say 'weak.' You will find that it will help to get your muscles into a state of slight tension or readiness to react whenever you hear the signal.

Results. The logic of the experiment rests with the comparison of the reaction-time to weak and loud auditory stimulation under the two conditions (*a*) and (*b*). If reaction to 'weak' under (*b*) is notably decreased, we have evidence of facilitation by way of increased tension. If the reaction-time is increased, the data favor the opposed interpretation. Reactions taken under (*a*) provided the necessary 'standards' for comparison.

TABLE IX

AVERAGE REACTION-TIME AND TONUS INCREMENT IN CONTROL SERIES

(Time in sigma; Tension in mm.)

S	Time order	Weak stimuli				Loud stimuli			
		Reaction-time		Tension		Reaction-time		Tension	
		Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
Sm	1	222.0	18.3	6.14	2.7	194.2	15.0	7.26	3.8
Ba	1	247.1	27.1	9.81	2.8	185.6	16.6	9.76	3.1
Ho	2	183.0	16.5	4.41	1.8	152.9	23.0	4.93	2.1
Fe	2	163.5	23.0	4.72	2.2	119.2	16.4	6.80	2.8
Av.		203.9	21.4	6.72	2.4	162.9	17.7	7.19	2.9

In Table IX are given the average reaction-times and tension scores obtained under condition (*a*). Table X gives similar data obtained under (*b*). The reaction-time scores are in sigma and the tension scores are in mm. of deflection (from a baseline established prior to the ready signal).

These tension scores represent the average height of increment per record when the changes in tendon deformation are magnified approximately 800 times.

TABLE X

AVERAGE REACTION-TIME AND TONUS INCREMENT IN TENSION SERIES
(Time in sigma; Tension in mm.)

		Weak stimuli				Loud stimuli			
S	Time order	Reaction-time		Tension		Reaction-time		Tension	
		Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
Sm	1	152.4	24.8	11.07	4.7	150.7	12.5	7.03	2.9
Ba	1	148.9	16.8	4.93	2.3	144.4	21.8	2.71	0.9
Ho	2	152.6	20.4	5.21	2.6	148.9	18.7	4.68	2.1
Fe	2	150.8	29.8	15.70	4.7	127.0	37.0	10.23	3.5
Av.		151.2	22.9	9.23	3.8	142.7	22.5	6.16	2.4

Discussion. The tables show that reaction-time to 'weak' stimuli in the tension series (*b*) was appreciably shorter than in the control series (*a*). This holds even for *Ho* and *Fe*, who reacted first under (*b*). (We might expect that practice would have reduced their reaction to 'weak' stimuli in (*a*), since a comparison of reaction-times to the loud stimuli shows such a reduction on the last 4 days of the experiment.)

That the *S* accepted instruction (*b*), is indicated by their tension scores, which are greater for 'weak,' than for 'loud' stimuli. They reported that the 'set' for 'weak' stimuli tended to carry over into their preparation for the 'loud' stimuli. We may conclude that the data of this experiment indicate that muscular tension (in the form of a preparatory set) may facilitate reaction-time to sound.[20]

EXPERIMENT IV

Problem. Previous work has shown that the normal tonus accompaniments vary with the type of performance under survey.[21] The problem of this investigation was to study the effects of approximately the same amount of tension upon (a) finger oscillation and (b) manual pursuit. The *Ss* used in the previous experiment showed lower tension scores for manual pursuit than for finger oscillation. Will increments in tension be equally facilitative in both performances?

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[20] We suggest further that the shortening of reaction-time under the 'traditional motor set' may be due to the presence of greater tension under this condition. This seems to be bourne out by the work of Judd (*Psychol, Monog.*, 7, 1905, 199-226) and Williams (*Psychol. Monog.*, 17, 1917, 55-155).

[21] Cf. Freeman, The compensatory effects of muscular tension subsequent to sleep loss, *J. Exper. Psychol.*, 15, 1932, 267-283.

Procedure. Two *Ss* who had received extensive practice in both performances (in connection with the other experiment), undertook a series of trials in which the amount of tension was under a certain degree of experimental control. Tests were of 30-sec. duration and were separated by a 5-sec. interval. Fifteen trials of each performance were completed on each of 5 experimental days. The time-order was properly counterbalanced. Photographic records of performance and tension were obtained after the usual procedure.

The experimental variation of tension was achieved by having *S* sustain different weights with the left arm while reacting with the right. The weights acted over a pulley and *S* was not allowed to support his elbow in sustaining them. A record of the tonus in a remote muscle group (quadriceps) indicated the 'spread' of the experimentally induced tension. Five trials of each performance were taken daily under (1) control, no induced tension, (2) arm sustaining 5 lb. weight, (3) arm sustaining 12 lb. weight. The *S* attempted maximal effort under all conditions and in so far as possible, sought to ignore any discomforture connected with holding the weights. That this occasionally occupied their attention was indicated by their reports.

Results. Figs. 7 and 8 reproduce typical records of finger oscillation taken with the left arm sustaining the 5 and 12 lb. weights respectively. Figs. 9 and 10 reproduce records of manual pursuit taken respectively under no induced tension and with the 12 lb. weight sustained. In the records, quadriceps tonus is noted as 'Q' and the line 'C' indicates that no movement of the right leg has occurred during the observation; in Figs. 7, 8, and 10, the movement of line 'W' indicates the variability in the *Ss* attempt to hold the weight steady. The reader will note the tendency for manual pursuit to be less accurate under the condition of greater tension, whereas the speed of finger oscillation suffers no reduction.

Table XI gives the average tonus and performance scores in the 3 tests of finger oscillation. Table X shows the average tonus and performance scores obtained in the test of manual pursuit. In both tables the tonus scores represent the average increment in mm. of deflection per sec. The performance scores are, respectively, the number of finger oscillations per sec. and the average accuracy of pursuit per sec. The method of determining these scores was the same as described in a previous communication.[22]

TABLE XI

QUADRICEPS TONUS AND FINGER OSCILLATION SCORES UNDER THREE CONDITIONS OF TENSION

S	No induced tension		5 lb. weight		12 lb. weight	
	Perf.	Quad. Tonus	Perf.	Quad. Tonus	Perf.	Quad. Tonus
Sm	5.5	7.2	5.4	10.8	5.7	12.5
Fe	5.7	15.3	5.9	19.8	5.7	22.2

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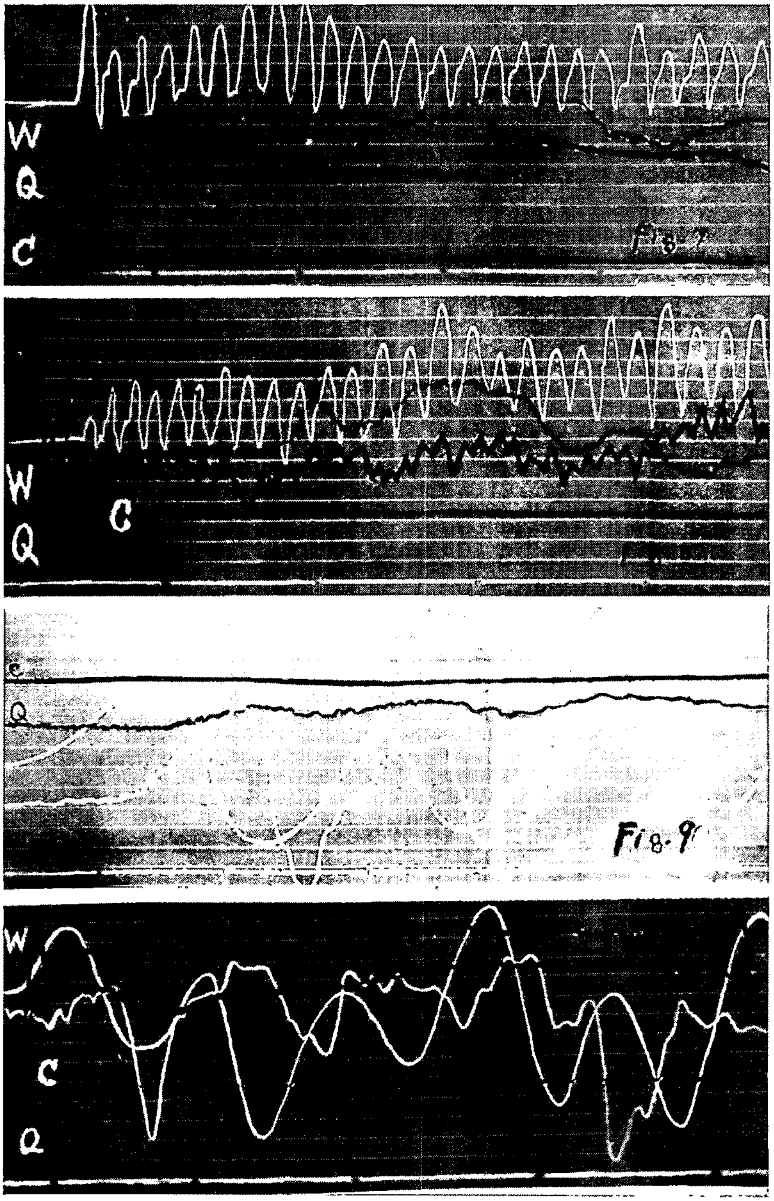
[22] *Loc. cit.*



Figs. 7–10. Sample Records Made in Experiment IV

TABLE XII

QUADRICEPS TONUS AND MANUAL PURSUIT SCORES UNDER THREE CONDITIONS OF TENSION

S	No induced tension		5 lb. weight		12 lb. weight	
	Perf.	Quad. tonus	Perf.	Quad. tonus	Perf.	Quad. tonus
Sm	.11	2.7	.18	5.8	.29	12.6
Fe	.15	4.5	.16	4.3	.24	15.7

Discussion. The indication from this rather limited data is that the facilitative effect of muscular tension on higher functions is dependent upon the type of performance under survey. The work output of a simple task (such as finger oscillation) may even increase in proportion to the amount of recorded tension. In any case, it suffers little or no retardation when the amount of tension is increased experimentally. Contrawise, the accuracy of manual pursuit decreases with the introduction of experimental tension. In this latter case, we have a performance which requires a high degree of sensori-motor coördination. It might be argued therefore, that the presence of excess tension tends to 'fog' or to disturb the finely integrated act. We thus begin to arrive at an explanation of Johnson's report that the reactor maintaining slight tension is most proficient in coördinated hand movements.[23] Additional work upon this problem is highly desirable.

EXPERIMENT V

Problem. This experiment sought to determine the effect of varying degrees of experimentally induced tension upon reactions to electric shock. While this problem has been previously attacked,[24] none of the investigators have compared the effects of tension, normal and relaxation states, in the same experiment, nor have any objective records been taken of muscular tension during the experimentally induced states. It seemed of value, therefore, to plan the present experiment along such lines.

Apparatus. In a study of responses obtaining under a variety of muscular conditions, it is important that the stimulating agent be a constant factor. The difficulties usually encountered in using electric shocks include (1) variations in intensity during stimulation, (2) no means of varying the intensity systematically, and (3) inability to measure intensity. After the trial of several less satisfactory methods, the following device was adopted.

Alternating current of 10 volt potential was obtained from a toy transformer. This passed to the primary of an induction coil with a resistance of 12 ohms. Three extra resistances (of 50, 13 and 1 ohms, respectively) could be thrown into series with this current by means of appropriate switches. The secondary of the inductorium, which could be moved as much as a meter away from the primary coil, was placed in series with the *S*s and with *E*'s key. Photographic registration of the onset and cessation of stimulation were possible by use of the Dodge mirror-telephone recorder. The diaphragm of the recorder was placed across a shunt resistance in series with the secondary circuit.

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[23] B. Johnson, Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 11, 1928, 329-341.

[24] Cf. E. Jacobson, *Psychol. Rev.*, 18, 1911, 24; this JOURNAL, 23, 1912, 345; 36, 1925, 73. More objective controls were employed by M. Miller, *op. cit.*

The system was used as follows: With the 50 ohm resistance in series with the primary, the sensory threshold of *S* was obtained at each experimental session. During the session two intensities were used, both above this threshold. The first intensity was obtained by placing the 25 ohm resistance in series with the *S* and the second intensity was obtained by using the 1 ohm resistance; that is, the less resistance in the primary circuit the greater the current passing through *S*. Since the potential remained constant and since the secondary coil was set for threshold stimulation with R_1 in series, stimuli obtained from use of R_2 and R_3 could be defined in terms of this threshold. Polarization changes in the tissues are practically eliminated by the use of alternating current.

The *S* sat in a morris chair (as in Fig. 1) with his right wrist strapped to a moistened brass electrode and the tip of his index finger resting in a liquid electrode. A string fastened around the first joint of this finger was suitably connected with a lever arrangement for photographing the extent of finger withdrawal. The index and middle fingers of the left hand rested lightly over two Lindley rotary switches used for discrimination reactions. These switches were connected with the Dodge-Travis duplex-marker, arranged for photographic registration. Right quadriceps tension was obtained after the usual manner, a three mirror system being used to make adjustment unnecessary during the session.

Procedure. The experiment was designed to measure (1) non-voluntary or 'reflex' reaction to electric shock, (2) extent of finger withdrawal, (3) discrimination time, and (4) accuracy of discrimination under different conditions of tension. These conditions were (*a*) relaxation, (*b*) hyper-tension, and (*c*) normal, *i.e.* the control. The 9 *S*s were divided into 3 groups, each group completing the experimental variations in a different time order. A 5-min. rest period at the beginning of each experimental session was used to establish the pre-work (tonus) baseline. The discrimination reaction (left hand) was introduced primarily to detract the *S*'s attention from the hand receiving the shock. This helped to make finger withdrawal automatic. The discrimination problem was a paired comparison; the first stimulus was always weak; and the second, which followed after a one second interval, was either the same or greater. Twenty reaction pairs each were given under conditions *a, b,* and *c.*

(*a*) *Relaxation.* A relaxed state is most difficult to achieve with unpracticed *S*s. In the first place the strangeness of the apparatus and situation is likely to result in an increase of tension. In the second place, it is hard to keep relaxed when there is expectation of shock. Preliminary explorations, however, showed ways of successfully handling this experimental variable. All *S*s were practiced in the method of progressive relaxation for several sessions prior to the major experiment.[25] Relaxation was favored further by arranging the chair back so that the *S* rested as on a bed. The right leg was stabalized and carried the tendon depressor as usual. All apparatus was screened from view and the room completely darkened. The ticking of a clock and the rhythmical hum of a motor served as suggestion devices, while at the same time masking any slight noise incidental to operating the camera. Pairs of shocks were given every 3 min., an interval which inspection had shown to be sufficiently long (1) to eliminate any sensory (adaptation) effect of the previous shock and (2) to permit the course of relaxation to be relatively undisturbed. The instructions were:

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[25] The *S* contracts the various muscle groups to obtain the experience of tension, then practices in the elimination of such experience. Cf. E. Jacobson, The technique of progressive relaxation, *J. Nerv. & Ment. Dis.*, 53, 1921, 283.

> After you are adjusted in the apparatus, begin to relax. During rather long intervals in the hour, you will feel a pair of electric shocks delivered to your right hand. Do not let these disturb the progress of relaxation. They are quite harmless and will cease as soon as you lift your finger. This you will do automatically, so do not attempt to attend to it. However, as long as you remember, try to discriminate between the intensity of the two shocks. Use the other hand for these reactions. The first shock given will always be the standard and you will strike the first key for this. Then second, which follows almost immediately will either be of the same or greater intensity. If the same strike the first key again. If greater, strike the second key.

(*b*) *Hyper-tension.* A familiar expedient in producing tension experimentally is to have *S* grip a dynamometer or sustain a weight. Previous experiments indicated, however, that such tasks as gripping the dynamometer frequently tend to become the major problem, to the relative neglect of the performance under observation. In the present research a means was found of producing tension which (as a problem) entered into the organization of the focal behavior pattern (reaction to shock) rather than interferred with it. The back rest of the chair was removed and the *S* was made to rest his chest against a board, so arranged that it placed him slightly off-balance. His arms were clamped into position on the reaction boards. Thus in order not to cause serious discomforture to his arms, *S* was forced to sit up against the chest-board. This in turn threw all the anti-gravity muscles of the trunk and neck into a state of heightened tension. The problem and general instructions were the same as used in the relaxed state.

(*c*) *Normal tension: control series.* The *S*s were seated comfortably, with back and head supported. They were instructed to remain alert to the problem, making their discriminations as rapid and as accurate as possible. The same general situation held as in (*a*) and (*b*).

The records of quadriceps tonus during (*a*), (*b*), and (*c*) gave some indication of the general neuro-muscular state of the *S*. The question arises, however, as to whether these records are comparable. This is presumptively true for records were obtained under (*b*) and (*c*), with *S* erect; but the situation in (*a*) produced a different postural adjustment. The work on the physiology of posture has shown that the muscles in question are affected by the position of head and trunk (Walshe).[26] It is not known how much such factors influenced the records. It seems probable, however, that the spread of excitation (except such as was caused by the postural shift) found similar reflection in records obtained under all the conditions.

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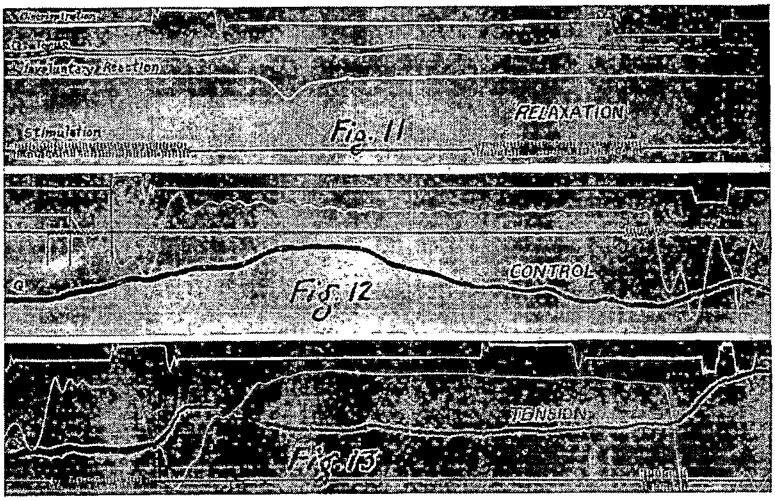
[26] F. M. R. Walshe, The work of Magnus and his collaborators on the nervous regulation of posture, and its bearings on some modern neurological problems, *Med. Sci. Abst. Rev.*, 7, 1923, 1909-118.

Results. The records indicate that there was a marked difference in muscular tension (as recorded from the quadriceps) and in general behavior under the three experimental conditions. Fairly typical responses



FIGS. 11–13. SAMPLE RECORDS MADE IN EXPERIMENT V

Fig. 11 reproduces a record obtained under conditions of relaxation, *i.e.* (*a*). Quadriceps tonus shows characteristic features of the muscle at rest. It will be noted that this *S* withdrew his finger only slightly at the first stimulus and not at all at the second. In the latter case 'reflex' time cannot be reckoned. In this record, the discrimination of the second stimulus is 'wrong' since it is of the same intensity as the first. It will also be noted that the first stimulus was cut off (by *E*) before *S* withdrew his finger. This is because *E*'s cue to release his key is the sound made by *S* in the discrimination reaction. This reaction usually follows after the automatic withdrawal. During the relaxation series it was found advisable to continue stimulation slightly after hearing the discrimination reaction.

Fig. 12 reproduces a record obtained under control conditions, *i.e.* (*c*). Not only is finger withdrawal more rapid under these conditions, but the amplitude is much greater. The discrimination reaction is 'right' since in this record the 'stronger' stimulus followed the 'weaker.' The method of reading 'reflex' and discrimination times is also indicated on this record.

Fig. 13 reproduces a record obtained under conditions of hyper-tension, *i.e.* (*b*). Prior to the first stimulus, *S* has made anticipatory reactions both with the discrimination-key and with the right index finger. The extent of withdrawal is pronounced. Another anticipatory reaction occurs before the second stimulus and the discrimination of the actual shock is corrected and recorrected.

are shown in Figs. 11, 12, and 13. In these records the uppermost white line gives the *S*'s discrimination-reaction. The variable white line shows the amount of finger withdrawal upon shock. The record of quadriceps tonus is easily recognized, as is the period of electrical stimulation. The fact that the amplitude of this line varies in the records is due to differences in threshold values. The upright time lines (produced by the neon tube) are less distinguishable. The horizontal reference lines are 5 mm. apart.

The quantitative treatment of the data has been influenced by arbitrary decisions which are readily appreciated. (1) In obtaining the average individual scores, reactions to both intensities of shock have been considered together. This is permissible since the number of reactions to 'weak' and 'strong' stimuli is the same in all three experimental situations. (2) The tonus scores are the average of the mean values obtaining in each record. These values (in mm. of tendon deflection per sec.) are reckoned from the point in the pre-work baseline just prior to the first stimulation. Increments in tension are thus represented by plus signs and decrements by minuses. (3) For purposes of exposition, the withdrawal of the right finger has been designated as 'reflex' on 'non-voluntary' to distinguish it from the more deliberative discrimination-reaction. This is somewhat misleading; the average 'reflex' time values for relaxation, tension, and control series are, respectively, 401, 247, and 259 sigma—all within the range of voluntary reaction-time. They are considerably greater than the averages reported by Miller in an experiment somewhat similiar to our own.[27] The times obtained by Miller for relaxation and control series are, respectively, 148 and 162 sigma. The reason for these differences between our results of those of Miller lies presumptively in the intensity of stimuli used, our stimuli being relatively weak. Plotting the frequency distribution of reaction-times obtained in our control series showed a bimodal curve in the case of 4 *S*s. This indicates that our stimuli were not always intense enough to cause a rapid non-voluntary reaction. (4) Occasionally, no observable reaction was made under relaxation. These cases have been eliminated from the quantitative comparisons. (5) In such instances as where the *S* made a wrong reaction but subsequently corrected it, discrimination-time was reckoned in terms of the correct reaction. (6) All reactions with the right hand have been averaged together, although only the last reaction of the pair rightly denotes a 'discrimination.' A comparison of the reaction-time to the first and last of the paired stimuli showed an average latency of 18.3 sigma for the second reaction.

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[27] Miller, *op. cit.*, 34.

TABLE XIII

Reaction and Tonus Scores During Relaxation

Time order	S	Reaction-time (in σ)		Ext. withdrawal (in mm.)		Disc.-time (in σ)		Disc. acc. (in %)	Tonus (in mm.)
		Av.	m.v.	Av.	m.v.	Av.	m.v.		
1	*Be*	370	66	16.0	7.0	870	103	.7	−20.6
	Bl	207	76	1.6	0.4	523	84	.6	−17.8
	Wt	454	82	1.1	0.9	624	93	.6	−71.6
2	*Kn*	483	54	24.7	8.2	845	68	.8	−17.3
	Wg	465	77	1.5	1.8	508	79	.6	−14.6
	Wo	544	72	7.2	3.0	598	73	.7	−21.9
3	*Br*	240	85	17.1	13.0	457	94	.8	−17.3
	Fr	205	72	17.7	8.0	525	106	.55	−12.3
	Or	646	102	5.1	4.0	756	86	.45	−68.8
	Av.	401	76.2	10.3	5.14	634	85.1	.65	−29.1

Time of withdrawal. A comparison of Tables XIII and XV shows that the time of the 'non-voluntary' finger withdrawal from shock is greatly lengthened under relaxation, for 7 of the 9 *S*s. It is about the same for *Bl* and less for *Br*. These scores might be due to practice effects, since inspection shows that *Br* took the relaxation-test on the third day and the control test on the second day. We are inclined, however, to attribute the deviation to the degree of relaxation present. Table XIII shows that *Br* was among the least relaxed of the *S*s.

Our differences between our relaxation and normal scores are more pronounced than Miller's. While this might be due to differences in the intensity of stimulation, we are inclined to emphasize the dissimilarity of the control procedures. It will be recalled that Miller allowed her *S*s to remain lying on the couch during the control series, with the negative instruction, "Do not be relaxed this time." In contrast, our *S*s sat erect and received the positive instruction to be 'alert and expectant.' This latter condition more nearly approximated our tension series than

TABLE XIV

Reaction and Tonus Scores During Tension

Time order	S	Reaction-time (in σ)		Ext. withdrawal (in mm.)		Disc-time (in σ)		Disc. acc. (in %)	Tonus (in mm.)
		Av.	m.v.	Av.	m.v.	Av.	m.v.		
1	*Br*	305	95	33.1	18.0	317	63	.55	+ 59.9
	Fr	164	38	40.0	10.2	207	41	.45	+ 47.4
	Or	299	52	24.0	11.0	222	56	.75	+ 66.6
2	*Be*	146	56	43.8	16.1	336	65	.9	+ 91.8
	Bl	145	40	43.7	11.0	290	22	.5	+ 76.6
	Wt	323	61	3.7	1.9	473	65	1.0	+ 17.7
3	*Kn*	271	48	17.1	3.2	396	54	.5	+ 117.6
	Wg	260	50	22.4	6.1	620	63	.9	+ 76.4
	Wo	313	64	19.6	6.2	335	68	.7	+ 30.2
	Av.	247	56	27.5	9.3	355	55	.69	+ 64.9

TABLE XV

REACTION AND TONUS SCORES IN CONTROL SERIES

Time order	*S*	Reaction-time. (in σ)		Ext. withdrawal (in mm.)		Disc.-time (in σ)		Disc. acc. (in %)	Tonus (in mm.)
		Av.	m.v.	Av.	m.v.	Av.	m.v.		
	Kn	295	57	50.7	12.1	755	68	0.9	+16.5
1	*Wg*	264	41	17.4	3.5	454	31	0.9	+21.1
	Wo	260	53	18.2	6.0	278	47	0.9	+22.3
	Br	315	65	34.2	12.0	381	63	1.0	+ 6.9
2	*Fr*	165	30	16.1	7.2	346	65	0.9	+22.9
	Or	318	50	30.4	10.0	432	76	1.0	+ 2.8
	Be	228	44	38.4	13.0	578	48	1.0	+25.8
3	*Bl*	208	58	39.1	10.3	358	55	1.0	+26.9
	Wt	279	45	7.6	2.1	398	49	1.0	+18.2
	Av.	259	49	28.0	8.5	442	56	0.97	+18.2

our relaxation, a fact that may explain why the difference in reaction-time is so slight between the tension and control series. The average time is less under tension for 6 of the 9 *S*s. It is about the same in both tension and control series for *Fr* and greater for *Wt* and *Wo*. Again, changes in recorded quadriceps tension indicate that *Fr*, *Wt*, and *Wo* were less tense in the tension series than were the other *S*s. Compare, for example, the average tension and reaction scores for *Kn* and *Wt* under control and tension series. While *Wt* is about as tense in the control as in the tension series, *Kn*'s tension is increased. *Wt*'s reaction-time is greater in the tension series; but *Kn*'s is reduced. This observation would argue for the facilitative effect of tension.

Extent of withdrawal. The extent finger withdrawal also proves to be a valuable indicator of reactivity under the three experimental conditions. Tables XIII, XIV, and XV show that the average was 10.3 mm. under relaxation, 27.5 mm. under tension, and 28 mm. in the control series. All *S*s except *Fr* showed a marked reduction under relaxation. While *Br*, *Or*, *Wt*, and *Kn* show less extensive movement under tension, their scores show a greater variability than was found in the control series. The very slight difference between the tension and control scores again indicates that the experimentally induced tension had relatively less effect upon the non-voluntary reaction to shock than did the state of experimentally induced relaxation.

Discrimination-reactions. If there was any doubt concerning the advisability of including this complication in the experimental situation, the end has justified the means. We have seen that the speed and amplitude of non-voluntary reaction was about the same under both tension and normal conditions. The discrimination time, however, is considerably reduced under tension. The discrimination is 97% accurate in the control series and only 69% accurate under tension. Under relaxation, the discrimination time is notably increased. Accuracy suffers as well, for only 65% were correct. The indication is that tension affects accuracy of discrimination in a manner analogous to the results of Experiment II.

Tonus changes. We have found the recorded changes in quadriceps tonus a helpful means for ascertaining the relative efficacy of the experimental variables in different *S*s. In fact, a comparison of the tonus changes aided materially in the interpretation of the *S*'s performance. In the control series *Br* and *Or* show very slight tonus changes. May these not be related to their low performance scores? Space does not permit the citation of further examples from the tables. Our suggestion points to the necessity for more detailed work.

Effects of progressive relaxation. Jacobson has reported the gradual diminution of conscious processes as the *S* relaxes progressively.[28] Since the recorded changes in tonus indicate that our *S*s were most relaxed at the end of the experiment, it has been decided to discuss certain differential effects apparently exerted by the neuromuscular state in the course of the session. For the sake of simplicity and to conserve space, these results are plotted in the form of composite curves. Fig. 14 shows the average tonus and non-voluntary reaction scores during the first 10 repetitions of the stimulus pairs. These presentations were separated by intervals of 3 min. and are represented on the abscissa of the graph. The ordinate carries both the performance scores (in sigma) and the tonus scores (in mm. of deflection). The reaction scores used in deriving the curve are to the first stimulus of the pair. Only 10 presentations are plotted, since the presence of 'non-reaction' after this point makes further compositing impossible.

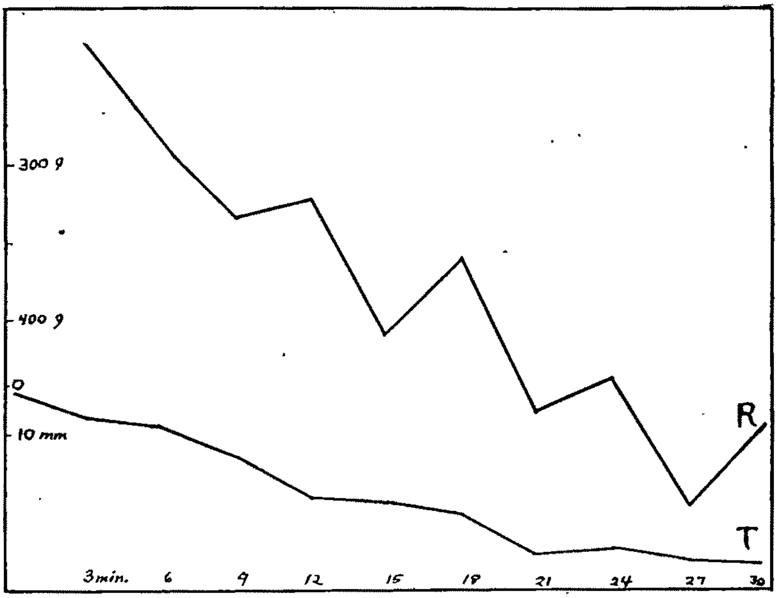

FIG. 14. COMPOSITE TENSION AND REACTION SCORES MADE DURING 30 MINUTES OF RELAXATION

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[28] E. Jacobson, *op. cit.*, this JOURNAL, 36, 1925, 73-87.

Discussion. Discrimination and non-voluntary reactions to two intensities of electric shock were recorded under three situations (a) relaxation, (b) typer-tension, and (c) normal tension. Relaxation was found to lengthen the reaction-time, shorten the extent of finger movement (withdrawal), increase the time and decrease the accuracy of discrimination. Hyper-tension decreased the accuracy of discrimination, while non-voluntary reaction tended to be more rapid. Occasional tests made at the close of the experimental sessions showed that the sensory threshold underwent greater alteration during tension and relaxation than during equivalent periods in the control series. The indication was that the sensory threshold was raised under relaxation and lowered under conditions of tension. This apparently confirms the earlier report of Jacobson,[29] which provided introspective evidence that the intensity of sensations was lessened under relaxation. It is also of interest to point out that White found that there is a higher electrical resistance of the body under relaxation than under tension.[30] This factor is intimately related to threshold values, such as we measured.

A curious phenomena developed with *Wg* in the relaxation series. During the latter half of this session, this *S*, while apparently asleep and showing no non-voluntary reaction to the first intensity of stimulation, continued to perform the discrimination-reaction with the other hand. Questioning of this *S* indicated that he had adopted some sort of 'set' to react to the interval. That such activity can be carried out upon a very low level of reactivity is seen by the fact that one can occasionally 'drive' a car in a hypnogogic state.

We may conclude that while this experiment agrees with the work of Miller—its closest analogy—performance is shown to be affected by hyper-tension as well as by relaxation. The question of whether the two effects are of the same order must be referred to a later discussion.

EXPERIMENT VI

Problem. That muscular contraction, both general and local, may effect ensuing response is now well established. There is an extensive literature on the facilitation and inhibition of reflexes and of voluntary reactions by such means.[31] In the case of voluntary movement, the effect is frequently referred to as the 'law' of dynamogenesis. Very little is known, however, of the relative facilitation received from the contraction of different muscle groups. While the work of Williams[32] and Judd[33] indicates that reaction-time is decreased by preliminary contraction of the reacting muscles, this effect has not been compared with contraction in other muscle groups. The present experiment sought to investigate the following question: Will tension in the reacting muscle facilitate reaction more or less than will tension in a remote group? The performance tested was that of rapid oscillation of the right index finger. The reader will recall that Experiment IV showed that the rate of oscillation was increased slightly when the left arm muscles were under tension. Here the tension of the quadriceps and flexor groups were systematically varied.

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[29] E. Jacobson, *ibid.*, 77, footnote 14.

[30] M. M. White, Relation of bodily tension to electrical resistance, *J. Exper. Psychol.*, 13, 1930, 267-277.

[31] Cf. the summaries in Jenkins, *op. cit.*, 3; and H. Peak, Modification of the lid-reflex by voluntarily induced sets, *Psychol. Monog.*, 42, 1931, (no. 188), 44-46.

Apparatus. In preliminary work, photographic records were taken of the thickening of the quadriceps and the flexors of the fingers. The apparatus used was the same as is described in a previous communication,[34] with the *S* fastened in a modified 'pillory,' and with recording levers resting upon the four muscle groups under observation. In the major experiment records of finger oscillation were obtained by attaching a mechanical counter to the above device.

Procedure. The purpose of the preliminary records of thickening was to observe the spread of excitation incident to placing, in turn, each of the various muscle groups under the same amount of tension. A loosely wound spring with a resistance of approximately 6 lb. would be attached so as to place the desired muscle group under tension. (It was found that the muscle tensions could be kept more constant by this means than by use of weights as in Experiment IV, because of the relative lack of inertia of the spring.)

Four *S*s were used in the major experiment. Each repeated in a different time order, twenty 30-sec. periods of finger oscillation (right index finger) under each of the following experimental variations: (1) flexors of right middle finger under tension, (2) flexors of left middle finger under tension, (3) right quadriceps under tension, and (4) left quadriceps under tension.

Results. The photographic records of muscle thickening showed that the spread of excitation from the focal contraction was largely bilateral; that is, contraction of the right quadriceps resulted in a thickening of the left quadriceps, and the spread from the contraction of the left finger flexors was greatest to the flexors on the right side. This is presumptively due to associated reflex connections between the bilateral groups and is supported by certain observations on the normal knee-jerk,[35] and on the neuromuscular spread of cortically initiated activity.[36] Such bilateral spread aids in explaining the relative amount of reënforcement derived from the contraction of the four muscle groups that we studied. Table XVI shows that the average number of finger oscillations per sec. is most rapid when the experimental tension is localized in a muscle near the reacting group. Oscillation is least rapid when experimental tension is localized in the anatomically and neurologically more remote quadriceps groups.

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[32] F. Williams, *op. cit.*

[33] C. H. Judd, *op. cit.*

[34] *Freeman, op. cit., J. Gen. Psychol.,* 5, 1931, 479-494.

[35] Cf. the summary of this work by G. R. Wendt; An analytic study of the conditioned knee jerk, *Arch. Psychol.,* 18, 1930, 1-97.

[36] Freeman, *loc. cit.*

TABLE XVI

AVERAGE NUMBER OF FINGER OSCILLATIONS PER SECOND WITH EXPERIMENTALLY INDUCED TENSION

Time order	S	Tension localized in			
		R.m. finger	L.m. finger	Right Quad.	Left Quad.
1	*Ga*	6.1	5.6	6.5	6.1
2	*Ka*	5.9	5.8	4.2	5.1
3	*Al*	5.8	5.7	5.1	5.8
4	*Ke*	3.8	4.5	3.3	4.1
	Av.	5.4	5.4	4.8	5.3

Discussion. The fact that subject *Ke* did so much better with the left finger under tension than with the right calls for comment. Since the difference cannot be explained as due to time order, we suggest the increased tension in the reacting members may have worked an inhibition. Had we taken photographic records of the thickening of the various muscle groups, we should be able to supply more exact information upon this point. As it is, we can only give it as our opinion that the subjects followed instructions not to voluntarily increase the tension of other than the group which resisted the pull of the spring.

Our work must be regarded as more exploratory than definitive and our conclusions are necessarily limited to a specific situation. In the case of finger oscillation, our results indicate that the facilitating effect of tension varies inversely with the neurological remoteness of muscle group under tension from the group which is primarily involved in the performance. This would hold, of course, only for equivalent or nearly equivalent amounts of tension in the muscles under survey. The results suggest that the facilitative effect of many 'motor' sets may be due to such optimal localization of tension. An important field of research is thus open to a new avenue of approach.[37]

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[37] We must be very critical of this type of research. Facilitation of any activity will depend upon three factors: (1) qualitative, the locus of the facilitating activity; (2) quantitative, the amount or intensity of the facilitating activity; and (3) temporal, the time relations existing between the onset of facilitating activity and the performance under observation. To obtain the qualitative locus of optimal muscular facilitation, the quantitative and temporal factors must be held constant. While this was partially accomplished in our research, more adequate controls need to be de-

PHYSIOLOGICAL BASIS OF TONIC FACILITATION AND INHIBITION

We have thus far written of favorable influences as instances of facilitation and of unfavorable influences as inhibition. The physiological interpretation of these results, a matter of general interest, is our next concern. We summarize the evidence under four general headings.

(*1*) *Performance accompanied by an increase in recorded muscle tone* (Lombard, Muskens, Tuttle, Freeman, and others). The chief difficulty with this type of evidence is its lack of conclusiveness. For instance, there is just as good reason to suppose a priori that the recorded changes are incidental consequences rather than causal factors in performance. Indeed the first assumption seems the more probable. We know that neural excitation, when intense, may spread to centers not primarily involved in a task, thereby causing expressive movements and the like. But if this were the case, we should expect a slight latency in recorded tonus changes as well as a rising curve during performance; whereas, study of the time relations indicates that maximal tension usually coincides with task initiation or else is slightly anticipatory (Golla and Antonovitch) (Freeman) and the rising curve is not a predominating feature of recorded changes. These facts make it seem that the tonus accompaniments of work are a part of the 'set' for activity rather than the overflow of excitation from centers already in action. This question cannot be settled on the basis of the evidence offered.

(*2*) *Comparison of recorded tonus changes with performance during variations in mental effort* (Golla and Antonovitch, Bills, and Experiments I and II above). Effort varies with the *S*'s estimation of task difficulty and his motivation. Assuming the subject to be highly motivated, variations in the difficulty of a task produce significant correlations between recorded tension and performance scores. Golla and Antonovitch report negative results; but Bills showed that tension increased in the more difficult parts of maze learning by adults, and the present report showed that increased tension incident to preparation for the more difficult tasks was accompanied by increased efficiency in performance. In Experiment II above, tension was notably increased under conditions of high-pressure motivation with a detrimental effect upon performance. All these instances of facilitation and inhibition provide little information upon the mechanisms involved.

(*3*) *Comparison of individual differences in performance with recorded tonus changes* (Duffy, Johnson, Bills, Freeman, and Freeman and Lindley). The work of Bills and Freeman shows that adults with high tension scores perform as well if not better than individuals with low tension scores. This is somewhat contradictory to the results obtained by Duffy on children. The difference is more apparent than real. The chief difficulty experienced by children with high tension scores was in the coördination of movements. Since it is possible that adults may achieve coördination in spite of high tension, this excess might well become facilitative. The ques-

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vised. Their necessity is recognized by other workers. Thus Peak (*op. cit.*, 53 f.), in observing that the amplitude of the lid-reflex to sound was greater under the 'lid-voluntary' than the 'finger-voluntary' reaction set, points out that her inference that the more proximate set has the greatest effect cannot be substantiated "by an experiment which does not control both the intensity of the facilitating activity and its temporal relation to the reflex in question."

tion of whether a given amount of tension will not inhibit performance in one individual and facilitate it in another cannot be answered by the evidence available. The necessity for further work is evident.

(4) *Comparison of performance under different states of experimentally induced tension* (Bills, Miller, Jacobson, and Experiments II, IV, V, and VI above). The most convincing evidence of muscular facilitation and inhibition comes from studies of this sort. Tension or relaxation is set up experimentally and performance under this condition is compared with a control series. Jacobson found that an *S* showed a diminution of conscious processes and reflex excitability under relaxation. Miller carried the problem further and reports that the effect of relaxation is to increase the reaction-time to electric shock, reduce the extent of movement and diminish the apparent intensity of the stimulus. Conversely, Bills demonstrated that the rate of learning various tasks was facilitated by increasing the tension experimentally. In Experiment V above, the effects of hyper-tension and of relaxation were investigated in the same *Ss*. Irritability was lowered by relaxation and raised by hypertension. The accuracy of discrimination suffered under both of these extreme muscular conditions. Experiment IV indicated that the facilitative effect of tension depends somewhat upon the performance under survey. While finger oscillation was not effected by an excess of tension, a coördinated pursuit task was inhibited. Experiment III showed that the facilitative effect of preliminary contraction of the reacting muscle upon reaction-time. Experiment VI indicated that the facilitative effect of muscular tension (during finger oscillation) varies inversely with the remoteness of the contracting muscle from the reacting member. A careful consideration of the controls utilized in the above-mentioned experiments convinces one of the validity of muscular facilitation as the normal accompaniment of performance. Detrimental effects result from hyper- and hypo-tension.

We are now concerned with the neural energy transformations which produce the effects just described. The mechanism of tonic muscular facilitation is not difficult to conceive. If facilitation is defined as the increase of neural action by increased stimulation, the summation of impulses will explain the reënforcement of higher neural arcs by muscular tension. It is when we attempt to push our analysis further that the relation becomes less clear. Where and in what manner does this summation occur? It is a well-known fact that there is summation of impulses on the motor horn cells and in the cerebral cortex as well as on the afferent side.[38] Cortical summation of proprioceptive with impulses of other origin apparently occurred in many of our experiments. The facilitation of selected acts by the preliminary contraction of the reacting muscle groups indicates that summation may also have occurred on the efferent side of the arc. The condition of optimal facilitation found in Experiment VI was probably favored by the proximity of the excitation to the efferent part of the reaction pattern. That is, the effects of equivalent excitation in more remote neurological groups is largely dissipated before its 'spread' includes the reacting member. Where there is no associated reflex connection between the muscles under tension and those directly engaged in the performance, whatever facilitation does occur will likely result from summation at the cortical level.

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[38] F. H. Pike and M. N. Chappell, *Science,* 71, 1930; M. N. Chappell, *Psychol. Rev.,* 38, 1931, 317-332. The original articles are given in these papers.

The complexity of nervous organization permits the possibility of summation at any and all levels. Jacobson has discussed at length the rôle of various portions of the nervous system in the regulation of tone.[39] The long-circuiting of impulses favors the view that cortical centers play an important part in such regulation. The present writer has reviewed the evidence for assuming that the action of cortical centers is in turn regulated by afferent impulses of proprioceptive origin.[40] Reënforcement of reaction patterns by muscular tension is a common if not universal occurrence. The effect is, of course, more obvious in some performances than in others and depends partially upon their nature. Muscular acts would normally require more reënforcement than would action patterns limited almost exclusively to cerebral fields. Our records offer proof that such 'mental' reactions frequently involve an increase of excitation in a remote muscle group. We do not wish to imply, however, that every reaction must be accompanied by an increase in tone. The neural mechanism is more than a mere touch-and-go system and normal waking tone may provide sufficient reënforcement for many reactions without recourse to additional increment.

It may be useful to presume that tonic facilitation is quite automatic during normal waking adjustment, whereas those increments in tension of which we are often aware during periods of stress are of more voluntary origin. Of course it does not matter whether one says "I will contract my muscles because it will increase my efficiency," or whether one finds that his muscles are contracted without such voluntary direction. The crucial fact is that in both cases the reënforcement of performance may occur.

When we consider the problem of inhibition as related to our topic we are upon more difficult grounds. The inhibitory effects of tension are of two kinds (a) decreased efficiency with decreased tension, and (b) decreased efficiency with increased tension. Now decreased neural action by decreased stimulation is as axiomatic as its converse, increased neural action by increased stimulation. But decreased action by increased stimulation needs explanation. We can understand how the diminution of afferent impulses from the muscles may diminish production through lack of necessary reënforcement. This was apparently the case in Experiments II and V. The inhibitory effect of hyper-tension in part (b) of these two experiments is more difficult to understand.

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[39] E. Jacobson, *Progressive Relaxation*, 1929, 220-269.

[40] G. L. Freeman, Mental activity and the muscular processes, *Psychol. Rev.*, 38, 1931, 428-449.

Dodge[41] lists the following varieties of reaction decrement from increased stimulation: (1) terminal or peripheral inhibition; (2) reflex inhibition or the reciprocal effects of antagonistic muscle action; (3) refractory phase inhibition, where the second stimulus falls within the relative refractory period of reaction to the first; (4) inhibition by rivalry of impulses in competition for the final common path; (5) inhibition of weaker by stronger stimuli (Hyman's Law); (6) anode block, where a region of decrement is produced by persistent electrical stimulation; and (7) protracted central inhibition consequent to frequent stimulation. To this we might add another (8) inhibition by occlusion,[42] where the combined impulses from two sources exceeds that sufficient for full tetanic contraction of a muscle and so gives contraction decrement by overlapping excitatory effects.

With the problem of inhibition in its present confused state, it is difficult to say which, if any, of these varieties can be applied to diminished response with increased tension. Certain analogies can be drawn from each of the above descriptions, but the last variety seems most suggestive in the present context. Under this analogy, the neural pattern is conceived to permit a certain ratio in the summed impulses which sustain it. If the number of these impulses exceeds such limits, the pattern is 'fogged up' with a consequent decrement in performance. While the higher neural integrations demand a certain amount of sustaining action from the muscular processes, an excess of this action—by its very prominence in the neural flux—disturbs the equilibrium of the organization. The proprioceptive impulses do not gain entire control of the reacting mechanism, as would follow from the 'rivalry' analogy; rather they cut down or limit the efficacy of the total reaction pattern.

The details of this analogy are necessarily vague. Yet the principle can be profitably applied to a number of facts which we have observed. Consider, for example, the fact that a performance requiring a high degree of coördination is 'inhibited' by hyper-tension, while a less difficult task may be 'facilitated' under similar conditions (Experiment IV). May not the first pattern be of such nature that the visual and kinaesthetic impulses must preserve a definite balance in order for manual pursuit to be properly executed? The effect of increasing the general background of tension might be that selected kinaesthetic impulses (from the reacting member) become confused with other impressions of this same modality and so lose some of their directive control.

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[41] R. Dodge, The problem of inhibition, *Psychol. Rev.*, 33, 1926, 1-12.

[42] C. S. Sherrington, Quantitative management of contraction in lowest level coordination, *Brain*, 54, 1931, 1-28.

Much remains to be known of the neural mechanisms responsible for the effects observed in our own and others' experiments; and although this aspect is particularly dependent upon the results of physiological experimentation, the psychologist must assume his share of the responsibility. We can no longer be content with the non-technical use of the terms *facilitation* and *inhibition* as applied to the differential effects of tension upon performance.

FURTHER EXPERIMENTS IN PHONETIC SYMBOLISM

By STANLEY S. NEWMAN, University of Chicago

Many persons believe that phonetic elements, apart from their function in a particular linguistic context, carry with them certain symbolic connotations.[1] The vowels *o* and *u*, for instance, are said to be heavier, gloomier, more ponderous than *i* and *e*.

Although one might extend indefinitely this type of phonetic interpretation, one cannot escape the conviction that normal individuals would tend to disagree considerably in the specific symbolism associated with any vowel or consonant. However true this suspicion of individual difference may be, it does not follow that phonetic sounds in themselves have no consistent symbolic relation outside of their linguistic associations. The two problems are generally confused, and both are branded with the stigma of being unreliably intuitive. The point is stressed because I found, from speaking to a number of the students acting as subjects in this investigation, that their assurance on this matter was at striking variance with their reactions in the experiments. They seemed to feel that the process disclosing the symbolic implications of phonetic elements was in some way a highly variable and arbitrary intuitive association. And this point of view, I am sure, reflects common opinion.

But the practical use of this extra-linguistic function of phonetic sounds is familiar enough in literature. The contemporary minor poet, for example, uses great ingenuity in employing this device to gain his effects.

This paper is an attempt to study the patterning of phonetic symbolisms on a non-linguistic plane. For this purpose a schedule was devised employing paired nonsense words. The pairs were so arranged that they would be similar in every respect but for the two vowels or consonants that were to be contrasted, *e.g. glupa: glopa*. The arbitrary meaning "horse"

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[1] Edward Sapir, A study in phonetic symbolism, *J. Exper. Psychol.*, 12, 1929, 225-239. I am deeply indebted to Professor Sapir for his assistance in this study. The data of Experiment I were collected by him and handed over to me for inclusion in this paper; a preliminary analysis of some of that material will be found on pages 229-236 of his article.

was applied to this pair, and the subjects were asked to choose whether the first word of the pair signified to them the larger or the smaller horse. The subjects were given a sheet with consecutive pairs of numbers; after the investigator pronounced the word pair and gave the arbitrary meaning, the subjects recorded their vote by checking the appropriate number.

TABLE I

Key to Phonetic Symbols

Experiment I		Experiment II			
i	as in French *fini*	*ι*	as in English *fit*	c	as *sh* in English *short*
e	“ “ “ *été*	*i·*	“ “ “ *seize*	*tc*	“ *ch* “ “ *churn*
ϵ	“ “ English *met*	*ai*	“ “ “ *right*	*θ*	“ *th* “ “ *thin*
ä	“ “ “ *hat*	*ei*	“ “ “ *rate*	*δ*	“ *th* “ “ *then*
a	“ “ German *Mann*	*yu·*	“ “ “ *huge*	*j*	“ *z* “ “ *azure*
u	“ “ English *put*	*ä·*	“ “ “ *sad*	*dj*	“ *g* “ “ *germ*
o	“ “ French *tôt*	*u·*	“ “ “ *pool*		
ɔ	“ “ “ *note*	*a·*	“ “ “ *father*	All other consonantal symbols had their common English value.	
		ɔ·	“ “ “ *tall*		

The statistical method employed in treating the data is a psychophysical method arranged by L. L. Thurstone.[2] The procedure consists essentially of reducing the experimental proportions to sigma scores on a normal curve of distribution. The mean sigma separations between adjacent stimuli are computed, and a final scale is then made from the accumulated steps of the individual stimuli.

Experiment I. One hundred word-pairs were used. Each phonetic contrast, however, was presented more than once throughout the hundred pairs; so that the proportions (See Table II) are average percentages of 2 to 5 responses for the given contrast.

The symbolism, on the basis of which the *S*s were to make their choice, was that of large versus small.

A variable was introduced in the form of concept classes; the arbitrary meanings of the first 40 pairs were nouns (large versus small objects), the next 10 pairs were verbal subjects (adult versus child activity), the next 10 were adjectives (qualities as applied to large versus small objects), etc. But a comparison of the proportions on the basis of these conceptual categories showed that the variable had no apparent effect on the responses.

The major purpose of this experiment, then, was to discover whether individuals tended to pattern phonetic sounds on the basis of size symbolism. Another purpose was to find whether age differences affected the patterning. The 606 *Ss*[3] taking part in the experiment were divided into three age groups:

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[2] For a more complete exposition of method see L. L. Thurstone, An experimental study of nationality preferences, *J. Gen. Psychol.,* 1, 1928, 405-425. The reader will find a detailed analysis of the logic underlying this procedure in other articles by the same author. Professor Thurstone has generously given personal advice on many points of statistical method involved in this paper.

Group 1 9-13 yr. old (223 *Ss*)
Group 2 14-15 yr. old (218 *Ss*)
Group 3 16 yr. old and above (153 *Ss*)

The consonant pairs were not of a sufficient number to permit the calculation of a scale, but will be used in Experiment II.

The tabulated results may be interpreted as follows. In Group 1, 32.2% of the *Ss* voted that *i* was larger than *e* (Proportion $i > e = 0.322$, Table

TABLE II

EXPERIMENTAL PROPORTIONS

		i	*e*	*ɛ*	*ä*	*a*	*u*	*o*	*ɔ*
Group 1	*i*		.678	.757	.812	.802	.772		
	e	.322		.564		.742		.732	
	ɛ	.243	.436		.713	.808			.782
	ä	.188		.287		.576	.637	.660	.717
	a	.198	.258	.192	.424		.446	.600	.656
	u	.228			.363	.554		.666	.595
	o		.268		.340	.400	.334		.497
	ɔ			.218	.283	.344	.405	.503	
Group 2	*i*		.705	.774	.801	.828	.871		
	e	.295		.592		.705		.761	
	ɛ	.226	.408		.718	.746			.847
	ä	.199		.282		.579	.591	.661	.659
	a	.172	.295	.254	.421		.486	.598	.642
	u	.129			.409	.514		.656	.595
	o		.239		.339	.402	.344		.450
	ɔ			.153	.341	.358	.405	.550	
Group 3	*i*		.708	.796	.864	.804	.870		
	e	.292		.593		.692		.748	
	ɛ	.204	.407		.742	.747			.806
	ä	.136		.258		.577	.559	.637	.706
	a	.196	.308	.253	.423		.501	.606	.567
	u	.130			.441	.499		.598	.590
	o		.252		.363	.394	.402		.516
	ɔ			.194	.294	.433	.410	.484	
Average	*i*		.696	.773	.821	.812	.834		
	e	.304		.582		.715		.747	
	ɛ	.227	.418		.722	.769			.812
	ä	.179		.278		.577	.600	.654	.693
	a	.188	.285	.231	.423		.475	.601	.628
	u	.166			.400	.525		.645	.594
	o		.253		.346	.399	.355		.485
	ɔ			.188	.307	.372	.406	.515	

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[3] Not included in this grouping are twelve schedules received from Chinese students; these will be held until further work is done with phonetic symbolisms among foreign-speaking individuals.

TABLE III

SIGMA SCORES

		i	*e*	*e*	*ϵ*	*ϵ*	*ä*	*ä*	*u*	*u*	*a*	*a*	*o*	*o*	*ɔ*	Scale Separations	
Group 1	*i*	.00	.46	.46	.70	.70	.89	.89	.75	.75	.85						
	e	−.46	.00	.00	.16							.65	.62			*i-e*	.5869
	ϵ	−.70	−.16	−.16	.00	.00	.56									*e-ϵ*	.1202
	ä					−.56	.00	.00	.35	.35	.19	.19	.41	.41	.57	*ϵ-ä*	.6222
	a	−.85	−.65	−.65	−.87	−.87	−.19	−.19	−.14	−.14	.00	.00	.25	.25	.40	*ä-u*	.2168
	u							−.35	.00	.00	.14	.14	.43	.43	.24	*u-a*	.0566
	o							−.41	−.43	−.43	−.25	−.25	.00	.00	.01	*a-o*	.3229
	ɔ					−.78	−.57	−.57	−.24	−.24	−.40	−.40	−.01	−.01	.00	*o-ɔ*	.0396
		−2.01	−.35	−.35	−.01	−1.51	.69	−.63	.29	.29	.53	.33	1.70	1.08	1.22		

		i	*e*	*e*	*ϵ*	*ϵ*	*ä*	*ä*	*a*	*a*	*u*	*u*	*ɔ*	*ɔ*	*o*	Scale Separations	
Group 2	*i*	.00	.54	.54	.75	.75	.85	.85	.95	.95	1.13						
	e	−.54	.00	.00	.23											*i-e*	.7106
	ϵ	−.75	−.23	−.23	.00	.00	.58	.58	.66							*e-ϵ*	.1944
	ä					−.58	.00	.00	.20	.20	.23	.23	.41	.41	.42	*ϵ-ä*	.6590
	a	−.97	−.54	−.54	−.66	−.66	−.20	−.20	.00	.00	−.04	−.04	.36	.36	.25	*ä-a*	.2162
	u							−.23	.04	.04	.00	.00	.24	.24	.40	*a-u*	.0236
	o							−.42	−.25	−.25	−.40	−.40	−.13	−.13	.00	*u-ɔ*	.3762
	ɔ					−1.02	−.41	−.41	−.36	−.36	−.24	−.24	.00	.00	.13	*ɔ-o*	.0905
		−2.24	−.23	−.23	.32	−1.51	.82	.17	1.24	.58	.68	−.45	.88	.88	1.20		

TABLE III (continued)

		i	e	e	ϵ	ϵ	ä	ä	a	a	u	u	o	o	ɔ	Scale Separations	
Group 3	i	.00	.55	.55	.83	.83	1.10	1.10	.86	.86	1.13						
	e	−.55	.00	.00	.24											i-e	.7248
	ϵ	−.83	−.24	−.24	.00	.00	.65	.65	.66							e-ϵ	.2121
	ä					−.65	.00	.00	.19	.19	.15	.15	.35	.35	.54	ϵ-ä	.6675
	a	−.86	−.50	−.50	−.66	−.66	−.19	−.19	.00	.00	.00	.00	.27	.27	.17	ä-a	.1515
	u							−.15	.00	.00	.00	.00	.25	.25	.23	a-u	.0448
	o							−.35	−.27	−.27	−.25	−.25	.00	.00	.04	u-o	.3281
	ɔ					−.86	−.54	−.54	−.17	−.17	−.23	−.23	−.04	−.04	.00	o-ɔ	.0424
		−2.24	−.19	−.19	.41	−1.34	1.02	.52	1.27	.61	.80	−.33	.83	.83	.98		

		i	e	e	ϵ	ϵ	ä	ä	a	a	u	u	ɔ	ɔ	o	Scale Separations	
Average	i	.00	.51	.51	.75	.75	.92	.92	.89	.89	.97						
	e	−.51	.00	.00	.21											i-e	.6647
	ϵ	−.75	−.21	−.21	.00	.00	.59	.59	.74							e-ϵ	.1728
	ä					−.59	.00	.00	.19	.19	.25	.25	.50	.50	.40	ϵ-ä	.6477
	a	−.89	−.57	−.57	−.74	−.74	−.19	−.19	.00	.00	−.06	−.06	.33	.33	.26	ä-a	.2274
	u							−.25	.06	.06	.00	.00	.24	.24	.37	a-u	.0000
	o							−.40	−.26	−.26	−.37	−.37	−.04	−.04	.00	u-ɔ	.3841
	ɔ					−.89	−.50	−.50	−.33	−.33	−.24	−.24	.00	.00	.04	ɔ-o	.0113
		−2.15	−.27	−.27	.22	−1.47	.82	.17	1.29	.55	.55	−.42	1.03	1.03	1.07		

II, top of *i*-column). This proportion corresponds to the sigma score —0.46 (Table III). The scale separation between *i* and *e* is the mean difference between the repeated sigma scores of the two stimuli multiplied by $2^{1/2}$, the discriminal errors of the stimuli.[4]

$$
\begin{aligned}
S_1 - S_2 &= x_{12}(2)^{1/2} \\
&= [(-.35 \times 2.01)/4](2)^{1/2} \\
&= (1.66\,'4)(2)^{1/2} \\
&= .415 \times (2)^{1/2} \\
&= .5869
\end{aligned}
$$

The scale values are derived from the accumulated scale separations between adjacent stimuli (Table IV). The position of any vowel on the symbolic scale is a medial point computed from its various positions when contrasted with individual vowels.

TABLE IV

Scale Values

	Group 1	Group 2	Group 3	Av.	Median*
i	.0000	.0000	.0000	.0000	.0000
e	.5869	.7106	.7248	.6647	.7106
ɛ	.7071	.9050	.9369	.8375	.9050
ä	1.3293	1.5640	1.6044	1.4852	1.5640
a	1.6027	1.7802	1.7559	1.7126	1.7802
u	1.5461	1.8038	1.8007	1.7126	1.8038
o	1.9256	2.2705	2.1288	2.1080	2.1833
ɔ	1.9652	2.1800	2.1712	2.0967	2.2229

* The Median Scale Values are determined on the basis of median scale separations.

It is further possible to determine the range of shifting that each item undergoes: thus, *i* will take one position on the scale when contrasted with *e*, another when contrasted with ɛ, etc. The scale separation 0.5869, or 0.415 σ, between *i* and *e* corresponds to the proportion 0.339 for *i*. The difference between the experimental proportion and the calculated proportion (Pe—Pc) is —0.017 (.322—.339 = —.017). Such a method of computing discrepancies is, as one may assume, a test of the internal consistency of the responses.

The subjective scales of the three groups reveal the same essential patterning (Fig. 1). The vowel *i* always occurs relatively far below the succeeding vowel *e*, which is followed rather closely by ɛ, after which comes *ä* separated by another considerable distance. Slight inconsistencies appear in the allocation of *a* against *u* and again in *o* against ɔ; but these two groups of vowels are close together on each of the three scales, and the inconsistencies, falling well within the average discrepancy area, are further reflected in a comparison of discrepancy ranks.

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[4] Thurstone, *op. cit.*, p. 407.

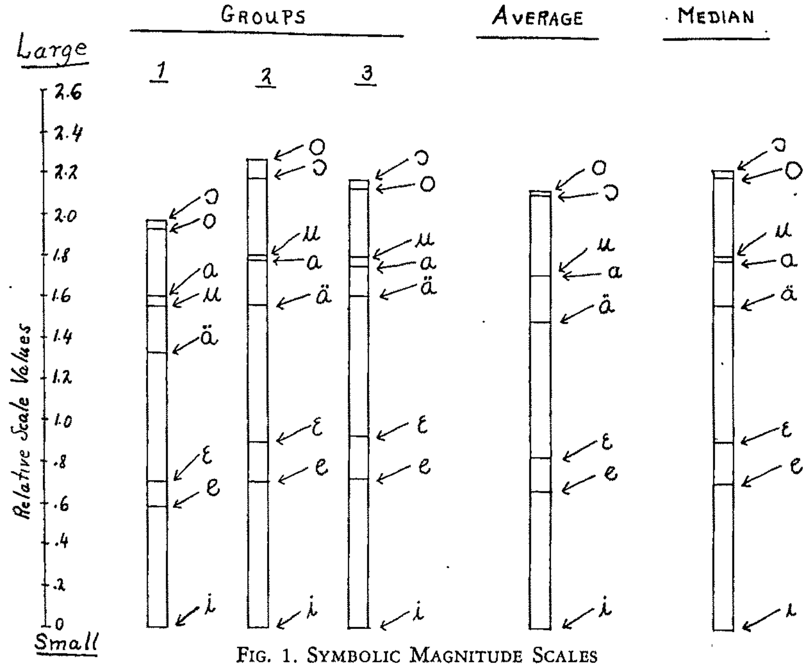

Fig. 1. Symbolic Magnitude Scales

The discrepancies in many ways tell a significant story. An examination of the average discrepancies (Table V) indicates that the youngest group shows the greatest degree of inconsistency (0.041), the older group displays less (0.035), and the oldest group has the least (0.032). This is a situation one might expect; and it bears out the fact that the scale separations of Group 1 are the smallest, this group tending to discriminate between the vowels less distinctly than the other groups. But it should be observed that these scale and discrepancy differences between the age-groups are only slight differences in the clarity and stability of discriminal perception, not in fundamental pattern. In other words, the youngest group has substantially the same subjective pattern as the older groups, but it tends to discriminate less sharply and less consistently than they do between the sounds.

A comparison of the individual discrepancies, however, suggests that the use of these to explain the range of instability interprets only a part of the evidence. In the majority of cases the shifting appears to be of the same general trend. A few examples are listed:

	Groups			Av.
	1	2	3	
i>*e*	−.017	−.013	−.012	−.015
i>*ɛ*	−.066	−.035	−.050	−.050
i>*a*	.069	.068	.089	.075
e>*ɛ*	−.030	−.037	−.033	−.033
e>*o*	.096	.104	.092	.099
ɛ>*ä*	−.043	−.039	−.060	−.045
ä>*o*	.003	.030	.008	.016
a>*u*	.038	.021	.012	.025
u>*ɔ*	.021	.010	.013	.013

These phenomena indicate that the discrepancies point not only to chance fluctuations and inconsistencies but to constant shifts. The reasons behind such constant variability cannot be satisfactorily explained from the details of the available data; but it is significant to note that even the low discrepancy of the average scale

TABLE V

DISCREPANCIES

(Pe—Pc)

		i	*e*	*ɛ*	*ä*	*a*	*u*	*o*	*ɔ*	*Total*
Group 1	*i*		.017	.066	—.014	—.069	—.091			
	e	—.017		.030		—.022		—.096		
	ɛ	—.066	—.030		.043	.071			—.031	
	ä	.014		—.043		—.001	.076	—.003	.043	
	a	.069	.022	—.071	.001		—.038	.010	.055	
	u	.091			—.076	.038		.060	—.021	
	o		.096		.003	—.010	—.060		—.014	
	ɔ			.031	—.043	—.055	.021	.014		
Σ(Pe—Pc)		.257	.165	.241	.180	.266	.286	.183	.164	1.742
Av.		.051	.041	.048	.030	.038	.057	.037	.033	.041
Group 2	*i*		.013	.035	—.065	—.068	—.028			
	e	—.013		.037		—.070		—.104		
	ɛ	—.035	—.037		.039	.014			.031	
	ä	.065		—.039		.018	.024	—.030	—.009	
	a	.068	.070	—.014	—.018		—.021	—.038	.031	
	u	.028			—.024	.021		.027	—.010	
	o		.104		.030	.038	—.027		—.025	
	ɔ			—.031	.009	—.031	.010	.025		
Σ(Pe—Pc)		.209	.224	.156	.185	.260	.110	.224	.106	1.474
Av.		.042	.056	.031	.031	.037	.022	.045	.021	.035
Group 3	*i*		.012	.050	—.008	—.089	—.029			
	e	—.012		.033		—.075		—.092		
	ɛ	—.050	—.033		.060	.028			—.003	
	ä	.008		—.060		.034	.004	—.008	.050	
	a	.089	.075	—.028	—.034		—.012	.002	—.048	
	u	.029			—.004	.012		.006	—.013	
	o		.092		.008	—.002	—.006		.004	
	ɔ			.003	—.050	.048	.013	—.004		
Σ(Pe—Pc)		.188	.212	.174	.164	.288	.064	.112	.118	1.330
Av.		.038	.053	.035	.027	.041	.013	.022	.024	.032
Average	*i*		.015	.050	—.032	—.075	—.053			
	e	—.015		.033		—.056		—.099		
	ɛ	—.050	—.033		.045	.037			—.002	
	ä	.032		—.045		.013	.036	—.016	.026	
	a	.075	.056	—.037	—.013		—.025	—.009	.021	
	u	.053			—.036	.025		.035	—.013	
	o		.099		.016	.009	—.035		—.012	
	ɔ			.002	—.026	—.021	.013	.012		
Σ(Pe—Pc)		.225	.203	.167	.168	.236	.162	.171	.074	1.406
Av.		.045	.051	.033	.028	.034	.032	.034	.015	.033

(0.033) is probably much too high if one takes this figure merely as an index of inconsistency. We are evidently dealing with a field of latent symbolism in which the patterning is even more rigid and constant than the discrepancies would lead us to believe.

A consideration of the discrepancies from another point of view helps to allocate the scale-inconsistencies referred to above. The vowel *u* falls below *a* on the scale of Group 1, contradicting the alignment of these vowels on the scales of the other two groups; likewise, the vowel *o*, in appearing above ɔ on the scale of Group 2, falls out of the pattern set by the other two groups. If the 8 vowels are arranged in rank order of their discrepancy-scores within each group, the *u* of Group 1 and the *o* of Group 2 will be found to have a much higher discrepancy rank than these vowels in the other groups. The two starred examples, it will be noted, depart farther from a median discrepancy rating than any of the other examples in the entire set. We may assume, then, that the normal position of *u* is slightly above *a* on the subjective scale, and *o* is slightly below ɔ; since in each case we are taking as the norm the choice of two groups having a low discrepancy-rank for these vowels as against that of one group having a strikingly high discrepancy-rank.

TABLE VI

DISCREPANCY-RANK

	Groups				
	1	2	3	Av.	Median
i	7	6	6	7	6
e	5	8	8	8	8
ϵ	6	4	5	4	5
ä	1	3	4	2	3
a	4	5	7	5	5
u	8*	2	1	3	2
o	3	7*	2	6	3
ɔ	2	1	3	1	2

In general, the rationale which underlies the symbolic magnitude-pattern appears to be of a mechanical nature. The front vowels of the symbolic scale follow the order, *i*, *e*, ɛ, *ä*, *a*. This sequence is similar to a vowel-series following (1) the receding positions of articulation made by the tongue within the mouth, (2) the decreasing frequencies of vocalic resonance as measured acoustically, and (3) the increasing size of the oral cavity used in pronunciation. Thus far the three factors work in unison to present the same picture of a vowel sequence as that found on the symbolic scale. But these factors are at variance in their arrangement of the back vowels. The situation might be schematized as follows:

(1) Kinesthetic factor: articulatory position of tongue (front to back), *i*, *e*, ɛ, *ä*, *a*, ɔ, *o*, *u*.

(2) Acoustic factor: characteristic high frequency of vocalic resonance (high to low),[5] *i, e, ɛ, ä, a, ɔ, o, u.*

(3) Kinesthetic or, possibly, visual factor: size of oral cavity (small to large), *i, e, ɛ, ä, a; u, o, ɔ.*

The symbolic magnitude-pattern conforms to the first two series in placing the back vowels above the front vowels; but it compensates for this arrangement, as it were, by following the sequence of the third series. But the group-scales are not entirely consistent in their sequence of back vowels, as was pointed out above; furthermore, their arrangement of back vowels leans more heavily on the patterns of factors 1 and 2—the matter of placing the back vowels above the front being a more thoroughgoing spatial discrimination than the matter of sequence. For these reasons we may assume that the size of the oral cavity is a less potent factor in judgments of magnitude-symbolism than the accompanying factors of articulatory position and acoustic resonance.

Experiment II. A related experiment was conducted to determine the bearing of unlike symbolisms on the subjective patterns of phonetic sounds. The symbolisms chosen were (A) large versus small, and (B) dark versus bright.

The experimental and statistical procedure was essentially the same as that of Experiment I. A schedule of 113 word-pairs was drawn up, contrasting 9 vowels and 15 consonants. Each contrasting pair of vowels and of consonants was presented only once, in distinction to the previous experiment. Another difference of procedure was that the phonetic elements in this experiment were restricted to English sounds. The purpose of this restriction was to permit, later on, a comparison of the distribution of sounds on a symbolic scale with their distribution in meaningful English words. The *Ss* were asked to choose the symbolically 'larger' word of each pair during the first half of the experiment; the same pairs were then given in random order and the *Ss* were asked to choose the 'darker' word.

TABLE VII

u· Contrasts

Contrasts	*Position*	*Accent*	*Syl. Structure*	
sú·ha:sɩ́ha	1st syl.	acc.	2 syl. word	
lä·bɩ:lu·bɩ	" "	unacc.	" " "	
hú·lɛn:há·lɛn	" "	acc.	" " "	final cons.
péiksa:pú·ksa	" "	"	" " "	cons. doublet
kalú·:kalái	2nd syl.	"	" " "	
dágu·:dágɔ·	" "	unacc.	" " "	
dzu·c:dẓyu·c			1 syl. word	

---

[5] Richard Paget, *Human Speech,* 1930, 86-93, 315-317. No adequate acoustic analysis has been made of the entire vocalic gamut for one voice. This acoustic scale has been assembled from the frequency-data of several voices; it is only an approximation, valid enough as an index of relative position, although it cannot claim any absolute numerical validity.

Because the total experiment contained 226 word-pairs, it was decided to vary the phonetic structure of the words in order to prevent, as far as possible, a deadening of the *Ss'* attention. The contrasted sounds were varied as to position in the word, accent, and syllabic structure. In the vowel-pairs, for instance, the contrasted elements might take any of the forms given in Table VII. But these variations were introduced consistently into the sounds, so that, if they should prove to be conditions affecting the responses, their total effect on the symbolic scale would be constant and uniform, although their individual values could still be computed.

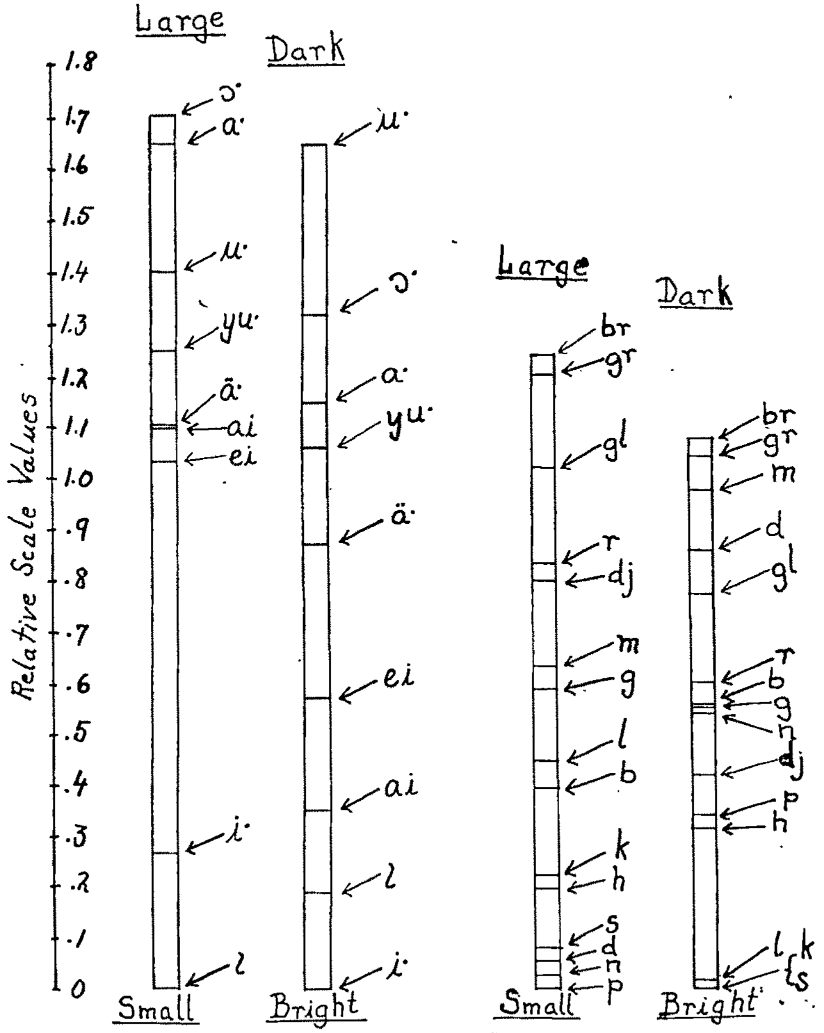

FIG. 2. SYMBOLIC SCALES

TABLE VIII

Experimental Proportions

Experiment 2A (Large vs. Small)

	ι	*i·*	*ei*	*ai*	*ä·*	*yu·*	*u·*	*a·*	*ɔ·*
ι		.456	.732	.775	.872	.721	.832	.920	
i·	.544		.779	.638	.737			.796	.755
ei	.268	.221			.623		.493	.759	.572
ai	.225	.362			.406	.500	.635		.667
ä·	.128	.263	.377	.594		.629	.607		
yu·	.279			.500	.371		.521	.610	.624
u·	.168		.507	.365	.393	.479		.532	.589
a·	.080	.204	.241			.390	.468		.655
ɔ·		.245	.428	.333		.376	.411	.345	

	p	*n*	*d*	*s*	*h*	*k*	*b*	*l*	*g*	*m*	*dj*	*r*	*gl*	*gr*	*br*
p		.511	.279		.593	.518	.773	.594	.851	.780	.580	.667		.809	.794
n	.489		.631	.566		.567	.775	.676	.467	.630	.709	.725	.695	.739	
d	.721	.369		.358					.655	.515	.688	.796	.674	.808	.757
s		.434	.642		.552	.387	.529	.629	.766	.713		.613		.787	.746
h	.461			.448		.544	.471	.736	.586	.714	.553		.746	.645	.766
k	.482	.433		.613	.465		.686	.277	.727		.560		.717	.674	.759
b	.227	.225		.471	.529	.314			.514		.752	.587	.723	.745	.872
l	.406	.471		.371	.264	.723			.442		.555	.650	.681	.745	.627
g	.149	.533	.345	.234	.414	.273	.486	.558		.438		.645	.664	.783	
m	.220	.370	.485	.287	.286				.562		.695	.589	.553	.581	.734
dj	.420	.291	.312		.447	.440	.248	.445		.305		.464	.606		.660
r	.333	.275	.204	.387			.413	.350	.355	.411	.536		.471	.783	
gl		.305	.326		.254	.283	.277	.319	.336	.447	.394	.529			.445
gr	.191	.261	.192	.213	.355	.326	.255	.255	.217	.419		.217			.539
br	.206		.243	.254	.234	.241	.128	.372		.266	.340		.555	.461	

TABLE IX

Experimental Proportions

Experiment 2B (Dark vs. Bright)

	i·	*ι*	*ai*	*ei*	*ä·*	*yu·*	*a·*	*ɔ·*	*u·*
i·		.489	.573	.706	.793		.754	.776	
ι	.511		.515	.537	.626	.765	.772		.846
ai	.427	.485			.567	.659		.755	.836
ei	.294	.463			.625		.689	.706	.689
ä·	.207	.374	.433	.375		.517			.698
yu·		.235	.341		.483		.661	.487	.643
a·	.246	.228		.311		.339		.678	.687
ɔ·	.224		.245	.294		.513	.322		.621
u·		.154	.164	.311	.302	.357	.313	.379	

	k	*s*	*l*	*h*	*p*	*dj*	*n*	*g*	*b*	*r*	*gl*	*d*	*m*	*gr*	*br*
k		.518	.440	.575	.526	.593	.726	.810	.717		.621			.548	.644
s	.482		.543	.544			.625	.548	.748	.625		.831	.654	.705	.755
l	.560	.457		.405	.575	.748	.493	.515		.675	.586			.816	.776
h	.425	.456	.595		.452	.365		.474	.493		.674		.652	.741	.664
p	.474		.425	.548		.544	.507	.513	.522	.500		.500	.754	.694	.646
dj	.407		.252	.635	.456		.595		.395	.640	.652	.684	.716		.379
n	.274	.375	.467		.493	.405		.415	.582	.566	.419	.504	.597	.689	
g	.190	.452	.485	.526	.487		.585		.412	.461	.496	.483	.434	.735	
b	.283	.252		.507	.478	.605	.418	.588		.419	.569			.629	.741
r		.375	.325		.500	.360	.434	.539	.581		.647	.328	.461	.770	
gl	.379		.414	.326		.348	.581	.504	.431	.353		.537	.560		.610
d		.169			.500	.316	.496	.517		.672	.463		.581	.368	.593
m		.346		.348	.246	.284	.403	.566		.539	.440	.419		.544	.478
gr	.452	.295	.184	.259	.306		.311	.265	.371	.230		.632	.456		.614
br	.356	.245	.224	.336	.354	.621			.259		.390	.407	.522	.386	

TABLE X

An Example of Scale Calculation

Vowels of Experiment 2A (Large vs. Small)

Sigma Scores

Original Calculation

	ι	*i·*	*i·*	*ei*	*ei*	*ä·*	*ä·*	*ai*
ι	.00	−.11	−.11	.62	.62	1.14	1.14	.76
i·	.11	.00	.00	.77	.77	.63	.63	.35
ei	−.62	−.77	−.77	.00	.00	.31		
ä·	−1.14	−.63	−.63	−.31	−.31	.00	.00	.24
ai	−.76	−.35					−.24	.00
yu·							−.33	.00
u·					.02	−.27	−.27	−.35
a·	−1.41	−.83	− 83	−.70				
ɔ·			−.69	−.18				
	−3.82	−2.69	−3.03	.20	1.10	1.81	.93	1.00

	ai	*yu·*	*yu·*	*u·*	*u·*	*a·*	*a·*	*ɔ·*
ι	.76	.59	.59	.96	.96	1.41		
i·							.83	.69
ei					−.02	.70	.70	.18
ä·	.24	.33	.33	.27				
ai	.00	.00	.00	.35				
yu·	.00	.00	.00	.05	.05	.28	.28	.32
u·	−.35	−.05	−.05	.00	.00	.08	.08	.22
a·			−.28	−.08	−.08	.00	.00	.40
ɔ·	−.43	−.32	−.32	−.22	−.22	−.40	−.40	.00
	.22	.55	.27	1.33	.69	2.07	1.49	1.81

Supplemented Calculation

	ι	*i·*	*ei*	*ai*	*ä·*	*yu·*	*u·*	*a·*	*ɔ·*
ι	.00	−.11	.62	.76	1.14	.59	.96	1.41	1.37*
i·	.11	.00	.77	.35	.63	.75*	.90*	.83	.69
ei	−.62	−.77	.00	.15*	.31	.21*	−.02	.70	.18
ai	−.76	−.35	−.15*	.00	−.24	.00	.35	.44*	.43
ä·	−1.14	−.63	−.31	.24	.00	.33	.27	.45*	.50*
yu·	−.59	−.75*	−.21*	.00	−.33	.00	.05	.28	.32
u·	−.96	−.90*	.02	−.35	−.27	−.05	.00	.08	.22
a·	−1.41	−.83	−.70	−.44*	−.45*	−.28	−.08	.00	.40
ɔ·	−1.37*	−.69	−.18	−.43	−.50*	−.32	−.22	−.40	.00
	−6.74	−5.03	−.14	.28	.29	1.23	2.21	3.79	4.11

* Supplemented sigma scores, calculated from the original scale

TABLE XI

Scale Values

Experiment 2A (Large vs. Small)

ι	.0000	*p*	.0000	*m*	.6354
i·	.2687	*n*	.0254	*dj*	.8023
ei	1.0370	*d*	.0555	*r*	.8362
ai	1.1029	*s*	.0800	*gl*	1.0210
ä·	1.1045	*h*	.1960	*gr*	1.2058
yu·	1.2521	*k*	.2243	*br*	1.2464
u·	1.4061	*b*	.3950		
a·	1.6544	*l*	.4497		
ɔ·	1.7047	*g*	.5901		

TABLE XII

SCALE VALUES

Experiment 2B (Dark vs. Bright)

i·	.0000	*k*	.0000	*r*	.6016
ι	.1918	*s*	.0000	*gl*	.7780
ai	.3520	*l*	.0151	*d*	.8590
ei	.5736	*h*	.3159	*m*	.9778
ä·	.8752	*p*	.3395	*gr*	1.0438
yu·	1.0606	*dj*	.4197	*br*	1.0816
a·	1.1534	*n*	.5413		
ɔ·	1.3247	*g*	.5545		
u·	1.6531	*b*	.5573		

TABLE XIII

AVERAGE DISCREPANCIES

Experiment 2A (Large vs. Small)

ι	.029	*p*	.079	*m*	.074
i·	.059	*n*	.055	*dj*	.073
ei	.061	*d*	.086	*r*	.062
ai	.032	*s*	.064	*gl*	.048
ä·	.041	*h*	.056	*gr*	.067
yu·	.037	*k*	.078	*br*	.049
u·	.046	*b*	.079		
a·	.040	*l*	.063		
ɔ·	.049	*g*	.075		
Av.	.044			Av.	.067

TABLE XIV

AVERAGE DISCREPANCIES

Experiment 2B (Dark vs. Bright)

i·	.021	*k*	.080	*r*	.089
ι	.032	*s*	.062	*gl*	.051
ai	.024	*l*	.062	*d*	.086
ei	.039	*h*	.046	*m*	.065
ä·	.020	*p*	.043	*gr*	.083
yu·	.065	*dj*	.108	*br*	.078
a·	.053	*n*	.067		
ɔ·	.056	*g*	.072		
u·	.034	*b*	.062		
Av.	.038			Av.	.070

Experiment 2A (large versus small) was taken by 141 *S*s, undergraduates at the University of Chicago. Five dropped out after completing Experiment 2A, leaving 136 in Experiment 2B (dark versus bright).

A casual examination of the experimental proportions was sufficient to show that the variations referred to above had a decided effect on the responses (Tables VIII and IX). It was necessary then to supplement the original sigma calculation with one which would make use of all the data, in order that each stimulus-position on the scale would be the result of the total conditions. An example of the two types of calculation is given in Table X. The final supplemented scale-values are to be found in Tables XI and XII and Fig. 2.

A comparison of the vowel-constellations of the two symbolic scales clearly shows two different patterns. The magnitude scale of the previous experiment appeared to be based upon three objective factors: articulatory position, acoustic frequency, and size of oral cavity. A point for point comparison of the symbolic scales of Experiment 1 and Experiment 2A is not possible, of course, for the vowels are different. But it will be noted that the same general plan is confirmed; the small-to-large symbolism follows the sequence of receding tongue position, decreasing characteristic high frequency, and increasing size of the oral cavity. The present experiment, however, reveals another factor entering into the judgment of size symbolism; *i.e.* vocalic length. The position of *i·* above ι on the present magnitude-scale runs counter to both the articulatory and frequency sequence. The conclusion can hardly be avoided that *i·* is symbolically larger than ι because the length of the former vowel makes it 'sound' larger. The dark-versus-bright symbolism, on the other hand, seems to be patterned exclusively on an articulatory and frequency basis; the factors of vocalic length and size-of-mouth-opening evidently have nothing to do with the symbolic judgment of relative darkness, a situation which is entirely plausible.

Turning to the consonants, we find similar objectively mechanical factors at work to form the subjective scales. In judgments of small to large the sequence of articulatory position is consistently dental, labial, palatal (*t, p, k; d, b; g; n, m*). This symbolic scale further follows the objective sequence of voiceless to voiced (*t, d; p, b; k, g; s, z; c, j;* etc.). The consonants conform to a series of phonetic sequences with a fairly definite mathematical regularity. The scale separations between some of the consonantal magnitude-values are listed.[6]

Position:

dental to labial		labial to palatal	
t-p	.3253	*p-k*	.2243
d-b	.3395	*b-g*	.1951

Voicing:

stops		fricatives		sibilants	
t-d	.3808	*θ-δ*	.4384	*tc-dj*	.5657
p-b	.3950	*f-v*	.4950	*c-j*	.6081
k-g	.3658				

---

[6] The reader should consult the consonantal magnitude-scale of Table XVII, which is more complete in having the consonantal responses of Group 3 (Experiment 1) grafted on the scale of Experiment 2A. The consonantal responses of only Group 3 were used because this group corresponded most closely in age and in number of subjects to the group of Experiment 2A.

The nasal consonants do not hold a consistent pattern in relation to the stop-consonants of their respective positions; but this apparent inconsistency is carried out in both of the symbolic scales. Although *m* (labial nasal) is both larger and darker than *b* (labial voiced stop), *n* (dental nasal) is both smaller and brighter than *d* (dental voiced stop).

The bright-to-dark symbolism is likewise objectively phonetic in its consonantal patterning. A voiceless-to-voiced sequence is adhered to, as in the small-to-large symbolism; apparently the voiced consonant, having more acoustic body than a voiceless sound, gives an impression of darkness as well as largeness. The articulatory sequence, on the basis of a bright-to-dark symbolism, in palatal, labial, dental—an order which is the reverse of that on the magnitude-scale. The palatal consonants, then, are bright but large; the dentals, on the other hand, are dark but small.

The symbolic darkness-scale is not complete enough for a presentation of mathematical relationships between consonants, as was furnished for the magnitude-scale. The pattern of symbolic darkness, as far as one can judge, does not show as thoroughgoing a mathematical regularity between the phonetic relations of consonants as that of symbolic magnitude. Yet when one compares the discrepancies of the consonants with those of the vowels (Tables XIII and XIV), the fact that the former indicate a less reliable index of symbolism makes it all the more surprising that they should be as consistently patterned as they are. Their symbolic ambiguity, in contrast to the vowels, reflects in a sense the embarrassment of acousticians confronted with the problem of making a physical analysis of consonants; if the symbolic judgments of phonetic sounds are based upon their objective properties, as these experiments seem to indicate, a subtlety and variability of objective composition will quite naturally be mirrored in a vagueness of symbolic reference.

The corrections for the variable of phonetic structure were computed from the formula

$$K = (Xe—Xc)\ (2)^{\frac{1}{2}}$$

K = the scale distance between the modal stimulus-value and the value of that stimulus under a particular condition of phonetic structure.

Xe = the experimental sigma-value corresponding to the experimental proportion of a stimulus-pair.

Xc = the sigma-value of the calculated proportion.

$2^{\frac{1}{2}}$ = a constant (the discriminal errors of the stimuli) used in the scale formula (*supra* p. 58).

An example of the procedure is given:

Stimulus Contrast	Xe	Xc	Xe−Xc
b-p	.6219	.2793	.3426
g-d	.3989	.3780	.0209*
gl-k	.5740	.5634	.0106
gr-m	.2045	.4034	−.1989
r-n	.5978	.5733	.0245
l-s	.3292	.2614	.0678
dj-h	.1332	.4287	−.2955

All of the consonant pairs in this illustration were given initially, accented, in three-syllable words (*báguli-páguli, dɔmali-gɔmali, glápuni-kápuni,* etc.). The median difference between the experimental and the calculated sigma-scores was taken as the normal value for this condition of phonetic structure. This value is starred in the above example. Using formula K = (Xe—Xc) $(2)^{1/2}$, we may conclude: Init., acc., 3-syl. word = 0.0209 $(2)^{1/2}$ = 0.0296. This procedure was applied to all pairs presented under the same conditions of phonetic structure. The calculated proportions employed in computing the discrepancies were in each case corrected by the structural condition under which the particular stimulus was given (Discrepancy = Pe—Pc, in which Pc is the proportion corresponding to Xc corrected by the sigma-value of K).

In spite of other indeterminate fluctuations disturbing the character

TABLE XV

Individual Corrections for Phonetic Structure

When the syllabic structure of a condition is not expressed in the table, it is understood to be a two-syllable word.

Vowels

	Exper. 2A	Exper. 2B
K 1. Unacc., first syl.	−.4111	−.1293
K 2. “ final	−.3379	−.1817
K 3. First syl., acc., final cons.	−.0202	−.0413
K 4. “ “ “ cons. doublet	.0006	−.1127
K 5. “ “ “	.0823	−.0443
K 6. One syl. word	.1266	.0092
K 7. Final, acc.	.1848	.2800

Consonants

	Exper. 2A	Exper. 2B
K 1. Final, acc.	−.2437	−.2876
K 2. “ unacc.	−.2353	−.4342
K 3. Initial, acc., final cons.	−.2333	−.0686
K 4. “ “ long final vowel	−.0535	−.0209
K 5. “ unacc.	−.0423	−.1622
K 6. One syl. word, initial	−.0412	.0083
K 7. “ “ “ final	−.0235	−.1273
K 8. Initial, acc., 3 syl. word	.0296	−.1397
K 9. “ “ cons. doublet in syl.	.0808	−.0870
K10. Medial, accent following	.0940	−.0908
K11. “ “ preceding	.1143	−.0358
K12. Initial, acc., long vowel following	.1584	.1863
K13. “ “ diphthong following	.2728	−.1988

of this variable of phonetic structure, the corrections tend to fall into a typological pattern (Tables XV and XVI). The vowel corrections follow essentially the same rank-order in both the magnitude- and the darkness-scales, but the consonants do not. The symbolic value of accenting the vowel is heavily in favor of largeness and darkness; the final position of the vowel likewise carries with it a large and dark symbolism. In the consonants, on the other hand, the final position is the weakest for symbolic magnitude and darkness. The structural pattern for the consonants of magnitude-symbolism shows three types of initial position. Psychologically considered, these might be termed: (1) *initial unemphatic,* when the syllable containing the consonant is unaccented, or when the word ends in a consonant or a long vowel—conditions which might be interpreted as emphasizing some other part of the word than the initial syllable; (2) *initial neutral,* when the syllable is closed by a consonantal doublet, or when it is part of a three-syllable word; (3) *initial emphatic,* when the syllable containing the consonant is emphasized by having a long vowel or a diphthong. The medial position is symbolically larger than the initial neutral, but smaller than the initial emphatic.

TABLE XVI

TYPOLOGICAL CORRECTIONS FOR PHONETIC STRUCTURE

Experiment 2A (Large vs. Small)

The scale-value of a typological correction is the mean of its individual corrections (Table XV). The individual conditions making up a type are given in parentheses.

Vowels	
KI (K1, K2) Unaccented	−.3653
KII (K3, K4, K5) First syl., acc.	.0256
KIII (K6) One syl. word	.1266
KIV (K7) Final, acc.	.1848
Consonants	
KI (K1, K2) Final	−.2403
KII (K3, K4, K5) Initial unemphatic	−.1011
KIII (K6, K7) One syl. word	−.0305
KIV (K8, K9) Initial neutral	.0499
KV (K10, K11) Medial	.1047
KVI (K12, K13) Initial emphatic	.2155

Phonetic symbolism and English words. The data amassed for the magnitude-values of vowels, consonants, and phonetic structure were employed to determine whether meaningful English words reflected this type of phonetic symbolism. For this purpose a group of words were gathered from Roget's *Thesaurus;* all the words under the rubrics, Greatness, Smallness, Size, and Littleness were listed (Nos. 31, 32, 192, 193), and the repetitions, derivatives, and phrases were struck out. The resulting list, containing almost five hundred words, included many terms which did not seem to belong clearly to the categories of denotative largeness and smallness. The list was then divided into the two denotative classes and given to 11 judges, who were asked to check the words that, in their opinion, did not conform in meaning to their classifications. The final list, taken from a majority vote of the judges, is printed in Tables XVIII and XIX.

TABLE XVII

Magnitude-Scale Values under Conditions of Phonetic Structure

In accord with the procedure of arranging scale-sequence throughout this paper, the stimulus with the lowest scale-value is taken as the origin. The consonant-responses of Group 3, Exper. 1, have been grafted on the consonantal magnitude-scale of Exper. 2A.

Vowels

	KI	KII	KIII	KIV
ι	.0000	.3909	.4919	.5501
i·	.2687	.6596	.7606	.8188
ei	1.0370	1.4279	1.5289	1.5871
ai	1.1029	1.4938	1.5948	1.6530
ä·	1.1045	1.4954	1.5964	1.6546
yu·	1.2521	1.6430	1.7440	1.8022
u·	1.4061	1.7970	1.8980	1.9562
a·	1.6544	2.0453	2.1463	2.2045
ɔ·	1.7047	2.0956	2.1966	2.2548

Consonants

	KI	KII	KIII	KIV	KV	KVI
t	.0000	.1392	.2098	.2902	.3450	.4558
c	.3215	.4607	.5313	.6117	.6665	.7773
p	.3253	.4645	.5351	.6155	.6703	.7811
n	.3507	.4899	.5605	.6409	.6957	.8065
d	.3808	.5200	.5906	.6710	.7258	.8366
f	.3819	.5211	.5917	.6721	.7269	.8377
s	.4053	.5445	.6151	.6955	.7503	.8611
h	.5213	.6605	.7311	.8115	.8663	.9771
k	.5496	.6888	.7594	.8398	.8946	1.0054
tc	.5619	.7011	.7717	.8521	.9069	1.0177
θ	.6222	.7614	.8320	.9124	.9672	1.0780
b	.7203	.8595	.9301	1.0105	1.0653	1.1761
l	.7750	.9142	.9848	1.0652	1.1200	1.2308
v	.8769	1.0161	1.0867	1.1671	1.2219	1.3327
g	.9154	1.0546	1.1252	1.2056	1.2604	1.3712
j	.9296	1.0688	1.1394	1.2198	1.2746	1.3854
m	.9607	1.0999	1.1705	1.2509	1.3057	1.4165
δ	1.0606	1.1998	1.2704	1.3508	1.4056	1.5164
dj	1.1276	1.2668	1.3374	1.4178	1.4726	1.5834
r	1.1615	1.3007	1.3713	1.4517	1.5065	1.6173
z	1.2255	1.3647	1.4353	1.5157	1.5705	1.6813

A frequency table was made for the distribution of phonetic sounds in each of these meaningful categories; the values used were those of Table XVII, which includes the symbolic magnitude-values for vowels and for consonants under the various conditions of phonetic structure.[7] This gave two independent approaches to the determination of symbolic magnitude in English words denoting largeness and smallness. It was necessary to construct a third frequency-table for the distribution of short vowels; the values used here were those of Experiment 1; but the variable of phonetic structure was not introduced, this variable having been computed from a

TABLE XVIII

WORDS DENOTING SMALLNESS

abate
abridge
animalcule
atom
attenuate

barely
bit
bubble
button

chip
chit
close
contract
corpuscle
crumb
crumble

decimate
decline
decrease
depreciate
descend
detail
deteriorate
diminish
diminutive
discount
dole
doll
dot
driblet
drop
dumpy
dwarf

ebb
elf
eliminate
embryo
epitome
evanescent

faint
few
flitter
fly
fraction
fragment

gleam
globule
gnat
grain
grub

hardly

inch
infinitesimal
iota
insect

jot
just

languish
least
less
light
Liliputian
limited
little
look
low

maggot
mannikin
meagre
mean
mediocre
melt
mere
microbe
microcosm
midge
miniature
minikin
minim
minimum
minnow
minor
minute
mite
mitigate
modest
modicum
molecule
monad
morsel
mote
mouse

narrow
near
nutshell

only

paltry
paring
part
passable
patch
paucity
pebble
petty
pigmy
pigwidgeon
pin
pocket
point
portable
powder
puny
puppet

rag
rare
rather
reduce
rudiment
runt

scant
scarce
scrag
scrap
scrubby
scruple
seed
seldom
shade
shadow
shaving
sheer
shiver
short
shred
shrimp
shrunk
simple
sip
slender
slight
slip
sliver
smack
small
snack
snatch
snip
sop
spare
spark
speck
spice
splinter
sprinkling
squat
stunted
subside
subtract
sup

tag
tatter
tenuous
tender
thin
tincture
tinge
tiny
tit
tittle
tomtit
touch
trifle

urchin

vanish

wane
waste
weak
wear
weazen
wee
whit
worm
wretched

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[7] The simplified set of typological conditions (Table XVI) was used in order to avoid unnecessary detail. The consonant doublets, *gl*, *gr*, and *br*, were not included in the table of frequency-values because their position on the magnitude-scale indicated that they did not behave as consonantal units.

TABLE XIX

WORDS DENOTING LARGENESS

absolute
abundant
amazing
ample
arch
assemblage
astonishing
awful

behemoth
best
big
brawny
bulk
burly
bushel

capacity
cargo
chief
chubby
colossus
comprehensive
considerable
consummate
corporation
corpulence
crass
Cyclops

deep
dimension
dreadful

egregious
elephant
emphatic
enormous

entire
exceed
excessive
exorbitant
expand
expanse
extent
extraordinary
extravagant
extreme

fabulous
famously
fat
flagrant
fleshy
flood
frightful
full
furious

Gargantua
giant
gigantic
glaring
goodly
grave
great
gross

heap
heavy
high
hippopotamus
horrible
huge
hulk

immense
important
increase
incredible
indeed
intense

lamentable
large
leviathan
load
lump
lusty

magnificent
magnitude
main
major
mammoth
many
marked
marvellous
mass
might
monster
mound
mountain
much
multitude

noble
notable
noteworthy
number

obese
outrageous
overgrown

perfect
plenary
plump
porpoise
portly
positive
pot
power
precious
preeminent
preposterous
principal
prodigious
profound
prominent
proportions
puffy

quantity

rank
remarkable
rich
roaring

several
shocking
size
soar
sound
space
stalwart
stark
stock
store
stout
strength
striking

strong
stupendous
sufficiency
supreme
superlative
surprising
swollen

terrible
thumping
thundering
tower
transcend
tremendous
truth

ultra
utter

vast
veriest
very
violent
volume

well
whacking
whale
whole
wholesale
whopper
wide
wonder
world

different experiment. The three indexes (Sf/f, in which S = the magnitude-value of a phonetic sound on the symbolic scale, and f = the frequency of a phonetic sound in English words) give the average symbolic magnitude of (1) long vowels and diphthongs, (2) consonants, and (3) short vowels. The values of the three indexes are as follows:

	Words Denoting Largeness	Words Denoting Smallness
(1) Long vowels and diphthongs	0.9832	0.9162
(2) Consonants	0.8476	0.8024
(3) Short vowels	1.3711	1.4072

From this table it must be admitted that the phonetic content of English words takes practically no account of magnitude-symbolism. The large/

small ratio of the three indexes is respectively 1.08, 1.06, and .97. It may be argued that English words do show, perhaps, a very slight regard for symbolic value, since the first two indexes are more reliable in taking account of the variable of phonetic structure; but this tendency is so minute as to be negligible. The actual distribution of sounds in the two denotative categories is fairly random, as far as phonetic symbolism is concerned.

These investigations lead us to several conclusions, which may be summed up as follows:

(1) Phonetic elements tend to be rigidly patterned on a non-linguistic symbolic scale. This rigidity of subjective patterning is evinced by the fairly consistent agreement between the three groups of Experiment 1 in respect to the sequence of phonetic stimuli on a symbolic scale as well as to the discriminal distances between them. The suggestions given by the apparent regularity of the discrepancies and by the consistent relations of objectively phonetic factors point to the same conclusion.

(2) The factor of age, as far as this investigation has gone, has little effect on the subjective scale, the slight variations in patterning being mere differences in the degree of consistency and of discriminal clarity. The child of nine years shows the same plan in his symbolic judgments as the adult.

(3) The basis of phonetic symbolism is fundamentally objective. The symbolic scale is constellated in accordance with such mechanical factors as position of articulation, acoustic resonance-frequency, size of oral cavity, vocalic length, consonantal voicing, and phonetic structure.

(4) Diverse types of phonetic patterns are formed by unlike symbolisms. This divergence of patterning is occasioned by a variety of objective factors entering into the symbolic judgments. The sequences of the tongue-position in articulation and of consonantal voicing appear to be basic factors in the patterns of magnitude- and darkness-symbolisms; but such factors as vocalic length and the size of the oral cavity are relevant only for judgments of magnitude.

(5) These symbolic judgments are not produced by linguistic associations. The investigation of the denotative categories of largeness and smallness in English showed practically no correlation between the meaningful and the symbolic distribution of speech sounds. The rigidity of the subjective scale would in itself preclude any such chance factor as linguistic association. The underlying objective rationale of the symbolic pattern would further tend to deny such an assumption, for it could hardly be maintained that the distribution of phonetic sounds in meaningful English words follows a mechanical and objective schema.

AN ACCESSORY STUDY OF "PHONETIC SYMBOLISM"

By MADISON BENTLEY and EDITH J. VARON, Cornell University

In 1929 Sapir published a brief study from which he concluded that certain vocalic and consonantal sounds have a definite symbolic significance unrelated to the associative and linguistic value commonly attached to words.[1] His method consisted in presenting two 'unfamiliar' words (*e.g. mal* and *mil*), to which a meaning (*e.g.* 'table') was arbitrarily attached. His subjects reported whether *mal* symbolized a larger or a smaller table when compared with the word *mil*. The results indicated that there was a tendency for vowels near the *a*-end of the vocalic scale to imply 'largeness' and for those near the *i*-end to imply 'smallness,' quite apart from linguistic uses of these sounds. A like reference was also discovered for certain consonants.

In the immediately preceding article in this JOURNAL S. S. Newman has, at the instance of Professor Sapir, again attacked the problem of phonetic symbolism, applying to his research a method of statistical treatment not used in the earlier study, and using a much larger body of material than the earlier study commanded.[2] Newman's results generally confirm Sapir's earlier conclusions, making the interpretation much more specific. Newman again makes phonetic symbolism apply differentially to 'large' and 'small' and adds also the contrasting pair 'bright' and 'dark.' The distribution of his vowel-sounds and consonant-sounds upon the scale of magnitude (large-small) and the scale of brilliance (dark-light) leads him to believe that it is determined by such mechanical factors as position of the tongue in articulation, resonance within the mouth, the size of the oral cavity, and length of the vowel sound. Some differences in these factors with respect to their influence upon the two scales are thought to be significant.

Upon the appearance of the first article, we were led to undertake a bit of experimentation upon the subject, partly to adapt the method to the psychological laboratory and partly to discover how far "phonetic

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[1] E. Sapir, A study in phonetic symbolism, *J. Exper. Psychol.*, 12, 1929, 225-239.

[2] S. S. Newman, Further experiments in phonetic symbolism, this JOURNAL, 45, 1933, 53-75.

symbolism" was actually inherent in the sounds used and how far it arbitrarily laid hold of variety in the make-up of the non-linguistic 'words' and turned them perforce into degrees of 'magnitude.' We used a small number of students trained in research, and we devised various means for extracting size or magnitude, without hinting at it, from the presented sounds. Furthermore, we wished to discover—in the event of 'a more or less,' 'a this or that'—whether such an intimation of degree or of contrasting quality pertained specifically to spatial determinations. In certain series, therefore, we added to *size* the nine categories of *angularity, foolishness, endurance, liquidity, sentimental attachment, motion, noisiness, solidity,* and *strength.*

Method I. (One category word: 'large.') Forty series of 10 nonsense syllables each (vowel between two variable consonants) were made up and presented to each of three *O*s (*D, G,* and *L*).

The *O*s were graduate students trained in the procedures of the experimental laboratory. They knew therefore how to observe under prescribed instructions and how to make relevant and precise reports. Each series contained either two or three familiar words and seven or eight non-linguistic (n-l) sounds. Of the latter, two were *critical*; *i.e.* they contained vowels taken from near the ends of Sapir's vocalic scales. One vowel was *a* (Ger. M*a*nn), the other *i* (Fr. f*i*ni). The completing consonants were varied. The critical sounds always appeared between 'nonsense' sounds. In the first 20 series they were separated from each other; in the last 20 series they directly followed each other, as a possible enhancement of the 'contrasting' vowels. *O* was screened from *E,* that mouth-setting, posture, or gesture might not be effective.

O was instructed to respond promptly with the first sound that came after hearing a given sound pronounced by *E.*[3] Since rhymes and assonances were at first frequently given in response, *O* was presently instructed either to give a synonym or otherwise to express the meaning of the heard sound, but to avoid rhymes and similar-sounding words. The forty series were three times given (in changed order) to each *O* (120 series × 10 sounds each × 3 *O*s = 3600 sounds altogether). Sometimes we found the sound completed to form a word (*saf*—Sappho); sometimes an onomatopoeic meaning came (*zin*—noise of a flying missile; *vub*—a frog sound; *zuv*—a soft, gentle something), and sometimes a near-lying word came (*kir*—care).

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[3] Although one experimenter (Varon) uttered all the sounds used in this study, the precise and consistent character of the non-linguistic sounds proved to be difficult. As Dr. Sapir has urged in correspondence, the common use by him and us of identical phonetic symbols offers no guarantee that the same sounds were actually employed. We suggested master-records on phonographic disks; but the matter could not be arranged. There remains the possibility, then, that certain differences between his results (together with Newman's) and ours may be due in part to unlikenesses in the actual sounds uttered. Of course the Sapir experiments have the advantage of expert knowledge of phonetic quality and quantity. At the same time, we must see that, however unfaithful our sounds may have been to the high phonetic standards set, we have without doubt attained such differences in the "mechanical factors" assumed by Newman (*supra* p. 61) to be operative as should make our results as well as his significant for the problem of "phonetic symbolism."

No responses definitely indicated, however, that such a factor as phonetic symbolization was present, at least in a degree that led to a response-sound that suggested size or any determination of magnitude. Of course this negative outcome does not imply that magnitudes and differences of magnitude might not be forced from the sounds under other conditions. 'Feeling significances' (Sapir) might be potentially at hand but too weak to compete with 'reproductive tendencies' (whatever these theoretical conditions of vocalization may be!) under our conditions of presentation. The results do suggest, however, that such sounds as we have employed do not *spontaneously* lead to other sounds (under the common conditions of 'free association') which denote or hint at spatial magnitude or its qualifications.

Method II. (10 category-words, each followed by an n-l sound.) Here we extended our search to the 10 categories named above. The method approached one step nearer that of Sapir (first article), but still preserved notable differences.

The general procedure was to sound one of the 10 category-words (angular, foolish, large, lasting, liquid, loving, moving, noisy, solid or strong) and then to sound a non-linguistic (n-l) sound with the instruction to report whether this sound implied some *degree* of the first or of its opposite (*e.g.* large or small). If it did, the report was to be 'slightly,' 'moderately,' or 'very;' if it did not, the report was to be 'unrelated.' Thus the second sound in each pair was non-linguistic (n-l). Choice was made here from a list of 50 monosyllables. For constructing these the vowel *e* (Eng. met) was added to the *a* and *i* used under Method I. Sometimes the initial consonantal sound was kept constant (*e.g.* gam, gem, gim), thus providing an opportunity to compare results with a single context for unlike vowels. Each series of ten pairs (*e.g.* for a pair, angular-kig) contained 4 *a*-sounds, 4 *i*-sounds, and 2 *e*-sounds. The category-words and the n-l sounds were combined in a haphazard order. As each category-word was given with each n-l sound, 500 combinations were presented to each *O* (10 words × 50 following-n-l-sounds × 3 *O*s = 1500 instances in all). The *O*s were *D, G,* and *L* as before.

Of the 1500 reports under this method 450 indicated some kind of relation (called *positive* report) between the category word and the n-l sound following it in each pair.

Agreements and disagreements among the *positive* reports stand as follows for the 450 instances;

Agreement			*Disagreement* (in direction)	Total
Reported by 1 *O*	2 *O*s	3 *O*s		
202	174	18	56	450

Thus well above two-fifths (202) of the cases of positive relation were reports made by a single *O*. Two *O*s agreed upon approximately four-ninths (192) of the cases; all three *O*s agreed only 18 times. Disagreement as to amount or direction of the relation was shown in 56 cases. These disagreements are illustrated by *lel,* reported as 'moderately large' (*D*), and as 'moderately small' (*G*) ; *wim* reported as 'slightly weak' (*G*), and as 'equally weak and strong' (*D*).

A little less than one-third of the whole number of reports (450:1500) indicate, then, that, to the n-l sounds used, the quality expressed in the category-words was applied by one or more of our *O*s, under the conditions imposed by our experiments. Upon most (960) of the 1050 'unrelated' or *negative* reports, two of the three *Os* agree. This high agreement upon the *want of relation* may be as significant as the *positive* reports. It seems to imply that a good many of the total n-l sounds used (*i.e.* the total 'nonsense syllable') symbolically represent *no* qualification of the categories used.

As regards our positive reports, an analysis of the results shows that a large number of our 50 n-l sounds led to some degree of relationship where each one of our 10 categories was used. With respect to the degree of relationship (slightly, moderate, very), few (only 8 altogether) 'very' reports were made. Nearly all the reports alleged a 'slight' relationship. Of all 10 categories, the fewest positive reports were given with the meaning *solid.* Some notable differences with respect to the vowel of the n-l sound were observable. Thus the positive 'angular' reports were chiefly returned with *a*- and *e*-sounds, seldom with an *i*-sound. The 'solid' reports seldom came with an *e*-sound or the 'strong' reports with *a* and *e*. The total number of positive reports is distributed as follows among the 10 categories; angular 35, foolish 48, large 32, lasting 42, liquid 41, loving 41, moving 34, noisy 32, solid 23, and strong 35. It is possible, therefore, to report a relation (of more or less) over a wide range of diverse qualities.

Method III. (5 categories and their opposites,with n-l sounds in pairs.) As a still closer approach to the original method of Sapir, we next followed our category-word with *two* n-l sounds. The *O* was instructed to report which (if either) of the two n-l sounds more closely approximated the meaning of the category-word just given.

Five categories were used; *foolish, angular, liquid, large,* and *strong.* First, one of these category-words was pronounced (*e.g.* 'foolish'), then (at a suitable interval) two n-l sounds (*e.g.* 'zat,' 'fim') in quick succession and with an even inflection and intonation. The sounds *o* (go) and *u* (tufa) were added in this series. We considered that, although our results would be few and not admissible to statistical treatment, they would be obtained with trained and practiced *Os* (the 3 *Os* of Methods I and II), working under evener and more constant conditions than could well be commanded in a group, and that they might give hints of selectiveness in the phonetic scales as Sapir and Newman have observed with fairly large groups of *Ss*. Twenty combinations with each category-word were made, in both temporal orders for the n-l sounds.

In the 300 pairings (5 x 20 x 3 *Os*), some degree of relationship was reported 165 times and for all 5 categories. In only 7 cases was the degree reported as 'very,' and as 'moderate' only 17 times. All the others are 'slight.' Comments by all *Os* bear out the suspicion that the relation was usually forced and that, when it was reportable, the difference in 'size,' 'liquidity,' etc., as between 'zat' and 'riv,' 'lel' and 'duf,' etc., was both doubtful and obscure. The case is different with Newman, who asked his *Ss* whether this or that n-l sound was the larger or the smaller 'horse,' and so on. In his case the two sounds were arbitrarily given the meaning 'horse' and the larger (or smaller) arbitrary 'horse-sound' was then checked. Our *Os* were left free to discover that either or neither of the n-l sounds applied to size, strength, and the other category-words. In both studies it is obvious that the formal and the occasional instructions are weighted—albeit by different devices—to force a meaning into an n-l sound and then to suggest that this meaning admitted of degree. Most of the Sapir-Newman results bear upon the "magnitude-pattern" forced upon names, activities and adjectival qualities. Newman added (Experiment II) a "darkness pattern." Our employment of a larger number of "patterns" (category-words) makes it again obvious that *Os* may be forced to report differentially upon degree in many other contexts as well. It is strikingly attested also by the fact that our *Os* reported 21 *opposites* for 'large,' 'liquid,' and 'strong;' *i.e.* certain non-linguistics (n-l) were reported as 'small' (6), as 'solid' (7), and as 'weak' (8), although these words had not been prescribed. Of these opposites, 14 were *e*- and *i*-sounds, and 6 were *a*-, *o*-, and *u*-sounds. But 'liquidity' and 'strength' favored the *i*-sounds quite as much as size did.

Method IV. (Group experiment: 3 categories and their opposites; partially forced meanings.) We now used the 'arbitrary meaning' device of Sapir. Twenty-six members of a class doing drill-experiments in psychology were used. The general setting was experimental, for a part of the laboratory-period was devoted to our observations. In order to enhance the experimental setting, the instruction as given by Sapir was somewhat modified.

Sapir found that his exercise with high school students "seemed to be enjoyed by the great majority of his subjects" (high-school students) "as a rather interesting game."[4] Although his conditions and instructions were not quite clear, he seems to have prearranged that two sounds, about to be uttered, were to be given an arbitrary meaning (*e.g. mal* and *mil* were to be given the meaning 'table'). Then when these sounds were given, the *S* checked in a column marked "large" if the first of the sounds "symbolized the larger reference," and in a column marked "small" if the first sound symbolized the smaller reference. Whether 'copying' was controlled among the 500 *S*s does not appear. To eliminate errors of time-order; in our experiments, and to make sure that the cumulative results of *O*'s own checking should not influence each succeeding check-mark, the written result was immediately removed from his view as soon as it was finished. In column-checking a common form of error from occasional instruction is to even the number of entries in both (or all) the columns in the serial run. Whether the children observed Sapir's face when he uttered the sounds, does not appear. The open-mouth continental *a* might be quite enough to swing the *a*-words to the 'large' column. That is one reason for our invariable use of a screen. Another possible source of degree-difference in n-l sounds emanates from the hearer. Sapir dwells upon this possibility, factoring it into auditory and kinaesthetic components. Unfortunately, this source was not sufficiently controlled, either in his and Newman's investigations nor in ours. How much all our *O*s used their vocalizing apparatus—either for incipient sounds or for 'gesturing'—no one knows. Five minutes' attention to the strains and postures incident to making brief vocal sounds will convince one that there is here plenty of material which, given a linguistic turn, will easily absorb 'degrees,' not only of magnitude but also of many other qualifications. We used 36 n-l paired combinations. First, the four sounds *fam, jaf, maf,* and *vag* were paired with *fim, jif, miv,* and *vig.* Thus 'contrasting' vowels were systematically distributed and combined. In the second place, we added sounds to pair off all the vowels among themselves (as in Method III). Finally, consonantal variations were used with each vowel (*e.g.* bav-pav, bav-pag, pav-pag, etc.) to discover the specific effect of the consonants. Haphazard order was followed throughout. There were 4 series of 9 sound-pairs each. Three categories (*angular, large, hard*) and their common opposites (*round, small, soft*) were used with all 36 n-l pairs. The order of utterance in each case was: (1) a pair of n-l; (2) object-, action-, or quality-word; (3) category-word. An instance is (1) fam-fim; (2) cabin; (3) which is the larger cabin? The vocal presentation had been preceded by the general instruction: "I shall utter two sounds (*e.g.* fam-fim), then an arbitrary meaning which you are to apply to them (*e.g.* cabin), then ask you to compare the two sounds, if you can, with respect to some attribute (*e.g.* size)." Here we departed from Sapir's procedure in leaving the possibility that the two sounds might not be comparable with respect to the attribute. We wished to force the comparison as little as possible.

Our tables show, as the Chicago studies had shown, a persistent inequality in the 'largeness' of the various vowel-sounds. Here are the results from a paired comparison of four *a*-sounds and four *i*-sounds with the arbitrary meaning 'table.'

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[4] *Op. cit.*, 229.

Object = Table (large and small)

	A-sounds (fam, jaf, mav, vag)	I-sounds (fim, gif, miv, vig)	totals
'larger'	176	46	222
'smaller'	41	189	230
totals	217	235	452

The *a*-sounds are *larger* than the *i*-sounds (*table*) in the approximate ratio of 4:1. Reading down each column, the large-small ratio for *a*-sounds (table) is also approximately 4:1. The reverse relation holds also for the *i*-sounds. While calculated reliabilities would have little significance for our small number of observations and of observers, the internal consistency is striking.

It is worth nothing that, in size-of-table, the *a*-sounds are conspicuously *more* in number of 'large' reports than the *i*-sounds, and conspicuously *less* in number of 'small' reports *in every one of the sound-pairs compared.* With respect to size, then, *a* (as in Ger. M*a*nn) is consistently *larger* (and less small) than *i* (as in Fr. f*i*ni).

We may add similar results with the same paired sounds when the arbitrary object is 'table' (angular and round) and is 'chair' (hard and soft).

Object = Table (angular and round)

	A-sounds (same as above)	I-sounds (same as above)	totals
'more angular'	65	193	258
'rounder'	199	55	254
totals	264	248	512

Object = Chair (hard and soft)

	A-sounds (same as above)	I-sounds (same as above)	totals
'harder'	88	164	252
'softer'	162	88	250
totals	250	252	502

As regards angularity we find again a decided difference as between the two n-l sounds. For both sounds, the ratios for angularity and round stand 1:3 and 3:1. Again the totals for the two sounds are about the same (264, 248.)

With respect to hardness, *a* is softer (and less hard) than *i*. An unlikeness in clarity of difference—with respect to the three qualities—appears in the three approximate ratios, 4:1 (size), 3:1 (roundness), and 2:1 (softness). That the *e* used (as in Eng. m*e*t) stands, in the respects noted, near our *i* is suggested by the fact that *fam* is reported larger than *fem* (table) and softer (and less hard) than *fem* (chair) by fairly large ratios (cf. Newman's quantitative results).

It appears at once that the *positive* reports form only a moderate fraction of the whole number. We have omitted all unrelated reports, as well as reports of *same* and *doubtful.*

Although Sapir and Newman are not clear upon the point, they seem to say that their method of forced meaning led only and always to the report of *unlike degree.* Our method left *O* free to note or to fail to note the applicability of the category to the comparison in question. The forcing instruction, in such cases, is bound to increase the number of *positive* reports, but it may well affect validity also. One wonders whether Sapir's *S*s may not, in 'playing their game,' have soon learned under forcing to apply a half-formulated rule with respect to sounds and magnitudes. Every one familiar with the conduct of psychophysical methods knows how readily even highly trained *O*s pick up devices which relieve them of making *bona fide* comparisons.

Of course the discovery that arbitrary meanings and forced choices may lead invariably to a comparative report of, say, magnitude does not prove that the 'magnitude' dimension came out of the n-l sound in the form of a 'phonetic symbol.' It only shows that, under the conditions, the *S*s apply, with more or less consistency, *some sort of difference* in the two n-l sounds to the problem proposed by the experimenter. In our own experiments we have found that the qualifications suggested by 'arbitrary meanings' come out in proportion to the amount of forcing. 'Free association' brought only a few conventional responses, either through a linguistic similar (mel-smell), a completion (saf-sappho), or an obvious onomatopoeia (bah-baa!). No real 'phonetic symbolism' in Sapir's sense appeared. The positive report did appear when *degree in some scale* was suggested and prescribed.

Sapir's fascinating problem lies on the border-line between linguistic material and non-symbolic sounds. It is natural that a student of languages and of phonetics should incline the problem toward his own field. The articles constantly refer to 'words' even where non-linguistic sounds (n-l, in our own shorthand) were used. It may be instructive, therefore, to drop for the moment this context and to regard the sounds as auditory or other objects of perception. This would be the psychologist's natural inclination. Then it would appear that the simplest and best-ordered series of sounds (the tonal series) is a one-dimensional series, proceeding from low, grave, voluminous, dull, and weak members, at the one end, to high, acute, small, bright, and strong members, at the other. After decades of conjecture and experiment upon the possible 'associative' character of certain of these qualifications, it is generally agreed among psychologists that some such set of aspects is actually attributive and therefore inherent in the tones and that they gradually change from one end of the series to the other. As everyone knows, the effectiveness and significance of speech, music, and natural sounds constantly exemplify—and often voluntarily use—these attributive qualifications of the simpler sounds. Intensity is another instructive attribute. It is capable of fine gradation and may directly (*i.e.* without symbolization) be apprehended as strength. Once more, there is in all these attributive qualifications a principle of surrogative function by way of which any one may stand for any other and may be interchangeably used. Thus a decided intensive difference will readily adopt the context of size or of gravity and so stand for a 'contrast' in volume or in pitch. In fact, it requires long and rigorous training to distinguish modifications in pitch, volume, dullness and intensity.[5]

When we turn to the simpler noises, we find virtually the same set of attributes; though there is probably no actual identity, quality for quality, in the two series. If then we proceed to those complexer sounds which form the basis of linguistic communication, we find again the same sort of distinguishing characteristics. But here the case is considerably complicated by hundreds of diverse fusions of individual sounds. Nevertheless, it is a commonplace to the students of language and of music that the simpler vocal sounds partake of the general nature and constitution of their fused and temporally colligated members (compare tone and note, note and vowel, vowel and larger phonetic unit).

As it happens, Sapir lighted upon a qualification for 'phonetic symbolism' which is exceedingly prominent in the simple tonal system; namely, voluminousness (magnitude), and Newman has added a second, namely, brightness (dull and bright). Possibly no other choice would have turned out to be so significant. The positions in the tonal scale from which the main ingredients of the vocalic sounds are drawn well illustrate obvious spacings with regard both to 'magnitude' and to 'brightness.'

Now these vocalic characteristics, and others like them in consonantal elements, pertain to the sounds *as sounds*. They attain a 'phonetic' or 'symbolic' signification only when we transfer them to other contexts. It appears that they have not generally been useful on the meaning side of language, albeit they possess high aesthetic value both in speech and in music.

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[5] Cf. R. H. Gundlach and M. Bentley, The dependence of tonal attributes upon phase, this JOURNAL, 42, 1930, 519-543.

It does appear from these studies that certain tonal, vocalic, and consonantal characteristics, which likewise bear a fixed relation to physical sources of sound and also to the human productive mechanisms (position of tongue, resonance, size of oral cavity, etc.), as Newman and Sapir have pointed out, are so well graduated as to be usable when forced by artifice into the representation of size and of various other concepts and categories as well. Nevertheless, there seems to be insufficient evidence that these graded attributes of sound carry in their own right (so to say) a symbolic reference. Newman discovered,[6] as we might expect, that linguistic usage derives its verbal and syntactical meanings (true symbolization) from other sources. The case is probably the same for colors, where also we have heard a good deal of 'symbol' and 'fixed emotive significance.' Even gustatory and tactual qualities are useful indicators of many things, but chiefly by an adventitious overlay of reference upon qualitative specificity.

As a demonstration of the use of attributive characteristics of simple vocal and instrumental sounds in discriminations of the sort exemplified by Sapir and Newman, we presented to an introductory class in psychology (327 university students) 5 pairs of n-l sounds, three of them vocal (*fam-jif*, *vig-vam* and *miv-vag*) and two instrumental, the first a succession of simple tones from tuning forks (512 vs. and 2304 vs.) and the other pair two fairly simple noises of the explosive sort (the first produced by the thud of a padded mallet on heavy wood, the other the crack of a small pith-hammer upon a wooden surface).

The following instruction preceded the giving of the sound-pairs. "I shall utter two sounds in succession from behind this screen, first warning you with a 'ready.' As you hear the two sounds, consider which of them appears (just as a sound) *larger* (*lighter, softer, smaller*) than the other." The first vocal pair was compared for largeness, the second for lightness (as contrasted with darkness), and the third for softness. The two tuning-fork tones were compared for largeness and the two noises for smallness. The results follow.

Reported	First	Second	Neither	Totals
fam-jif ('larger')	165	83	79	327
vig-vam ('lighter')	240	62	25	327
miv-vag ('softer')	184	72	71	327
tones ('larger')	265	38	24	327
noises ('smaller')	15	304	8	327
	869	559	207	1635

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[6] *Supra* pp. 71-75.

It appears, therefore, that untrained observers find it possible to distinguish these characteristics in n-l vocal and instrumental sounds without any such linguistic reference as Sapir and Newman used. We have no reason to believe that there was involved in these comparisons anything like a 'feeling significance' or a 'symbolic reference.' The comparisons seem to have been based directly upon inherent differences within the sounds themselves. It is notable that the greatest difference between the first and the second sounds (as implied in the distribution of the reports) occurs in the *simplest* sounds, where the lower tone is reported 'larger' than the higher in a ratio of approximately 7:1 and the higher noise is reported 'smaller' than the lower in a ratio of approximately 20:1.

Again, 'magnitude' (and perhaps other graded qualities) may find a direct means of representation in the immediate visual perception of an articulating organism. The open mouth, the stretched jaw, the tense lip, all doubtless carry spatial and dynamic meanings which immediately enter into the specific characterization of the vocalized sound as heard.

Finally, the act of sound-production itself, which includes the feel of the tongue, lips, jaw, chest, and other parts of the body, contributes its large share to the sound as heard. As the context is favorable, this tactual and kinaesthetic component, variable in many directions and through wide ranges, probably serves in quite essential ways to convey its own meaning.

In all these cases, the materials serve, not by any inherent power of specific symbolization but rather as a graded and labile system, to inform the occasion—whether linguistic or not—in some appropriate and significant manner.

Psychology is deeply indebted to these phonetic studies for suggesting a number of interesting and significant problems which are now ripe for further research.

A QUALITATIVE AND QUANTITATIVE ANALYSIS OF A MENTAL TEST

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Numerous psychologists have recognized the possibility of the mental test movement losing itself in purely statistical refinement, while the qualitative aspects of the material measured are often entirely neglected. As early as 1914 Whipple says:

> Those who see in mental tests nothing but statistics of performance, and who contend that in the absence of introspection it is impossible to get any information upon mental content, have failed entirely to appreciate how significant for qualitative exploration are tests when handled by a competent investigator who puts less emphasis upon mere accumulation of figures than upon the patient search for qualitative aspects of mental life implicit in the outcome of his investigation.[1]

Mateer, in 1917, voiced similar opinions,[2] and more recently Crane has said:

> The data secured by psychometric technique are of immense importance to the clinical psychologist, as representing measures of traits, but there their contribution ends.[3]

With the rapid development of the test movement and with the present day stress on the quantitative psychology of individual differences, it is but natural that refinement of statistical method and creation of new tests have been of major interest. To be sure, leaders in the field have recently been stressing the necessity of a more penetrating analysis of the tests themselves, but hitherto this analysis has been confined almost exclusively to quantitative exploration. The stress upon quality rather than quantity as well as upon 'units of experience' or 'series of events' has received a great impetus from the more recent movements in psychology, as represented by the *Gestaltpsychology* of Köhler and Koffka, the *Ganzheitspsychologie* of Krueger, the *Personalismus* of Stern, the *Two Factor Theory* of Spearman,

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[1] G. M. Whipple, *Manual of Mental and Physical Tests*, 1914, 2.

[2] F. Mateer, Some criteria for the evaluation of mental tests and test series, *Mental Hygiene*, 1, 1917, 241.

[3] H. W. Crane, The limitations of psychometrics in clinical practice, *J. Abnor. & Soc. Psychol.*, 26, 1931, 199.

as well as from newer systematic attempts in this country, (Hollingworth, Bentley, and others).

Reymert's discussion of such a tendency,[4] and the present paper offers an account of a rather intensive qualitative and quantitative study of the well-known Knox Cube Test.[5]

Method and procedure. The material for this test consists of 5 cubes of the same size and color. Four of the cubes are placed in front of *S*, separated by intervals of 2 in. *E* then taps the blocks with the fifth cube in a certain definite order and at a certain definite rate (about one tap per sec.) always beginning with the cube at the child's left hand (*E*'s right if he is facing the child). *E* then lays the fifth cube down in front of the child equidistant between the third and fourth cube, but nearer the child, and says: "Do that."[6]

The series, consisting of 12 lines lettered from A to L, increases in difficulty supposedly forming a scale. A record of the number of lines passed or failed is kept. The examiner always continues with at least 3 lines after the child fails, and in many cases with more than 3 lines if there seems to be any possible chance of the child's succeeding in additional lines.

Subjects. We limited ourselves to an intensive study of 50 children and 100 adults. The children ranged in age from 13-14 yr. and the adults from 21-30 yr. The *S*s were tested at 2-week intervals, the children 8 times and the adults 5 times.

In addition to these results, other information was secured. *E* knew the children intimately and besides her knowledge of them gained from that source she gave them the following tests: "Silent Reading Test" (Gates), "The Multi-Mental Scale" (McCall), "The Standard Composition Test" (Hudleson), "Standardized General Survey Arithmetic Scale" (Monroe), "Intelligence Test," Form A and Form B (Terman), "Standard Reasoning Test in Arithmetic" (Monroe), and "Imagination Test" (Rorschach).

In retesting the children with the Knox Cube Test care was taken to work as nearly as possible at the same hour of the day as that of the first sittings. The tests were conducted in a separate room where the child was free from all disturbances, with no one but *E* present. The children, with very few exceptions, apparently enjoyed the test.

RESULTS

(1) Reliability of the test with children. It is apparent from Table I that the reliability of the Knox Cube Test for a group of 13-14 year old children is extremely low. The intercorrelations in the table were determined by the Pearson product moment method. Although the probably

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[4] M. L. Reymert, Kvalitative Faktorer I Mental-Maalninger (Qualitative Factors in Mental Measurements), *Arkiv for psykolgi och pedagogik*, 1928, 27-38; Personal equation in motor capacities, *Scand. Scient. Rev.*, 2, 1923, 177-222.

[5] H. A. Knox, A scale based on the work at Ellis Island for estimating mental defect, *J. Amer. Med. Assoc.*, 62, 1914, 742.

[6] This is the procedure used by R. Pintner and D. G. Patterson, *A Scale of Performance Tests*, 1925, 68.

errors of the individual coefficients are rather high, the consistent lowness of the coefficients reveal definitely the very low reliability of the test. The coefficients vary from 0.12 (correlation between first and seventh and third and seventh trials) to 0.58 (correlation between second and third trials). The average intercorrelation is only 0.36. If the first trial is considered as practice, the average intercorrelation of the remaining trials is 0.40. Since the first trial of the test is often used as a measure of an individual's ability, the question arises as to the reliability of this trial.

The data show that performance in the first trial correlates, on the average, the least with performance in the other trials. The coefficient is 0.27 and for the other trials it varies from 0.28 to 0.43. This indicates that the first trial is not a true index of the individual's ability for this task and that a standard performance is not obtained until a rather late trial, if ever. The correlation, between the first and the second trial is 0.29 for children, and 0.42 for adults.

TABLE I

INTERCORRELATIONS, MEANS, AND STANDARD DEVIATIONS OF SCORES OF 50 CHILDREN ON SUCCESSIVE TRIALS OF THE KNOX CUBE TEST

Trial	1	2	3	4	5	6	7	8
2	.29							
3	.26	.58						
4	.20	.51	.39					
5	.23	.52	.34	.48				
6	.31	.40	.39	.56	.43			
7	.12	.29	.12	.45	.26	.50		
8	.48	.40	.22	.44	.41	.41	.21	
Av. r of each trial	.27	.43	.33	.43	.38	.43	.28	.37
Av. when Trial 1 is excluded		.45	.34	.47	.41	.45	.31	.35
Means	8.70	9.32	9.28	9.30	9.58	9.86	9.88	9.92
S.D.	1.45	1.35	1.25	1.17	1.36	1.10	1.26	1.26

Av. r is 0.364; av. when Trial 1 is excluded is 0.396.

To determine whether a reasonable reliability could be obtained by increasing the number of items in the test, the values in Table II were calculated by means of the Spearman-Brown formula. The table shows that in order to attain a reliability of 0.90, which is a reliability that should be demanded of a test used for individual measurement,[7] the number of items in the test would need to be increased 25 times. The administration of such a test would require an unreasonable amount of time and would therefore prove a prohibitive task.

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[7] A similar idea is advanced by H. A. Edgerton and H. A. Toops when they make the statement that "the authors propose, for purposes of discussion, that no intercorrelation table should ordinarily be presented unless based on tests or units (those on which the intercorrelations are based) which have at least a reliability coefficient of 0.90, unless the reliability coefficients are given, or unless the nature of the experiment precludes the possibility of such reliability being attained." (A table for predicting the validity and reliability coefficients of a test when lengthened, *J. Educ. Res.,* 1928, 225.)

On the basis of the average correlation of a trial with each of the other trials, Table I shows that Trial 1 is the most unreliable of the eight trials; excluding Trial 1, the averages indicate no improvement in reliability with successive trials. If we consider Trial 1 a practice test and take Trial 2 as a measure of the child's ability, then the situation is improved somewhat, but the reliability of the test is still very low.

The average of the correlations between Trial 2 and each successive trial is 0.45. Lengthening the test in number of items for Trial 2 would improve the reliability more than it would in the case of trial one, but even then, as Table III shows, a lengthening of 11 times would be required to attain a reliability of 0.90. Since the average intercorrelation of all trials is only 0.36 and even the average intercorrelation of the last 7 trials is only 0.40, we would obtain even poorer results by using these values as the reliability coefficients.

TABLE II

Predicted Reliability of Trial 1 if the Test were Lengthened
(Av. correlation of Trial 1 with other trials = 0.27)

	Times lengthened						
	2	3	4	5	10	15	25
Predicted r	.42	.53	.60	.65	.79	.85	.90

Since we have the scores on the test for 8 trials we can actually determine what would be the reliability of combined scores for 2, 3, or 4 trials and in this way check some of the values predicted by means of the Spearman-Brown formula.

The correlation between the sum of the scores on Trials 2 and 3 and the sum on Trials 4 and 5 was determined.[8] This coefficient is 0.58. If the number of items in Trial 2 were doubled a reliability coefficient of 0.62 would be obtained. The correlation between the sum of the scores on the first 4 trials and the sum of the scores on the last 4 trials is 0.68; according to the Spearman-Brown formula, lengthening Trial 2 four times will give a reliability of 0.77.

The reason for the discrepancy between the predicted and the empirical result is easily discovered. The assumptions underlying the Spearman-Brown formula are that the standard deviations of the successive tests are all equal and that the reliability coefficient used for prediction is equal to the average of all the possible intercorrelation coefficients. The coefficient 0.45, used as the reliability coefficient of the second trial in predicting the values of Table III, is 0.09 larger than the average of all the possible intercorrelation coefficients which is 0.36. If this latter value is used as the initial reliability from which to predict the reliability of a test of four times the original length, the predicted value is 0.69 which is only 0.01 larger than the actual correlation between the sum of the scores in the first 4 trials and the sum of the scores on the last 4 trials.

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[8] The general formula for determining the correlation between weighted sums is given by T. L. Kelley, *Statistical Method*, 1924.

It may be concluded that neither the predicted results nor the empirical results give evidence that satisfactory reliability might be obtained for a reasonable length of the test.

When scores are used for individual diagnosis, the probable error of a score is a more significant measure of reliability than is the reliability coefficient. The formula for probable error of a raw score which was used is $P.E._{r.s.} = 0.6745\ S.D.\,(1-r_{12}^2)^{\frac{1}{2}}$. The probable error of a raw score based on the standard deviation and reliability coefficient of Trial 1 and the probable error of a raw score based on the standard deviation and reliability coefficient of Trial 2 are:

TABLE III

PREDICTED RELIABILITY OF TRIAL 2 IF THE TEST WERE LENGTHENED

(Av. correlation of Trial 2 with later trials = 0.45)

	Times lengthened					
	2	3	4	5	10	11
Predicted r	.62	.71	.77	.80	.89	.90

Trial	$P.E._{r.s.}$	S.D.	Reliability Coefficient
1	.94	1.45	.27
2	.81	1.35	.45

The smaller probable error is 0.8. The meaning of this probable error is that if a score of 6 is made on one trial, there is a one to one chance that the score made on another trial will lie between 5.2 and 6.8, and a chance of about 4 to 1 that the score will lie between 4.4 and 7.6.

Pintner gives the average number of lines passed on the Knox Cube Test by groups of children of various ages as follows:[9]

	Age in Years																
	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Adult
Median No. Correct	1	1	2	4	5	6	6	6	7	7	7	8	7	8	9	10	8

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[9] *Op. cit.*, 137, f.n. 6.

If these averages are used as age-norms, then a score of 6 corresponds approximately to the age of 10 yr. From the explanation of the probable error above it is plain that one-half of the individuals classified as 10 yr. of age on one trial would on another trial probably be classified below 8 or above 13, and that one out of five would be classified below 7 or above 15. In view of such large errors in classification of individuals, there seems to be hardly any value at all to the Knox Cube Test. This conclusion may be reënforced by the statement of Mateer that "children whose mental ages differ as much as ten years may score the same on this test."

TABLE IV

Intercorrelations, Means, and Standard Deviations of Scores of 100 Adults on Successive Trials of the Knox Cube Test

Trial	1	2	3	4	5
2	.42				
3	.38	.48			
4	.36	.44	.55		
5	.33	.59	.54	.54	
Av. r of each trial	.37	.48	.49	.47	.50
Av. when Trial 1 is excluded		.50	.53	.51	.56
Means	8.84	9.65	9.61	10.08	10.05
S. D.	1.59	1.56	1.73	1.42	1.40

Av. r is 0.463; av. when Trial 1 is excluded is 0.524.

(2) Reliability of the test with adults. In the case of 100 adults, the situation, with respect to reliability, is practically the same. The intercorrelations given in Table IV are all rather low, the average being only 0.46. If Trial 1 is omitted, the average of the intercorrelations of the remaining trials is 0.52.

Table V shows that a lengthening of 15 times would be required to attain a reliability of 0.90 if 0.37, the average of the correlations of the Trial 1 with each of the others, is used as the original reliability coefficient. According to Table VI, if 0.50, the average correlation of Trial 2 with each of the later trials is used, a lengthening of 9 times is still required.

TABLE V

Predicted Reliability of Trial 1 if the Test were Lengthened

(Av. correlation of Trial 1 with other trials = 0.37)

	Times lengthened					
	2	3	4	5	10	15
Predicted r	.54	.64	.70	.75	.86	.90

We saw that with the data on the children summarized in Table I, the average intercorrelation of all trials (0.36) and the average intercorrelation of the last seven trial (0.40) were both lower than the average correlation of Trial 2 with every one of the later trials (0.43). The data on adults, summarized in Table IV, show that the average correlation of Trial 2 with every one of the later trials is 0.50, the average intercorrelation of all trials is 0.46, and the average intercorrelation of the last four trials is 0.52.

TABLE VI

PREDICTED RELIABILITY OF TRIAL 2 IF THE TEST WERE LENGTHENED

(Av. correlation of Trial 2 with later trials = 0.50)

	Times lengthened				
	2	3	4	5	9
Predicted r	.67	.75	.80	.83	.90

Assuming this latter figure to be the reliability of the test, Table VII indicates what the reliability coefficients would be if the test were lengthened. Even by taking this highest figure as the reliability of a trial a lengthening of eight times would still be required in order to attain a reliability of 0.90.

As for the children, an empirical check on the values predicted by means of the Spearman-Brown formula was attempted. The correlation between the sum of the scores of Trials 2 and 3 and the sum of the scores of Trials 4 and 5 were found to be 0.71. If 0.52 is considered to be the initial reliability of the test, Table VII shows that doubling the test should yield a coefficient of 0.68. The difference between empirical and theoretical result is only 0.03. If 0.50 is used as the initial reliability, Table VI shows that by formula doubling the test should yield a reliability of 0.67. This is 0.04 smaller than the empirical result 0.71.

TABLE VII

PREDICTED RELIABILITY OF THE LAST FOUR TRIALS IF THE TEST WERE LENGTHENED

(Av. intercorrelation of last four trials = 0.52)

	Times lengthened				
	2	3	4	5	8
Predicted r	.68	.77	.81	.84	.90

For the adults the probable error of a raw score calculated from the standard deviation and reliability of Trial 1; and the probable error of a raw score calculated from the standard deviation and reliability of Trial 2 are:

Trial	P.E. $_{r.s.}$	S.D.	Reliability coefficient
1	1.00	1.59	.37
2	.91	1.56	.50

Both of the probable errors, 1.00 and 0.91 are larger than were these values for the group of children. Thus we see that although in the case of adults the intercorrelation coefficients are on the average slightly higher than in the case of the children, the probable error of raw scores is somewhat larger for the adult group.

It may clearly be seen from the foregoing that almost an innumerable number of variables, most of which are not under control, will determine the results of this test, of so-called "imitation." We may mention here, in spite of Knox's assumption to the contrary, that learning enters into this test and is taking place from first to last trial both for children and adults. The different degree of ability in the various individuals for learning such a task as this test presents, then will, of course, be a very definite factor of disturbance in our rankings.

TABLE VIII

Correlation between Various Tests and the Knox Cube Test

Test	r	P.E.
Pressey's Arithmetic Test in Reasoning	.02	.09
Monroe Multi-Mental Scale	.14	.10
Monroe Arithmetic	.09	.10
Hudleson's Composition Test	.12	.09
Gates Silent Reading Test	.21	.09
Rorschach Imagination Test	.00	.09
Terman Intelligence Form A	.25	.09
Terman Intelligence Form B	.01	.09

(3) Quantitative results and their relation to other measures supposedly involving intelligence. In order to find out the possible influence of general intelligence or certain special abilities, the 50 children were subjected to standard tests and the results were correlated. These data are listed in Table VIII.

The abilities tested through the above tests (as claimed by the respective authors of them) such as reasoning, logical memory, ability to organize, imagination, skill in figuring, etc., apparently do not enter into the performance on the Knox Cube Test to any significant degree. The highest correlation was found to be between the Terman Intelligence, Form A, and our test, but as will be seen this also is exceedingly low (0.25).

It might be appropriate here for further illustration of the discrepancies of Knox Cube scores with measures of general intelligence to present the table above which offers an outlook over the relationship between Knox Cube scores and Terman I.Q. for 14 pupils in the Opportunity Room of the Troy Public School.

(4) Qualitative aspects of the Knox Cube Test. We first endeavored to discover the variety of methods of procedure employed in solving a task of the nature of this test. Pintner tried to throw some light on this by occasionally asking the candidates how they did it. We questioned all our *O*s at the end of each sitting: "I wonder if you can tell me exactly how you remembered to tap the cubes in the same order as I did." This generally brought full spontaneous reports from both children and adults. An amazing array of differences of procedure was apparent.

In regard to the children, we first of all made it a point to find what percentage would be stable, *i.e.* would use one and the same method throughout all consecutive trials in spite of the rather long time interval. Five out of the group of 50 children (10%) reported the same method throughout every one of the 8 sittings, never at any time varying that procedure. When these children were tested one year after the eighth trial, they again reported identically the same procedure. Of the remaining 45, the majority did not only jump from one procedure to another at consecutive sittings but would change their procedure frequently from trial to trial within one and the same sitting. All experiential reports from the *O*s were taken down verbatim, and analyzed. On the basis of these analyses it was possible to identify at least two types of procedure used by the children. These we denote as (1) voco-motor procedure, (silently naming the cubes in some way or other, A, B, C, etc., or by number, 1, 2, 3, etc.) for example: "I called each cube a number and then said the numbers as you tapped them," "I called the cubes A, B, C, D, and then said the letters to myself as you tapped that certain cube." Thirty percent of the children used this method predominantly. (2) Purely visual memory (apparently relying on pure sensory visual memory without the aid of any cues) examples are: "I don't know, I just watched," "I did just what you did," "I got a picture of what you did and did the same thing." Forty-four percent of the children used this method predominantly. A few used what we denoted as "auditory" (guided by the sounds of the taps). Examples are: "I listened for one melody which I sang to myself as you tapped the cubes, each one had a slightly different sound," "I listened for the difference in the time it took to go from one cube to the other."

The remaining children were so changeable from trial to trial and from sitting to sitting in regard to the above procedures that we found it impossible to typify them.

On the basis of all the above, and especially with later reference to the bearing of these various procedures upon the quantitative scores, we do not find ourselves able to accept the summary statement of Pintner.

> Without attempting to enter into a useless discussion as to the actual mental processes involved, we may say in a general way that it depends largely upon imitation, at the same time affording every opportunity for other factors involving intelligence to assert themselves.[10]

The type of procedure found by Pintner in his standardization of the test, which he calls "the error of perseveration" was never found as was to be expected from Pintner's own statement that this is seldom met with among older children.[11] "The error of reverse order" occurred three times, twice on line A and once on line X.

We found with the adults, as with the children, a great variety of procedures with two main groups predominating; namely, (1) the voco-motor and (2) the purely visual memory. This is contrary to what might be expected from Pintner.

> The other kind of counting which assigns constant numbers or letters to each block was found to be very rare. We found it only in about twenty cases out of our 867 individuals. This kind of counting did not seem to occur to the majority of adults who were tested by us.[12]

Twelve percent of the adults showed consistency in using one and the same procedure throughout as has been found among 10% of the children. Thirteen percent fluctuated regularly in their procedure in one and the same sitting as compared to 20% of the children.[13]

(5) Relationship between type of procedure and quantitative scores. It

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[10] R. Pintner, Standardization of Knox Cube Test, *Psychol. Rev.*, 22, 1915, 377-401.

[11] *Ibid.*, 397.

[12] *Ibid.*, 380.

[13] Some said: "I organized the cubes which you tapped into a sort of pattern, so that I took one in relation to the other, all of them together forming a unit." Seventy-four verbal reports out of a total of 252 in the purely visual memory group were of this type. These had superior scores.

Another group who "just watched" discovered that the difficult combination was built upon the simple basic pattern of the scheme of presentation and so focused their attention on the new added part for learning in each successive trial of the sitting. There were some in the visual group who said they "just looked" or "just did what you did." Others gave reports as the following: "I saw the curves you made as you touched the cubes, going from one to the other. At the same time I could see the straight lines from one cube to another and I formed a mental picture of the figures made."

There were still others who said, "I 'felt' keenly the number of taps as a sort of rhythm. The rest of the procedure was visual." Those who organized in patterns on "feel" of rhythm entertained an unpleasant feeling if they were unable to execute the same number of taps as given by the *E.* They frequently put their cube down on what they knew was, at the time, the wrong cube, from some kind of a feeling of suspense which more or less compelled them to tap again until they had the right number of taps. The number of taps was felt as something of a rhythmic closed whole and never analyzed into smaller part wholes. The final closure was accompanied by a distinct feeling of satisfaction, whether rightly or wrongly executed. This seemed to us as example of temporal sequence as discussed by Köhler in his *Gestalt Psychology,* 164-165.

now seems natural to inquire into the question whether there is such a thing as an optimum method for doing the task. In a general way, we may state that there does not seem to be any such definite method. This statement should be qualified, however, to the effect that the few adults who always perceived the presentation as a unified whole or pattern or design, and who in each successive trial just added a new feature to a pattern formerly experienced, in other words, those who discovered and consciously used

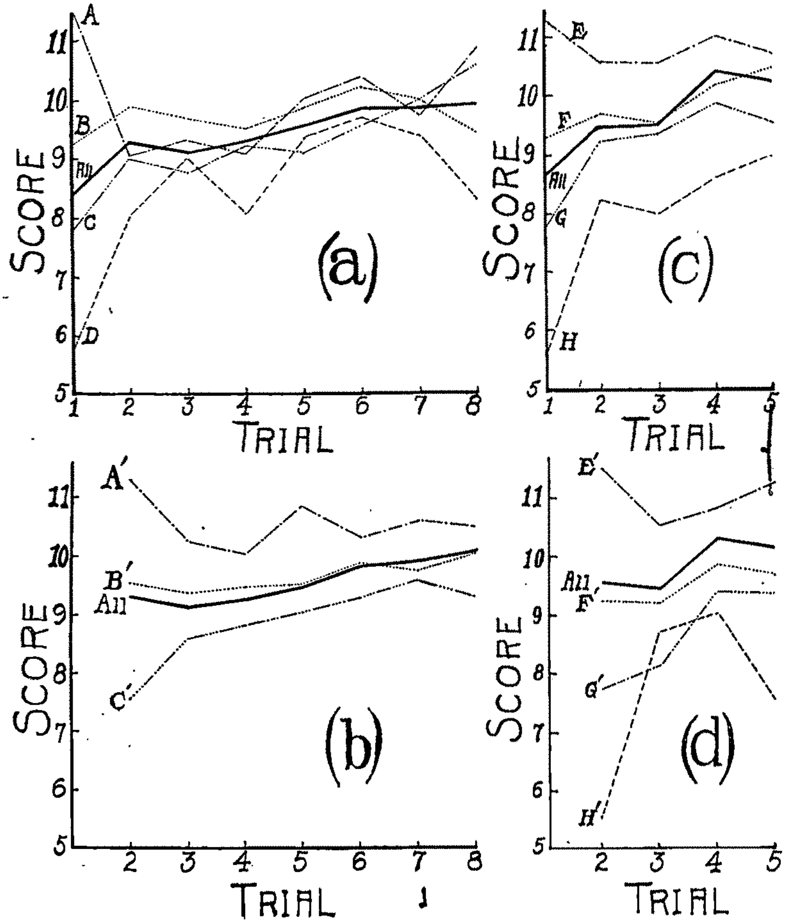

Fig. 1. Curves Showing Means of Sub-Groups for Every Successive Trial

(a) Children: Sub-groups selected on basis of score on Trial 1.
(b) Children: Sub-groups selected on basis of score on Trial 2.
(c) Adults: Sub-groups selected on basis of score on Trial 1.
(d) Adults: Sub-groups selected on basis of score on Trial 2.

the plan underlying the construction of the progressive series from the first to the twelfth trial, had superior scores. The visual group of the children gave the highest scores with the voco-motor group coming next, while the group which changed procedure very often gave the poorest score. With adults, however, the highest score out of our two main groups was found in the voco-motor, changeable group coming next and the visual group coming last. The finding that purely visual group of the children gets the highest score agrees well with an earlier study of Reymert.[14] The result for the adults that they do better with voco-motor procedure also seems a natural one. The fact that the children do worse while frequently changing method and that adults do better when changing method would find its explanation in the fact that, while the change with the children may be a haphazard affair, with the adults it most likely means a definite scheme for improving previous method.

TABLE IX

Scores of Sub-groups of Children on Successive Trials, when Score on Trial 1 is Considered Initial Ability

Sub-group	Score trial 1	Trial 1	2	3	4	5	6	7	8	No. cases
A	11–12	11.17	9.00	9.33	9.17	10.00	10.33	9.83	10.67	6
B	9–10	9.38	9.90	9.71	9.57	9.95	10.14	10.00	10.43	21
C	7– 8	7.70	9.00	8.85	9.25	9.10	9.45	9.85	9.40	20
D	5– 6	5.67	8.00	9.00	8.00	9.33	9.67	9.33	8.33	3
All	5–12	8.70	9.32	9.28	9.30	9.58	9.86	9.88	9.92	50

The possible relationships of general intelligence entering into either choice of procedure or persistence of procedure we inquired into by taking the upper quartile and the lower quartile of the Terman I.Q. and comparing these two groups in this regard. No clear differences were found.

(6) Learning. The curve of learning for the group of children is indicated in Fig. 1 (a) by the solid line which connects the means of the scores on successive trials. Most of the learning seems to take place between the first and second trials. Table IX shows that the difference between the means of the first and last trials is 1.22 (P.E. = 0.13), which is over 9 times the probable error of the difference. The difference between means of second and last trials (Table X) is 0.60 (P.E. = 0.13), which is nearly 5 times its probable error. Although the total amount of learning seems to be considerable, the learning from trial to trial is much less. Dividing 1.22 by 8, we find an average learning of 0.15 per trial. For the last 7 trials the average is 0.09.

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[14] *Op. cit.,* f.n..4.

The fact that more learning does not take place after the second trial may be due in part to the fact that the task has not sufficient range of difficulty for the group tested, since several of the group attained the highest possible score at the first sitting. Moreover, from the nature of the task, it is evident that it is a scale, that is, the difficulty of the items increases.

In order to determine whether there is any difference in learning between the poorer and the better children, the group was divided into four sub-groups on the basis of the scores made on the first trial.

In Table IX are given the means of the scores of each sub-group for successive trials. The extreme groups A and D show considerable variation, which is to be expected since they contain only a few cases.

Sub-groups B and C have curves which are very nearly parallel to the curve of the whole group so that it appears that there is not a great deal of difference in the improvement in scores, of the poorer and better individuals. The group was divided also on the basis of the scores made on Trial 2. Table X gives the means of the scores made by these sub-groups on successive trials. No scores of 5 or 6 were made on Trial 2. The sub-groups again show the same approximately parallel rise in Fig. 1 (b) as did the sub-groups A, B, C, and D, formed on the basis of scores on the first trial.

TABLE X

SCORE OF SUB-GROUPS OF CHILDREN ON SUCCESSIVE TRIALS, WHEN SCORE ON TRIAL 2 IS CONSIDERED INITIAL ABILITY

Sub-group	Score trial 2	Trial							No. cases
		2	3	4	5	6	7	8	
A′	11–12	11.33	10.33	10.00	10.89	10.33	10.56	10.44	9
B′	9–10	9.50	9.32	9.39	9.46	9.93	9.82	10.00	28
C′	7–8	7.54	8.46	8.62	8.92	9.38	9.54	9.38	13
All	7–12	9.32	9.28	9.30	9.58	9.86	9.88	9.92	50

The adult group was treated in the same way with approximately the same results as can be seen from Table XI and XII.

It cannot be said that there is either convergence or divergence in ability to perform the task set by the test. Although in the case of both the children and the adults there is greater variability on the first trial than on later trials, it must be remembered that there is probably a limitation in range at the upper end of the distribution and that the task is a scale in which the steps increase in size.

Judging from the standard deviations of the first and last trials for both adults and children, there appears to be convergence in the ability tested. Since Peterson maintains that the coefficient of variability is the proper

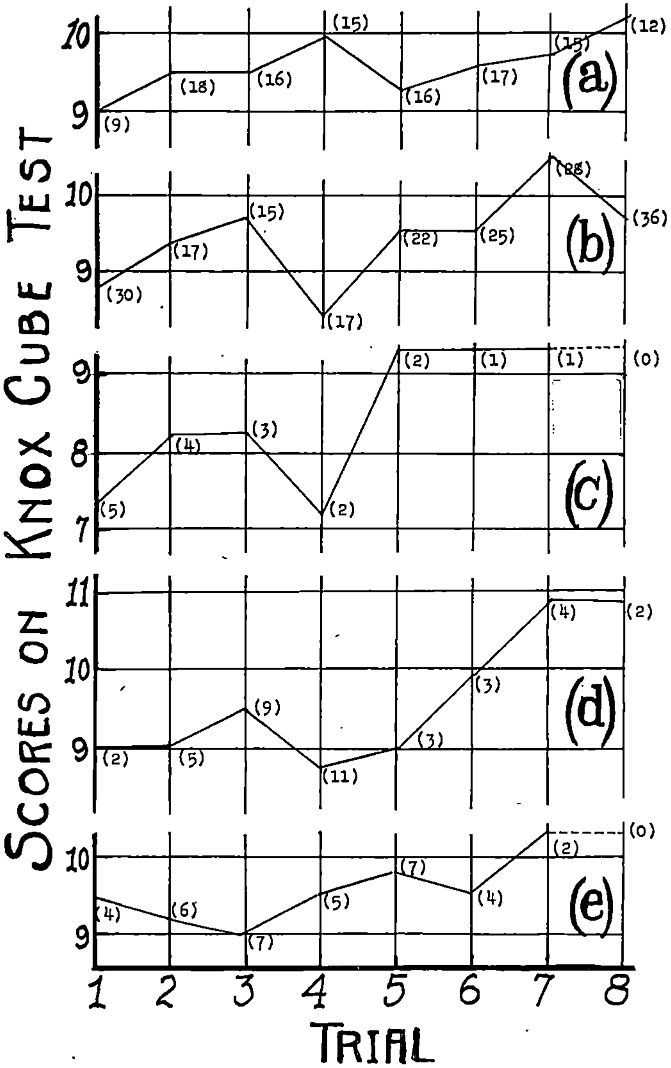

FIG. 2. LEARNING CURVES PLOTTED FROM MEAN SCORES ON SUCCESSIVE TRIALS OF CHILDREN USING VARIOUS TYPES OF PROCEDURE

(a) Voco-motor; (b) Visual; (c) Auditory; (d) Changed from voco-motor to visual; (e) Changed within trial from visual to voco-motor. Figures in parentheses give the number of subjects using the procedure.

measure to be used in determining whether there is an increase or a decrease in individual differences with practice, the coefficients of variability (S.D./M) were calculated and are presented in Table XIII.[15]

These coefficients do show a slightly greater tendency to decrease than do the standard deviations. However, that decrease of variability coefficients with practice really shows convergence in the ability tested should be questioned. Peterson did not demonstrate that. At any rate, the objections previously mentioned still apply; namely, limitation in range of difficulty of the test and inequality in difficulty of the units.

TABLE XI

SCORES OF SUB-GROUPS OF ADULTS ON SUCCESSIVE TRIALS, WHEN SCORE ON TRIAL 1 IS CONSIDERED INITIAL ABILITY

Sub-group	Score trial 1	Trial 1	Trial 2	Trial 3	Trial 4	Trial 5	No. cases
E	11–12	11.16	10.68	10.68	10.95	10.74	19
F	9–10	9.47	9.81	9.67	10.08	10.28	36
G	7– 8	7.59	9.23	9.28	9.87	9.67	39
H	5– 6	5.83	8.17	8.00	8.67	9.00	6
All	5–12	8.84	9.65	9.61	10.08	10.05	100

The next problem to inquire into would be the question of learning from trial to trial for the various procedures as well as for changes of procedure. Because they contain the greatest number of frequencies, or rather the greatest precent of subjects, the curves of average scores, per sitting in the visual group and that of the voco-motor group are most reliable. The curve for the voco-motor type, Fig. 2(a), for the children shows definite, but very slightly marked learning, while the curve for the adults, Fig. 3(a), shows a great rise. This seems to be natural.

TABLE XII

SCORES OF SUB-GROUPS OF ADULTS ON SUCCESSIVE TRIALS, WHEN SCORE ON TRIAL 2 IS CONSIDERED INITIAL ABILITY

Sub-group	Score trial 2	Trial 2	Trial 3	Trial 4	Trial 5	No. cases
E′	11–12	11.39	10.65	10.84	11.13	31
F′	9–10	9.51	9.51	9.96	9.82	49
G′	7– 8	7.75	8.13	9.25	9.25	16
H′	5– 6	5.50	8.75	9.00	7.75	4
All	5–12	9.65	9.61	10.08	10.05	100

The visual group of children, Fig. 2(b), shows rather marked improvement from sitting to sitting, while we note just a slight rise for the adults in this group, Fig. 3(b). For the children a systematic change from voco-motor to visual, the latter perhaps being a more natural method for children, shows a definite rise, Fig. 2(d), while the same curve for the adults has a downward trend, Fig. 3(e). On the other hand, the change from visual to voco-motor for children seems, from the curve Fig. 2 (e), just a fumbling proposition, but might have reached a conscious adult usage at the seventh sitting, while again, as would be expected, the change from visual to voco-motor, Fig. 3(d), for adults shows more of a definite rise. From this it can be seen that the qualitative differences of method have a marked influence on the nature of the learning curve.

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[15] J. C. Peterson, A new device for use in teaching, testing and research in learning, *Trans. Kansas Acad. Sci.*, 33, 1930, 41-47.

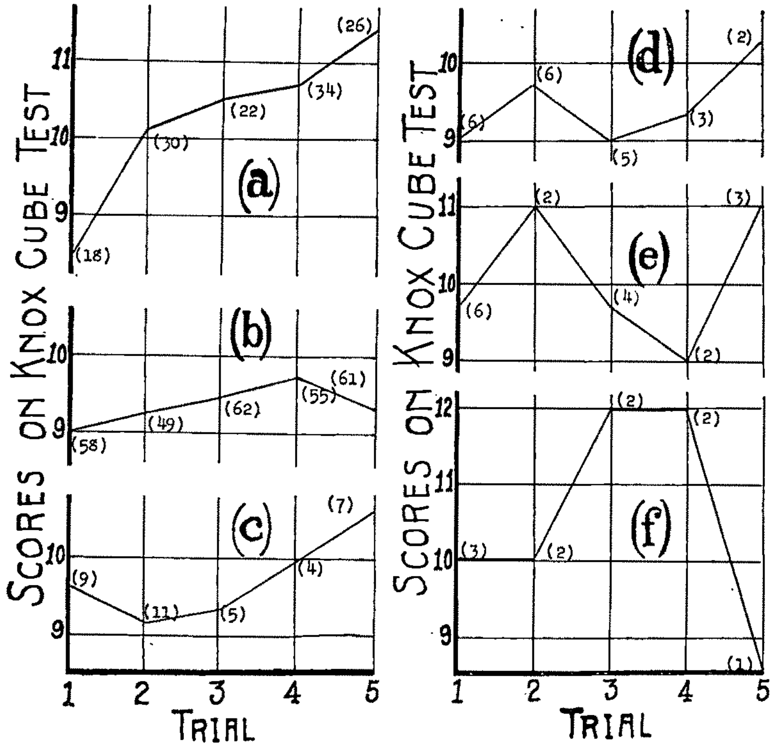

FIG. 3. LEARNING CURVES PLOTTED FROM MEAN SCORES ON SUCCESSIVE TRIALS OF ADULTS USING VARIOUS TYPES OF PROCEDURE

(a) Voco-motor; (b) Visual; (c) Visual-auditory; (d) Changed within trial from visual to voco-motor; (e) Changed within trial from voco-motor to visual; (f) Difficult patterns based upon simple ones. Figures in parentheses give the number of subjects using the procedure.

(7) Social status. Although this group was unselected with respect to social background, a comparison of the upper quartile with the lower quar-

TABLE XIII

MEANS, STANDARD DEVIATIONS, AND COEFFICIENTS OF VARIABILITY FOR THE VARIOUS TRIALS

		Trial							
		1	2	3	4	5	6	7	8
Adults	Mean	8.84	9.65	9.61	10.08	10.05			
	S.D.	1.59	1.56	1.73	1.42	1.40			
	S.D./M	.18	.16	.18	.14	.14			
Children	Mean	8.70	9.32	9.28	9.30	9.58	9.86	9.88	9.92
	S.D.	1.45	1.35	1.25	1.17	1.36	1.10	1.26	1.26
	S.D./M	.17	.14	.13	.13	.14	.11	.13	.13

tile as to home environment demonstrates the well-known fact that the children coming from homes of the higher social status possess, on the whole, superior intelligence (Table XIV).

TABLE XIV

COMPARISON OF THE SOCIAL STATUS OF THE UPPER AND LOWER QUARTILES

	Pupil	I.Q.*	Father	Mother	Knox Cube
Upper Quartile	A	134	Minister	College graduate	9 3/4
	B	127	Surgeon	Music teacher	11 3/8
	C	125	Doctor	Trained nurse	11 5/8
	D	123	Salesman		11 3/8
	E	120	Cook		9 3/8
	F	119	Asst. P.M.	Printer	9 7/8
	G	118	Grocer		9 5/8
	H	118	Insurance	H. S. Graduate	9 1/2
	I	117	Florist	H. S. Graduate	8 3/8
	J	115	Laborer		9
	K	114	Factory Supt.		9 7/8
	Av.				9.98
Lower Quartile	P	99	Mechanic		8 1/4
	Q	99	Butcher		8 7/8
	R	99	Factory Foreman		9 5/8
	S	98	Laborer		9 1/4
	T	98	City Fireman		8
	U	97	Laborer		9 7/8
	V	97	Factory Foreman		9 1/4
	W	94	Sheet Metal Work		9 5/8
	X	93	Odd Jobs		8 3/8
	Y	92	Factory Man		8 7/8
	Z	90	Factory Man		9 3/8
	Av.				9.03

* Computed from scores on Stanford-Binet tests.

SUMMARY AND CONCLUSIONS

The present study is an intensive exploration of the Knox Cube Test, made for the purpose of emphasizing the importance of the qualitative aspects of test results, as against the aspects gained by techniques of standardization or by refined statistical procedures.

With the method and procedure standardized by Pintner, 50 children (aged 13-14 yr.) were tested 8 times at two-week intervals, and 100 adults (college summer school students), 5 times at similar intervals. General information about the subjects, other standard test scores, and verbal reports were used as a check on the factors influencing the scores on the Knox Cube Test. Summary of the results follows:

(1) Quantitative Results. (a) As computed by the Pearson product moment method, the test from trial to trial revealed surprisingly low correlation for both children and adults. The average intercorrelation of all the trials is 0.36. Considering the first trial as practice, the average intercorrelation of those remaining is raised to 0.40.

(b) The first trial, frequently taken as the final estimate of ability, correlates least with successive trials. As calculated by means of the Spearman-Brown formula, the number of items in the first trial must be increased 25 times in order to approximate a reliability of 0.90.

(c) Taking trial two as the final score, the reliability is 0.45 and the length of the test must be increased 11 times in order to approach the reliability of 0.90.

(d) From these calculations, it may be concluded that neither theoretical nor empirical results give evidence that a satisfactory reliability might be obtained from a reasonable length of the test.

(e) The correlations of Knox Cube scores with scores of specific abilities and general intelligence tests are very low. The highest correlation, 0.25, was with the rank of I.Q. in Terman A and the rank in the first trial of the Knox Cube Test.

(2) Qualitative Results. (a) From the experimental reports, the qualitative aspects of this much used test in regard to procedures employed for solving the task, are manifold and varied, and the variations of method from sitting to sitting in the same individual seems to be the rule rather than the exception for both children and adults.

(b) Two or three typical procedures can be made out somewhat arbitrarily (voco-motor naming or numbering, purely visual sensory memory, etc.), but the relation of these to excellency of capacity is obscure. The most superior method is probably "the organization of the cubes tapped into a pattern or unit" (recalled as a whole).

(c) Comparing the types of procedure with intelligence, according to Stanford-Binet I.Q. reveals a slight relationship but is accompanied by too many exceptions to be considered significant.

(3) Learning. (a) The learning curves of the various groups reveal

marked differences between the children and adults. The children in the voco-motor group show a definite but slight rise, while the adults show a great rise. The visual group, on the other hand, shows great improvement among the children but only slight for adults.

(b) The actual learning from sitting to sitting was slight, averaging 0.15 per trial in raw score and being greatest between the first and second trials.

(c) Dividing the subjects according to scores on the first trial, it cannot be said that there is either convergence or divergence in ability throughout consecutive sittings. There is greater divergence on the first sitting but this might be attributed to the fact that there is a limitation in the range of difficulty which may account for lack of greater divergence in later sittings.

(4) Social status. Although the data indicate that the median intelligence levels of groups of varied social status differ, they do not point to a uniform grade of ability in any group.

(5) General conclusions. In spite of seemingly adverse facts against using the Knox Cube Test as a measure of ability, it may still prove useful in helping to differentiate subnormal levels of general intelligence. It is hoped that this study will suggest that, in order to use such tests as a finer measure of ability, there must be a qualitative analysis accompanying the quantitative standardization of the data.

After analysis of the scores obtained on such a simple test of intelligence, it would appear that very little information is gained concerning ability by knowing an individual score.

RELATION OF EYE-MOVEMENTS TO VISUAL IMAGERY

By Crawford Goldthwait, Harvard University

The fact that the visual perception of an object is usually accompanied by movements of the eyes suggests the possibility that the centrally excited visual image of such an object possesses an efferent component of ocular movement. Experiments by G. M. Stratton,[1] and Judd, McAllister and Steele,[2] among others, have demonstrated that in visual perception the eyes do not necessarily follow accurately the outlines of the object, so that it is scarcely to be expected that they would do so in the case of an image. Nevertheless, there remains the possibility that the amount or character of the movements may bear some relation to the type of image or to one or more of its aspects.

In addition to the numerous experiments concerned with imagery as such there have been a few which have emphasized the accompanying motor activity and have attempted to discover relationships between the aspects of the visual image and characteristics of the simultaneously occurring eye-movements. One such experiment, carried out by C. W. Perky,[3] yielded results pointing to the conclusion that visual "memory images" (images of particular things, in a particular context and having personal reference) were accompanied by a markedly greater amount of eye-movement than were "images of imagination" (images of things recognized but lacking the particularity characteristic of "memory images"). Perky found also that images of the memory type were usually "scrappy" and "filmy," while those of imagination were clear and stable. Detection of eye-movement was made dependent upon the *O*'s awareness of faint luminous spots (on a dark field) normally falling within the blind spot but so placed as to stimulate sensitive marginal areas when the eye wandered from its position of fixation. Experiments by L. J. Martin[4] and R. M. Ogden[5] failed to support

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* Accepted for publication October 1, 1931.

[1] G. M. Stratton, Eye-movements and the aesthetics of visual form, *Philos. Stud.*, 20, 1902, 336-359.

[2] C. H. Judd, C. N. McAllister, and W. M. Steele, Introduction to a series of studies of eye movements by means of kinetoscopic photographs, *Psychol. Monog.*, 7, 1905, (no. 29), 1-16, and following articles.

[3] C. W. Perky, An experimental study of imagination, this JOURNAL, 21, 1910, 422-452.

[4] L. J. Martin, Die Projectionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder, *Zsch. f. Psychol.* 61, 1912, 322-546.

[5] R. M. Ogden, Experimental criteria for differentiating memory and imagination in projected visual images, *Psychol. Rev.*; 20, 1913, 378-410.

Perky's findings with respect to the characteristics of memory images and images of imagination (neither *E* used apparatus for recording ocular movement), and H. Clark,[6] as a result of experiments similar to Perky's, concluded that the amount of eye-movement varied, not with the type of image involved, but with "conditions of attention." For detecting eye-movement Clark used an improved form of Perky's apparatus, the nystagmograph of A. Schackwitz and the method of directly observing through an enlarging glass a point of light reflected from the cornea.

The experiments described below constitute an attempt to resolve the inconsistencies of the earlier work. Objective records of eye-movements seemed highly desirable for a foundation and two recorders were therefore utilized, yielding, it is believed, a rather high degree of accuracy for the small movements involved. One, the *mirror-recorder,* recorded movements of the closed eye by means of a small mirror held against the lid by a device designed by R. Dodge.[7] The mirror turned on a vertical axis when the bulge of the cornea moved behind the lid, and the beam of light which it reflected moved across a sheet of flashed glass on which was focused a camera with motor-driven film. It permitted the recording of horizontal movements only, however, and was replaced by the *ciné-recorder.* This device was made up of a cine-matographic camera, taking pictures directly of the open eye, and machinery for projecting the film, frame by frame, and recording changes in the position of the iris. Both recorders were so designed that head-movement was eliminated from the final record, and both were equipped with appropriate mechanisms for recording the giving of the stimuli, the beginning and end of the image, identification of the record and time in fractions of a second.

Experiment I. The mirror-recorder was used. The *O* was instructed to get a visual image based on the substantive spoken by *E.* He was to signal when the image appeared and disappeared and was then to describe it. No reference was made to type of image in the instructions. The substantives, of which 80 were used, were chosen to render memory images available and to occasion as little eye-movement as possible.

There were 4 *Os,*[8] who gave 198 records. The images were classed as M-images (those of specific objects, in a particular experiencial setting), S-images (those of specific objects, but lacking the particular setting) and

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[6] H. Clark, Visual imagery and attention, this JOURNAL, 27, 1916, 461-492.

[7] R. Dodge, A mirror recorder for photographing the compensatory movements of closed eyes, *J. Exper. Psychol.,* 4, 1921, 165-174.

[8] Eight *Os* took part in these experiments: three seniors, four graduate students and one member of the faculty.

I-images (those of no specific object). They were also classed, in accordance with the *O*'s report, as clear, moderately clear or dim; as stable, moderately stable or fluctuating.

In Table I, which gives the results of this experiment,[9] it will be seen that only in the case of dim imagery is the amount of movement sufficiently different from the general average to be of significance. The figure for "moderately stable," being the datum for only one image, is of little experimental value.

Experiment II. With the exception that the 4 *O*s were instructed to try for "images that represent objects which have had a place in your remembered experience" (M-images) in half of the cases, the procedure was the same as for Experiment I. The yield was 238 records, data for which appear in Table II.

TABLE I

SHOWING THE RESULTS OF EXPERIMENT I

	M-image	S-image	I-image
No.	17	34	147
Av. movement	31	21.2	23.6
m.v.	29.4	14.5	16.3
	Clear	Mod. Clear	Dim
No.	150	16	32
Av. movement	20.2	19.3	43.1
m.v.	12.7	13.8	33.8
	Stable	Mod. Stable	Fluct.
No.	181	1	16
Av. movement	23.4	168	19.7
m.v.	16.5	—	13.0

The tendency here for dim imagery to be accompanied by larger amounts of eye-movement is somewhat obscured in the table because 2 *O*s reported no "dim" images but registered markedly increased movement for their "moderately clear" images. Again, the significance of the data for stability of images is lessened by the facts, first, that 2 *O*s showed a definite tendency toward decreased movement for the less stable imagery, and, second, that the general trend was in opposition to that of the earlier experiment.

Experiment III. Following a short preliminary experiment with the ciné-recorder, the meager results of which were in agreement with those of the previous work, it was determined to use that apparatus in obtaining a series of images graded (by the *O*) on a clearness scale of 1 to 10, in order that correlations might be worked out between degree of clearness and amount of accompanying eye-movement. There resulted 115 records. Coefficients obtained for the 4 *O*s were —0.60 ± .082, —0.60 ± .079, —0.56 ± .085 and —0.62 ± .075, indicating a marked tendency for images of the greatest clearness to be accompanied by the least eye-movement. The four subjective scales were not, of course, comparable, but the coefficient —0.46 ± .049, obtained from the combined data for all *O*s, reveals a fair degree of uniformity.

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[9] Data in Tables I and II are in terms of arbitrary units of length as measured on the developed film. They represent amounts of eye-movement per unit of time during the presence of the images.

Experiment IV. In order to get a basis for comparison with Perky's characterizations of memory and imaginative images, 4 *O*s were asked for careful reports on 10 images of each kind, attention being directed to clearness, stability, detailedness, completeness and duration. Stimulus words were of the same sort as those used in the other experiments and the program was continued until the *O*s themselves were satisfied that they had experienced 10 representative images of the two classifications. Data of these 80 images are tabulated in Table III.

TABLE II

SHOWING THE RESULTS OF EXPERIMENT II

	M-image	S-image	I-image
No.	33	76	129
Av. movement	24.8	33.4	26.9
m.v.	18.5	20.5	17.1
	Clear	Mod. Clear	Dim
No.	161	61	16
Av. movement	26.0	33.3	38.1
m.v.	16.4	12.9	19.0
	Stable	Mod. Stable	Fluct.
No.	170	45	23
Av. movement	26.1	34.6	36.2
m.v.	16.0	13.9	21.0

Turning first to attributes of image-type, we note that, so far as a distinguishing trend is perceptible in Experiment IV, it is that memory images are not more "filmy" or "scrappy" than images of imagination, a result which agrees with those of Martin, Ogden, and Clark. Moreover, the first three experiments, involving more than 500 images, show a scatter of unclear imagery over all classes.

When amount of eye-movement is examined, however, it is found that the unclear imagery, of whatever class, involved the most movement. When an *O* is confronted with a dim image which he is expected to describe later, it is reasonable to suppose that his attentive effort is greater than when a clear and brilliant bit of imagery presents itself to him. In such a situation we find that his eye-movements are generally increased, a result in consonance with Clark's conclusions and tending to support Washburn's contention that "the essence of attention is movement."[10] This is not to say that all ocular movement during imagery is to be attributed to the effort of secondary attention, but only that this effort seems to be the most important factor under these experimental conditions. The rather small range within which the individual coefficients of correlation between clearness and movement fall suggests that attention (adjusted to degree of clearness) plays a definitely circumscribed part and that other factors, perhaps physiological ones, take rôles as definitely proportioned.

TABLE III

Showing the Results of Experiment IV

		Memory (40 cases)		Imagination (40 cases)
Clearness	Greater	20		11
	Equal		7	
	Uncertain		2	
Stability	Greater	19		20
	Equal		1	
	Uncertain		0	
Detailedness	Greater	21		12
	Equal		1	
	Uncertain		6	
Completeness	Greater	13		8
	Equal		12	
	Uncertain		7	
Duration	Greater	19		14
	Equal		4	
	Uncertain		3	

CONCLUSIONS

(1) There is no characteristic relation between type of visual image and amount of accompanying eye-movement.

(2) Unclear visual images are usually accompanied by a greater amount of eye-movement than are clear images.

(3) The amount of eye-movement tends to increase when conditions are such as to demand greater attentive effort.

(4) Characteristics such as clearness, stability, detailedness, completeness and duration cannot be assigned with much assurance in greater degree to one type of image than to another.

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[10] M. F. Washburn, *Movement and Mental Imagery,* 1916, 35.

THE ADAPTATION OF CUTANEOUS PAIN

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Despite reports of the adaptation of pain published by Von Frey,[1] Murray,[2] Goldscheider,[3] and Straus and Uhlmann,[4] we are told that "the pain sense does not appear to show the phenomenon of adaptation,"[5] or, as is true of most books, the topic is altogether ignored. This may mean that the authors of these books were unconvinced by the experimental evidence, or that they were unfamiliar with it. In any case, the need for further work is apparent. Since the question is of systematic importance, and since none of the studies, with the exception of Straus and Uhlmann's, was primarily concerned with pain adaptation, the purpose of the present study is to consider whether the phenomenon does occur; and if so, what are the sufficient and necessary conditions for its appearance.

Method and procedure. An area of the skin from the middle of the dorsal surface of the left forearm was chosen as the experimental tissue.[6] This area, extending longitudinally, was 3 x 8 cm. in extent, and 10 cm. from the ulnar prominens. It was shaved, so as to avoid the distracting experiences aroused by stimulation of the hair; and, to soften the skin, it was scrubbed, at the beginning of every experimental period, with soap and water and a hand brush.

After the tissue had thus been prepared, *O* seated himself in a comfortable chair at a table, and placed his hand and forearm in a plaster-of-paris arm-rest that had been especially cast for him. His arm, held firmly in a fixed position during the periods of stimulation, was therefore relatively free from accidental movements and tremors. When *O* had placed his arm in the cast, *E* adjusted the stimu-

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[1] M. von Frey, Beiträge zur Physiologie der Schmerzsinnes, *Ber. ü. d. Verhandl. d. königl. sächs. Ges. d. Wiss. z. Leipzig, math.-phys. Kl.*, 46, 1894, 193; *Untersuchungen über die Sinnesfunktionen der menschlichen Haut*, 1896, 242.

[2] E. Murray, A qualitative analysis of tickling: Its relation to cutaneous and organic sensation, this JOURNAL, 19, 1908, 304 f.

[3] A. Goldscheider, Über Schmerz und Schmerzbehandlung, *Zsch. f. physik. u. diät. Therap.*, 19, 1915, 289-309; *Das Schmerzproblem*, 1920, 20, 71.

[4] H. H. Straus and F. R. Uhlmann, Adaptation of superficial pain, this JOURNAL, 30, 1919, 422-424.

[5] E. B. Titchener, *A Text-Book of Psychology*, 1909, 154.

[6] Straus and Uhlmann used the ventral surface. We believe, however, that the dorsal surface is to be preferred. For not only is it more homogeneous than the ventral, showing fewer arteries, veins, and tendons; but work upon it is less fatiguing to the *Os*, as the arm can be held more easily and steadily with dorsal side up.

lator—a modified algometer that could be applied mechanically at a constant rate and with unvarying intensity[7]—so that the stimulus-point was approximately 1 mm. above the surface of the skin. At a prearranged signal, *E* then brought the point to bear upon the skin by lowering the entire apparatus with the rack-and-pinion gear of the universal support. The stimulus was, therefore, applied at once with its full intensity. Three intensities were used: 2.5, 5.0, and 7.5 grm.[8] Every stimulus-intensity was applied in the main experiments 20 times in haphazard order to every *O.*

Three *O*s, *A, B,* and *C,* all graduate students and members of the class in advanced experimental psychology served in the experiments. Though none of them had previously observed in cutaneous experiments, all were familiar with the problem of pain adaptation. No attempt was, therefore, made to conceal from them the object of the present study. On the contrary, advantage was taken of their knowledge, as the following instructions, given at the beginning of every experimental period, clearly show.

> At a "ready, now" signal you will be stimulated with a sharp point. Report immediately the experiences aroused, and then describe the temporal course of your experiences. Give particular emphasis to the report of the appearance and disappearance of pain.
>
> Relax and remain as motionless as possible during the course of the experiment. Close your eyes at the "ready, now" signal and do not open them until told to do so.

O's oral reports were recorded by *E* together with the time at which they were made, as indicated by a stop watch. *O* kept his eyes closed from the starting signal to 20 sec. after his last positive report, when the experiment was discontinued. Between experiments, *O* was permitted a short rest, during which he was free to remove his arm from the cast and to walk about the room.[9]

A short preliminary series of experiments familiarized the *O*s, as well as the *E,* the junior author, with the routine of the work and the nature of their tasks. During this and the main series, the apparatus was concealed from the *O* by a cardboard screen. In the main series, 10 experiments were performed during every experimental period, which lasted from 1½–2 hr. An experiment was not started until the after-effects of a previous experiment had worn off.

The pain spots stimulated were not localized in advance. Their stimulation was left to chance, the needle being applied haphazardly to different points of the experimental area. If pain was aroused, the experiment was allowed to run its course; if *O* reported merely pressure, the apparatus was shifted to another spot. Such instances, however, were not numerous, because of the care taken in preparing the tissue and of the large number of pain spots in the experimental area.

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[7] A hollow brass cylinder, approximately 15 cm. long, formed the body of the apparatus. Through this, in holes which had been carefully centered, projected a thin rod (a steel knitting needle). The stimulus-point (a No. 10 sewing needle) was attached to one end of the rod, and a weight-holder to the other. When the apparatus was held perpendicularly, the rod could be moved freely up and down through the cylinder with practically no side-play.

[8] The values, obtained by means of a pressure-balance, include the slight friction of the instrument, and the weight of the stimulus-point, connecting rod, weight-holder, and weights.

[9] *O* was not required to use a blindfold or to retain his arm in the rest between experiments because we wanted him to be keenly alive, for the duration of the experiments, to his experiences, and not become drowsy and sleepy—conditions which would of themselves make for adaptation.

Results. (1) Adaptation. We obtained reports of the adaptation of pain in every one of the 60 experiments with every *O*. The time of adaptation, however, varied considerably: from 6–780 sec. for *A*, from 4–160 sec. for *B*, and from 5–110 sec. for *C*. The average adaptation-times, given in Table I, also show large individual differences and wide fluctuations. The m.v. of the averages vary, for example, for *A* from 43–71%, for *B* from 55–69%, and for *C* from 46–59%. These variations are so large and the ranges of dispersion are so great that little, if any, significance may be attached to the fact that *A*'s and *B*'s averages increase with intensity of stimulus. Though the results of these two *O*s vary in the same direction they differ greatly in amount: *A*'s averages vary from 43.1 sec. for the weakest intensity to 176.9 sec. for the strongest; *B*'s from 18.9 to 42.1 sec. *C's* average adaptation-time, moreover, varies in the opposite direction. It is the shortest (32.8 sec.) for intermediate intensities, and largest (41.2 sec.) for weakest intensities.

TABLE I

Showing, for Every *O* and for Every Weight of Stimulus, the Average Time of Adaptation in Seconds, the M.V. from the Average, and the Range of the Dispersion

(60 experiments for every *O*)

O	Stimulus weight in grm.	Adaptation time in sec.			m.v./av.
		Av.	m.v.	Range	
A	2.5	43.1	18.6	12–75	43%
	5.0	99.1	70.1	6–302	71%
	7.5	176.9	111.2	35–780	69%
B	2.5	18.9	13.6	4–60	69%
	5.0	36.5	22.5	5–105	62%
	7.5	42.1	23.0	6–160	55%
C	2.5	41.2	21.3	12–105	52%
	5.0	32.8	19.4	5–90	59%
	7.5	40.7	18.9	10–110	46%
All	2.5	34.7	19.8	4–105	57%
	5.0	56.1	41.7	5–302	74%
	7.5	86.6	70.5	6–780	81%

Our results give no indication, therefore, that the phenomenon of cutaneous pain adaptation is conditioned, to any great extent at least, by the intensity of the stimulus. We have no explanation to offer for the fluctuations obtained. The adaptation-time seems to be entirely a matter of the spot stimulated. It cannot be correlated with fatique or ennui, for the fluctuations were as marked during the first as during the last part of an experimental period, and during the early periods as during the later.

Straus and Uhlmann suggest that their long adaptation-times may be due to the fact that "for all our precaution, some slight movement of the observer's arm may have taken place,"[10] the inference being that if movement had taken place the pain experience would be prolonged or revived. They are led to this suggestion by their belief that movement inhibits adaptation. They state that "it is essential to success [*i.e.* adaptation] that there be no movement either of the stimulus-point or of the skin beneath the stimulus, and also that the intensity of the stimulus remain constant during the period of stimulation."[11] In order to test this suggestion we varied our procedure slightly during the last two sittings (*i.e.* 20 experiments) with every *O*. Five seconds after *O* reported complete adaptation, we instructed him to wriggle his hand and fingers, or to move his forearm slightly, or to make both movements. After all our precautions to avoid movement we were greatly surprised to discover how stable adaptation was once it had been reached, and how readily it could be obtained under conditions of movement. In 56 of these 60 experiments, *i.e.* in about 93%, pain did not return; and in the 4 instances in which it did reappear it lasted only for a brief interval and adapted again in spite of the fact that the movements were continued. Straus and Uhlmann's tentative explanation of the long adaptation-time is, therefore, untenable; and so also is their contention that motionlessness is the *sina qua non* of pain adaptation. We are, in the light of these results, led to the conclusion that movement in and of itself does not inhibit adaptation.[12] All that is required for the adaptation of cutaneous pain is a stimulus of unvarying intensity. Movement is effective, we believe, only when and insofar as it makes for an increase of intensity.

(2) The temporal course. The temporal course of the phenomenon of pain adaptation varied among the different *O*s, and at different times and spots for the same *O*. The course of adapation appears to be conditioned by the spot stimulated, and not by any of the objective conditions, such as stimulus-intensity, over which we had control. Sometimes pain appeared as soon as the needle-point was brought into contact with the skin, at other

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[10] *Op. cit.*, 424.

[11] *Ibid.*, 423.

[12] Once this has been pointed out, it seems obvious; for instances are frequent in every day life. Hot water, for example, which is at first painful to the hands, becomes indifferent with continued use. The pain does not reappear when the hands are moved.

times there was a short delay. The average time of report was 2.2 sec. for *A,* 1.5 sec. for *B,* and 1.9 sec. for *C.* The *O*s were, however, under no constraint to hasten their reports. They frequently delayed their reports in order to make certain of them; consequently the values just given cannot be regarded as indicative of the time of the appearance of the pain. These values merely mark the first step in our *O*s reports of the temporal course of their experiences.

(a) Normal type. All of our *O*s, in all of the experiments, reported the incursion of pressure experiences in the course of adaptation. The normal or most frequent course of adaptation was a gradual decline in the intensity of the pain, which was followed by the disappearance of pain and the emergence of pressure, and the decline and disappearance of pressure. The following is a composite of the reports of these typical cases: "(1) sharp intense pain, (2) sharp pain, (3) pain, (4) dull pain, (5) weak pain, (6) pressure, (7) weak pressure, (8) tickle, (9) nothing." At which point did the adaptation of pain occur? Between (5) and (6) or at (9)? Our answer will depend upon our point of view. If we fall into the stimulus-error and ask at which point is the pain *stimulus* adapted, our answer will obviously be at (9); but if we adhere to the qualitative point of view our answer will just as patently be between (5) and (6). The quality of pain is adapted when the quality disappears. As stated above, however, we continued our experiments until 20 sec. after *O*'s last positive report of an experience derived from our stimulus—*i.e.* 20 sec. after (9) had been reported—because we wished to bring our work into line with the earlier studies.

Von Frey[13] and Goldscheider[14] were both concerned with the temporal course of the phenomenon and they arrived at different conclusions. Von Frey found that cutaneous pain, under continued and weak stimulation, became weaker and weaker and finally disappeared. Goldscheider found, on the contrary, that pain passed over into a new "unterschmerzlichen Empfindung" before complete adaptation was reached. The difference between these results is more apparent than real. The answers depend upon the form of question asked. Our results corroborate Goldscheider, but they were obtained in answer to the question: What is the course of experience in the adaptation of painful stimuli? If our question had been: What is the course of experience in the adaptation of pain? we might have obtained results confirming Von Frey. Our experimental conditions were not set to answer that question. Our stimuli were applied at superliminal pressure values. They aroused pressure as well as pain experiences. That the pressures reported by our *O*s may have been nothing more or less than the residual effects of our own stimuli, is borne out by unchecked and uncontrolled observations. The adaptation of the hands, for instance, to the pain of hot water does not seem to pass through a stage of pressure. The only way, however, of deciding this question is to arouse pain with stimuli, for example radiant heat, that do not give rise simultaneously to pressure experiences.

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[13] *Op. cit.*

[14] *Op. cit.*

(b) Variant types. In addition to the normal course of adaptation, which starts with maximal pain and gradually subsides through pressure to indifference, two other types were reported. In one type, as the following report shows, maximal pain recurred once:

A. 2.5 grm.: Sharp pain (3″);[15] dull pain (8″); pressure (12″); sharp pain (15″); dull pain (25″); pressure (37″); adaptation (40″).

In the other, pain-maxima recurred twice:

B. 7.5 grm.; Sharp pain (1″); dull pain (13″); very sharp pain (16″); pain (25″); very sharp pain (35″); dull pain (40″); pressure (45″); nothing (50″).

These types were reported by all the *O*s, and as frequently in one part of the experiment as in another. We thought, when they first occurred, that they might be due to accidental movements of the stimulus-point, or of the tissue under the stimulus-point; but when we tested this possibility we found no confirmation of it:[16] pain recurred when there was no movement; and it did not invariably or even frequently recur when there was movement. Of the other possible explanations considered—changes in breathing and pulse showed nothing—volumetric change seems the most hopeful. We did not, however, have an opportunity of testing it. Indeed, we do not know, as a matter of fact, whether volumetric changes occurred during our experiments, but from the results of other studies it seems probable that they did occur.[17] If they did, the changes from a smaller to a larger volume may have, by an upward thrust against the stimulus-point, increased momentarily the intensity of the stimulus and thus the intensity of the experience. This explanation of the recurrence of pain-maxima is theoretically plausible; the test by experiment remains to be done.

(3) After-sensations. The removal of the stimulus was frequently followed by after-sensations which were so insistent that the *O*s reacted to them by rubbing the area and by volunteering descriptions. The qualities most frequently reported were itch and prick, but heat, warmth, cold, pain, and pressure were also mentioned. These after-sensations endured at times

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[15] This and the following values show the time in seconds that elapsed between the application of the stimulus and the report here recorded.

[16] The device used in recording these movements was constructed as follows: a small thread was fastened to the stimulus-point and the short arm of a writing-point. If the stimulus-point was moved, either by jarring or by a movement of the *O*'s arm, a record was made by the writing-point upon a kymograph.

[17] K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 408, 410.

for several minutes. The pain, including itch and prick, was usually patterned, radiating from the point stimulated up and down the radial side of the arm, and was usually localized nearer to the elbow than to the wrist. The intensity and insistence of the after-sensations seemed to be correlated with the intensity of stimulation: adaptation to the heaviest stimulus, *i.e.* to 7:5 grm., was most frequently followed by after-experiences.[18]

SUMMARY AND CONCLUSIONS

(1) Cutaneous pain, aroused by a No. 10 sewing needle at a pressure varying from 2.5–7.5 grm., is adaptable. The adaptation-times vary considerably, however, with the *O* and with the spot stimulated.

(2) Adaptation-time is not conditioned by intensity of stimulus, by fatigue or ennui, or by movement.

(3) Slight movements of the stimulus or of the tissues beneath the stimulus do not interfere with the phenomenon of adaptation. All that appears to be required in order to obtain the phenomenon is a stimulus of unvarying intensity. Movement seems to inhibit adaptation only insofar as it increases the intensity of the stimulus and makes for variability.

(4) The pain experiences aroused by punctiform stimuli are replaced by pressury experiences before the stimulus is completely adapted. Whether this is inherent in the adaptation of pain or is merely the residual effect of the pressure-pain stimulus used in this experiment is a problem for further work, that may be definitely answered by using painful stimuli which do not simultaneously arouse pressure.

(5) The normal course of adaptation, under the conditions of this experiment, is a gradual subsidence from a maximal pain through pressure to indifference. In addition to this type, two variants were obtained, which are characterized by the recurrence of intense pain. In one the maximum pain recurs once, in the other it recurs twice.

(6) The cause of the recurrence of the pain-maxima in the variant types of adaptation is not known. It does not appear, however, to be due to accidental movements of the stimulus-point or of the tissues beneath the point, nor to changes in breathing or pulse. Volumetric changes offer a plausible explanation, but one that requires the test of experiment.

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[18] Since the acceptance of this paper, E. F. Wells and L. B. Hoisington (Pain adaptation: A contribution to the Von Frey-Goldscheider controversy, *J. Gen. Psychol.,* 5, 1931, 352-367) have published a phenomenological study of pain adaptation. Their results and ours stand in close agreement. In addition, however, to the normal and variant types of adaptation described above, they obtained a nonpressury type in which "pain simply gets weaker, less dense, and fades out of experience" (361).

THE OCCUPATIONAL BACKGROUND OF FEEBLE-MINDEDNESS

By DONALD G. PATERSON and EDWARD A. RUNDQUIST, University of Minnesota

Taussig's suggestion of an intellectual hierarchy of occupations has been verified by the use of objective mental tests during the World War and subsequently by many investigators.[1] This knowledge now permits definite description of the social-economic composition of groups of children (through classification of parental occupations) or adults selected for any particular psychological study. In the absence of such methods some writers have misinterpreted the intellectual significance of the occupational information contained in their studies. A notable example is Pearl's analysis of the occupational background of eminent men.[2]

Terman[3] and Cox[4] using such occupational classification methods in their studies of genius have shown that approximately four-fifths of the fathers of gifted children and a similar proportion of the fathers of eminent men have been furnished by the upper social classes in society. These data confirm in a more systematic manner the earlier work of Galton, Ellis, Cattell, DeCandolle, Odin, Clarke, and others.

In view of the significance of the findings resulting from the use of these methods in studies of genius it is surprising to find little or no data con-

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[1] F. W. Taussig, *Principles of Economics. II,* 1912, 134-138; Psychological examining in the United States Army, *Memoirs Nat. Acad. Sci.,* 1921, XV, Pt. III, Ch. 15, Intelligence Ratings of Occupational Groups, 819-837; D. Fryer, Occupational intelligence standards, *School & Soc.,* 16; 1922, 273; M. E. Haggerty and H. B. Nash, Mental capacity of children and parental occupation, *J. Educ. Psychol.,* 15, 1924, 559-572; W. D. Scott and R. C. Clothier, *Personnel Management,* 1923, 643; F. L. Goodenough, The relation of the intelligence of pre-school children to the occupation of their fathers, this JOURNAL, 40, 1928, 284-294.

[2] R. Pearl, The biology of superiority, *Am. Mercury,* Nov. 1927, 257-266; The present status of eugenics, *Sociological Press,* 1928, 1-20. For reply, see Donald G. Paterson and Edmund G. Williamson, "Raymond Pearl on the doctrine of 'Like Produces Like,'" *Am. Nat.,* 63, 1929, (May-June), 265-273.

[3] L. M. Terman, *Genetic Studies of Genius,* I, 1925, 64.

[4] C. Cox, *Genetic Studies of Genius, II,* 1926, 37.

cerning the social-economic composition of the feeble-minded groups in society. One familiar with the data showing a positive correlation between social-economic status and intelligence for random samplings of the population could infer that the facts in regard to the feeble-minded would be the antithesis of those found to be true for the mentally gifted. The present study was designed to accumulate and classify objective data whereby the validity of such an inference might be thoroughly tested.

Not only will information regarding the occupational classification of the parents of the feeble-minded add to our knowledge of the mentally deficient but also such data will permit proper account to be taken of social status whenever the feeble-minded are compared with samplings from the general population in regard to head-size, height, weight, dentition, or other physical traits. One of the writers[5] experienced great difficulty in evaluating the significance of such comparative studies because of the absence of definite information concerning the social status of the feeble-minded. In the publication referred to, considerable attention was devoted to the complications introduced into the consideration of the problem of the relation between physical traits and intelligence by the factor of social status.

Source of data. The present study consists of an analysis of the occupational status of the parents of 823 admissions to the Minnesota School for Feeble-minded at Faribault. The study also involves a similar analysis for 516 applicants for admission ("waiting list") to the same institution.

Data for each case were taken from the official records and included the following items: sex, chronological age, mental age, I.Q., test employed, occupation of father, and weekly or monthly income of father. The records are gratifyingly complete due to the services of a full-time social worker who carefully investigates the family background of each case and also due to the practice of having the cases examined by qualified mental testers under the direction of Dr. Fred Kuhlmann.

The data were collected during the Spring of 1931 and may be regarded as representative of institutional experience in receiving inmates during the preceding five years. The depression had not been operative a sufficient length of time in this area to inject selective factors which would distort the institutional picture one might have gained at any time during the preceding decade.

In utilizing the mental test data it was decided to rely upon I.Q. as the most significant single index of mentality. However, in 229 cases no computed I.Q. was available because the subjects were so low mentally as to preclude definite I.Q. determination. In all such cases the notation on the records is "Idiot" and the writers assumed an I.Q. between 10 and 20. The writers also assumed the equivalence of ratings obtained from the Stanford-Binet (317 cases) and from the Kuhlmann-Binet (793 cases) for the low levels involved in this study.

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[5] D. G. Paterson, *Physique and Intellect,* 1930, 1-304.

Treatment of the occupational data. The Minnesota Rating Scale for Occupational Intelligence was employed in classifying the parental occupations.[6] This scale is a revision of the Barr-Taussig Scale.[7]

The Minnesota Scale differs from the Barr-Taussig Scale in the following particulars. Twenty industrial psychologists furnished estimates of the intellectual level typifying each occupation. These psychologists were guided by a standard classification of occupations into six major categories. The Minnesota Scale also listed many more specific occupations (243 as against 120). Furthermore, subdivisions of occupations requiring different levels of intellect are listed as separate occupations. For example, 20 retail dealers are specified according to products sold and are actually rated by the judges in four of the six major categories.

The standard classification of occupations into six major categories was derived from a careful study not only of Taussig's classification and Barr's extension of the Taussig Scale but also of the objective mental test results as determined by the Army Psychologists and as subsequently revised by Fryer.[8] The six major categories are defined as follows.

(I) High professional and major executives:

Requiring very superior abstract intelligence with training equivalent to a college graduate from a first class institution.

High standards, with ability for creative and directive work; such as, lawyer, college president, president of a large manufacturing concern, etc.

(II) Lower professional and business men:

Requiring superior abstract intelligence with training equilavent to 2 or 3 years of college or executive of moderately large business. Achievements less creative than in Group I, but also demanding executive and leadership ability.

Executive of a moderately large business, veterinary doctor, high school teacher, etc.

(III) Technical, clerical, supervisory occupations:

Requiring high average abstract intelligence with training equivalent to high school graduation.

Minor executives (foreman, department head) or highly technical work often involving dealing with abstract classifications and details; such as, R. R. clerks, some retail dealers, photographers, telegraphers, shop foremen, stenographers, etc.

(IV) Skilled tradesmen and lower grade clerical workers:

Requiring average abstract intelligence with equivalent of some training beyond the eighth grade.

Mechanical work demanding specialized skill and knowledge; tasks mostly of a complicated but concrete nature and requiring particular technical training; such as, auto mechanic, stationary engineer, file clerks, typists, etc.

(V) Semi-skilled occupations:

Requiring low average or slightly below average abstract intelligence, with training equivalent to seventh or eighth grade.

Work demanding a minimum of technical knowledge or skill but a maximum of special abilities, such as dexterity in the performance of repetitive and routine work; such as, delivery men, packer in factories (operate machines but do not understand principles and are unable to repair or set up the machine), and lowest grades of clerical work also, such as number sorters, etc.

(VI) Unskilled occupations:

Requiring inferior abstract intelligence only, with no formal training necessary.

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[6] E. S. Brussell, A Revision of the Barr-Taussig Scale of Occupational Intelligence, M. A. Thesis, University of Minnesota Library, 1930.

[7] See Terman, *op. cit.*, 66-72.

[8] *Op cit., School & Soc.*, 16, 1922, 273.

Routine manual work under supervision and requiring no skill or technical knowledge; such as, day laborers, R.R. section hands, etc.

In general, there was close agreement among the twenty industrial psychologists in assigning each occupation to one of the six major categories. The Minnesota ratings also agree closely with ratings of common occupations in the Barr-Taussig Scale (r = + 0.95). Furthermore, in using the Minnesota scale for rating occupations, Brussell found that two independent ratings of a list of occupations yielded a scoring reliability coefficient of + 0.98. Likewise, the writers independently rated the occupations involved in the present study and confirmed this result.

Results. Table I presents the main comparison of the occupational background of the two groups of the feeble-minded and of a random sample of adult males in Minneapolis.[9] It will be noted that only 12.6% of the fathers of 'admissions,' and only 9.3% of the fathers of 'applications' are classified as engaged in the upper three occupational levels, whereas 41.0% of the random sample come from these levels. On the other hand, 87.4% of the fathers of 'admissions,' and 90.7% of the fathers of 'applications' are drawn from the lower three occupational levels, whereas only 59.0% of the random sample come from these levels. In brief, it is clear that the fathers of feeble-minded persons committed to a state school come predominately from the skilled, semi-skilled, and unskilled labor groups.

TABLE I

CLASSIFICATION OF OCCUPATIONS OF FATHERS OF THE FEEBLE-MINDED AND OF A RANDOM SAMPLE OF MALE ADULTS IN MINNEAPOLIS, MINNESOTA

Occupational Category	Occ'ns of Admissions		Occ'ns of Applications		Occ'ns of Random Sample of Minneapolis	
	No.	%	No.	%	No.	%
(I) High Prof. and Major Executives	3	0.4	3	0.6	26	3.0
(II) Lower Prof. and Bus. Men	34	4.1	6	1.2	69	8.0
(III) Tech., Cler., and Minor Executives	67	8.1	39	7.5	260	30.0
(IV) Skilled trades and Lower Clerical	259	31.5	170	33.0	234	27.0
(V) Semi-skilled Occupations	140	17.0	106	20.5	234	27.0
(VI) Unskilled Occupations	320	38.9	192	37.2	43	5.0
Total	823	100.0	516	100.0	866	100.0

If one's attention be directed to the two lowest classes (semi-skilled and unskilled) it is apparent that over half (55.9% and 57.7%) of the fathers of these institutional cases are semi-skilled or unskilled whereas only one-third (32.0%) of the random sample are similarly lacking in occupational efficiency.

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[9] The random sample of adult males in Minneapolis was obtained by listing and rating the occupations of 866 adult males as given in the Minneapolis City Directory, 1930. In all cases where the occupational information was ambiguous or inadequate the employer was visited and more specific information was secured. The writers are indebted to Miss Brussell (*op. cit.*) for these data.

To check the possibility that the above comparisons are vitiated by the inclusion of farm owners, farm tenants, and farm laborers among the fathers of the institutional cases whereas such rural occupations would be lacking in the urban random sample, the writers tabulated the above rural occupations separately. When this was done no significant change in the percentage was apparent. The small percentage of the random sample in Class VI (unskilled) is to be explained by the fact that Minneapolis is a commercial and small industry area.

Table II shows the relationship between I.Q. groupings of the feeble-minded inmates (admissions) and the classification of the occupations of their fathers into the six occupational intelligence levels. Mere inspection of the table shows the presence of a negative relationship between occupational class and I.Q. Over 75% of the admissions with I.Q. of 61 or higher come from fathers who are semi-skilled or unskilled. To a lesser extent the same trend is true for those who have I.Q. between 41 and 60. At the other end of the I.Q.-scale the reverse is found. The 'idiots' (I.Q. from 1 to 20) and the 'imbeciles' (I.Q. 21 to 40) come from fathers who are more nearly like a random sample of the adult male population. As a matter of fact, only one-third (36.6%) of the fathers of 'idiots' and only two-fifths (42.0%) of the fathers of 'imbeciles' belong to the semi-skilled and unskilled laboring classes. When the data in Table II were tabulated separately for urban and rural occupations the trend was in no way modified. The writers believe that the tendency for fathers of 'idiots' to be more representative of the general population in terms of occupational efficiency would

TABLE II

Relation between I.Q. of Feeble-Minded Inmates (Admissions) and Occupational Classification of Their Fathers

I.Q. Groups

Occupational Category	1–20		21–40		41–60		61+		Total	
	No.	%	No.	%	No.	%	No.	%	No.	%
I	0	0.0	2	1.4	1	0.3	0	0.0	3	0.4
II	11	5.5	11	7.6	8	2.9	4	2.0	34	4.1
III	21	10.4	19	13.1	18	6.5	9	4.5	67	8.1
IV	96	47.5	52	35.9	75	27.0	36	18.2	259	31.5
V	39	19.3	31	21.3	50	18.0	20	10.1	140	17.0
VI	35	17.3	30	20.7	126	45.3	129	65.2	320	38.9
Total	202	100.0	145	100.0	278	100.0	198	100.0	823	100.0

be more clearly evident were certain selective factors not operative. We refer to the fact that the small number of private custodial homes for the feeble-minded generally handle low grade cases whose parents are wealthy enough to afford such care. The result is that parents from Class I are conspicuous by their absence from such institutional data as is presented here.

Table III presents the results for applicants for admission. As in Table II, the trend toward an inverse relationship between I.Q. and occupational level of father is unmistakable.

The negative relationship between I.Q. and occupational level disclosed in Tables II and III is due primarily to the fact that low grade feeble-mindedness is caused, for the most part, by accidental, pathological, non-hereditary factors which would be distributed more or less at random among the various classes of society whereas simple feeble-mindedness is transmitted by biological heredity. In the latter group of cases it is to be expected that they (high grade imbeciles and morons with I.Q. over 30 for the most part) would spring from mentally deficient or mentally dull parents who are unable to compete economically except in the semi-skilled and unskilled levels of work. Striking evidence with respect to the non-hereditary character of idiocy in contrast to the rôle of heredity in the higher grades of feeble-mindedness is contained in the significant work of Ordahl.[10]

TABLE III

RELATION BETWEEN I.Q. OF FEEBLE-MINDED APPLICANTS AND OCCUPATIONAL CLASSIFICATION OF THEIR FATHERS

I.Q. Groups

Occupational Category	1–20		21–40		41–60		61+		Total	
	No.	%	No.	%	No.	%	No.	%	No.	%
I..........	2	1.3	1	1.0	0	0.0	0	0.0	3	0.6
II..........	2	1.3	3	2.9	1	0.6	0	0.0	6	1.2
III..........	15	9.6	8	7.8	11	6.5	5	5.8	39	7.5
IV..........	62	39.7	42	40.8	46	27.0	20	23.0	170	33.0
V..........	33	21.2	24	23.3	28	16.5	21	24.1	106	20.5
VI..........	42	26.9	25	24.2	84	49.4	41	47.1	192	37.2
Total.....	156	100.0	103	100.0	170	100.0	87	100.0	516	100.0

SUMMARY

(1) Absence of quantitative data regarding the social-economic status of inmates of state institutions for the feeble-minded led to the present study.

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[10] George Ordahl, Heredity in feeble-mindedness, *Training School Bulletin,* March-April, 1919, 2-16. See also H. H. Goddard, *Feeble-mindedness: Its Causes and Consequences,* 1914, 1-599.

(2) The following items of information were secured for each of 823 admissions and 516 applicants for admission to the Minnesota School for Feeble-minded: sex, chronological age, mental age, I.Q., test employed, occupation of father, and weekly or monthly income of father.

(3) The Minnesota Revision of the Barr-Taussig Scale of Occupational Intelligence was used in classifying the occupations of the fathers of the feeble-minded admissions and applicants and in classifying the occupations of 866 adult males of Minneapolis used as a random sample for comparative purposes.

(4) The fathers of feeble-minded persons committed to a state school come almost solely from the skilled, semi-skilled, and unskilled labor groups. The three upper occupational classes contribute a relatively small number of institutional cases in comparison to their numerical strength in the general population.

(5) There is a negative relationship between I.Q. and occupational level among the feeble-minded. The occupations of fathers of 'idiots' tend to be representative of the general occupational distribution found in society whereas the occupations of fathers of the higher grades of the feeble-minded are largely confined to the semi-skilled and unskilled levels. These results suggest that pathological and accidental cases of feeble-mindedness occur on all levels of society whereas cases of simple feeble-mindedness due to biological heredity arise from parents whose low mentality confines them to the semi-skilled and unskilled levels of work.

THE CONSTANCY OF CERTAIN BEHAVIOR PATTERNS IN YOUNG CHILDREN

By ARTHUR T. JERSILD, Columbia University

This study deals with the predictive value of measurements of the conduct of young children. The subjects were kindergarten children who previously had been studied while enrolled in a nursery school.[1] A technique developed by Arrington for recording children's overt behavior, use of materials, talking, laughing, and contacts with other children was used.[2]

This technique involves simultaneous observation by two persons of the child's activity during his free play. Mimeographed record blanks and stop watches are used by the observers. One person records four types of behavior: use of material, inactive contact with material, physical activity not involving material, and no overt bodily activity. This behavior is recorded in terms of its duration in seconds. The other observer records talking, physical contacts with other children (initiated and received), laughing and crying in terms of their occurrence within 5-sec. intervals. Complete details as to the construction of the record blank and the method of recording are given in Arrington's monograph. Each observation lasts 5 min.; repeated 5-min. samplings of behavior, extending over a long period of time, are taken in the case of each child. The final score of each child is based upon twenty-four 5-min. observations.

The reliability of this technique as a whole, the reliability of the observers, and of the recording of behavior in terms of each of the various categories, the consistency of the data as compared with data in other contemporary studies, are thoroughly discussed by Arrington. From every approach, high reliability was shown.

In the present study this technique was applied once more to children who had been subjects in Arrington's investigation. The records were taken by Monema Greenough and Lena Cowles, both of whom took part in the original studies in which the reliability of the method and of the observers was determined.

As in the original study, twenty-four 5-min. records were taken of the behavior of each child. The observations were made one or two forenoons each week over a period of several months. The *S*s included 11 of the children who had been

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* Accepted for publication July 20, 1932. This study was done with the assistance of Monema Greenough and Lena Cowles.

[1] This study was conducted under the auspices of the Child Development Institute, Teachers College, Columbia University. The author is grateful to Dr. Lois Hayden Meek, Director of the Institute, for the facilities for conducting the study; for access to the kindergarten children he is grateful to Dr. Rollo G. Reynolds, Director, and Miss Alice Thorn, of the Horace Mann School, and to Dr. Jesse H. Newlon, Director, and Miss Virginia Carroll of the Lincoln School.

[2] Ruth E. Arrington, Interrelations in the behavior of young children, *Child Develop. Monog.*, 1932, (no. 8).

studied during the previous year. Four of these were enrolled at the kindergarten of the Lincoln School, 7 were enrolled in Horace Mann (both of these schools are associated with Teachers College). On a given forenoon, each *S* was observed 5 min. in turn and, if time would permit, a second series of 5-min. observations was made. The observations were rotated to provide, as far as possible, similar conditions as to time of day and day of week for each *S* in the study as a whole. The average age of the *S*s (on February 1, approximately the mid-point of the study) was 52.04 mo.; the range was from 46.7 to 60.7 mo.

As a supplementary project, observations were made of the *S*s independently of the formal observational technique, and records of case studies were obtained.

It can be seen that the environment in which the present records were taken differed from that of the original study in the nursery school. In the present study, as in the earlier one, the procedure was to take records only during the *S*'s free activities. But the kindergarten routine entails more direction usually than the procedure in a nursery school, even when no formal project is under way. Further, the *S*s when first studied were united in one group whereas in this investigation they were divided between two different groups. Every *S*, in other words, was observed in a new environment containing only a part of his previous group.

For the purpose of this study the difference between the previous and the present environment of the *S* is all to the good. The motive in studies of young children is not so much to uncover episodic features of their behavior as to discover underlying behavior tendencies. It is necessary, however, that the new environment should allow equal freedom to the two groups of children if the records of the two groups are to be combined. The procedure in the two kindergartens varied in one respect. In one school the children spent all of their time, whether at play or at work, indoors. In the other school, attended by 4 of the *S*s, it was necessary to take half of the records while the children were spending their time on the roof of the building. Steps which were taken to provide for this discrepancy are described in the treatment of the results.

Results. At the end of the study the data for each child consisted of twenty-four complete 5 min. records. These data were compiled to yield for each *S* a score under each of the following headings:

Average number of seconds per five-minute observation period spent in: (1) Use of material. (2) Contact with material (inactive). (3) Contact with material—total of (1) and (2). (4) Physical activity without material. (5) Physical activity—total of (1) and (4). (6) No overt activity (without contact with material). (7) No overt activity—total of (2) and (6).

Average number of 5-sec. intervals per 5-min. period during which there occurred: (8) Talking to persons. (9) Talking to self. (10) Talking—total of (8) and (9). (11) Physical contacts initiated by the *S*. (12) Physical contacts received by the *S*. (13) Laughing—crying, originally included in this group, very seldom occurred in the follow-up observations and was therefore excluded.

In addition to this compilation of the data for the group as a whole, the scores of the 7 *S*s who attended one of the kindergartens, and of the 4 in the other school were segregated for separate study.

In the case of the 4 *S*s only half of the observations were made indoors. The results of the 12 indoor records were separately compiled and, to provide comparable data, a separate compilation was made of the 12 odd-numbered records of the *Ss* who were observed indoors exclusively. One *S* was transferred from one kindergarten to the other after 12 records had been taken, so she appears only once in the following table.

Where so few *Ss* are involved the results lend themselves better to detailed study of individual cases than to statistical treatment. As a basis for discussion, however, correlations were obtained between the records in this study and the *S*'s records of the previous year. The coefficients, obtained by the rank difference method, follow in Table I.

TABLE I

Correlation between 1931 and 1932 Scores

Behavior	No. 5-min. observations		
	24 with 10 *Ss*	12 indoors, with 11 *Ss*	24 with 7 *Ss* belonging to 1932 group
(1) Use of material	.36	.30	−.07
(2) Contact with material (inactive)	.58	.39	.64
(3) Contact with material, (1) and (2)	.52	.51	.21
(4) Physical activity without material	.37	.34	.00
(5) No overt activity	.38	.47	−.11
(6) Total physical activity, (1) and (4)	.38	.23	.94
(7) Total no overt activity, (2) and (5)	.61	.35	.79
(8) Talking to persons	.19	.08	−.07
(9) Talking to self	.40	.45	.10
(10) Total talking	.28	.33	.32
(11) Physical contacts initiated	−.11	−.10	−.25
(12) Physical contacts received	.35	.75	.27
(13) Laughter	.11	.21	.05
Median	.37	.34	+.10

The coefficients above suggest that there is a small degree of conformity between the *Ss*' behavior of this year and last. Most of the coefficients are low, and, because of the small number of cases, unreliable, but they tend to the positive side.

The data were examined to find whether any uniform factor, such as a consistently high deviation on the part of only one or two *Ss*, was responsible for the low correlations. This examination left the results much as they were. In each correlation table there were *Ss* who deviated considerably from their rank of last year, but in the tables as a whole many different children were responsible for the high deviations.

The correlations here obtained correspond quite closely to findings reported by Arrington. Sixteen children were studied by means of her technique during two separate years while the children attended the same nursery school. The correlations between their scores in 1930 and in 1931 ranged from —0.22 to +0.62, with a median (crying excluded) of +0.28. Even under conditions as uniform as continued attendance at the same nursery school the behavior of children, as studied by this technique, shows little constancy.[3]

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[3] Nine of the present children had been studied by means of this technique during each of two preceding years. The correlations between their records of 1930 and their records in this study ranged from +0.55 to —0.86, with a median correlation of +0.005.

Observations of the *Ss*, independent of the technique, supplemented by case studies and teachers' reports, revealed items which help to account for the low correlations. In some instances the *Ss* deviated widely from their previous scores by reason, apparently, of a change of companions. In other instances the change in scores could be attributed to a change in interest in play material. Two *Ss*, for instance, received high scores in physical activity, and in use of material, by reason of constant play in the sand-box at the nursery school. Their movements in the kindergarten were more versatile and vigorous, but this change in the content of their activities was not reflected in the scores. The procedure of recording in terms of units of time spent in an activity gave a higher score to continuous sedentary manipulation of sand than to vigorous, but not so continuous, play with heavy objects and apparatus.

A comparison between the *Ss'* relative scores in the present study and their scores of the previous year suggests that the behavior studied by means of the observational technique is largely a function of the particular situation and the particular individuals surrounding the child. The fact that most correlations are positive, although low, indicates that there is some degree of constancy in the behavior which differentiates one child from another. But in view of the low degree of correspondence between the records of two years it appears that the records reflect mainly the episodic interests and adjustments of the child rather than basic symptoms of his personality.

Apart from the observational technique, many similarities and some differences could be discerned in the behavior of the *Ss* as compared with their behavior of the previous year. One *S* who was so aloof and unenterprising in the nursery school as to be quite a distinguished character appeared to be a changed creature a year later in the kindergarten. Where once he mumbled softly to himself he now conversed freely with others, where previously he played alone, carrying in his hand a little stick or button, he now was energetic in the use of equipment and in joining in the play of others. In contrast to his marked change, several *Ss* showed characteristics which were observable the previous year. One *S* who frequently played the part of a ring-leader in the nursery school continued to initiate group activities in the kindergarten. Other *Ss* who seldom took the lead in initiating projects or in influencing the activities of others in the nursery school continued to play a rather inconspicuous and subordinate rôle in their new environment. One *S* who was active and alert, but quite independent in her activities in the nursery school continued her solitary enterprises the following year in the kindergarten. Many features of this kind, which could, perhaps, be reduced to quantitative terms, indicate the need for a more discriminative device for the study of children's behavior.

As far as the quantitative results of the present study are concerned there seems to be little constancy in the behavior of a child, as compared with other children, from year to year. But the foregoing discussion suggests at least two limitations in the technique by means of which the quantitative results were obtained. First, we cannot conclude concerning a child's characteristics simply by observing him in a particular environment in the company of a particular group of associates. His adjustments are likely to be quite specific. To obtain an adequate picture of his personality, with a view to inquiring how constant his characteristics are likely to be from year to year, it seems that it is neccessary to observe him in a wide variety of situations. This point is raised by Goodenough in a study which made use, in part, of separated short samplings of children's spontaneous behavior.[4] She observed that a change in situation does not affect all children in a precisely similar manner, and that to obtain a truly valid measure of behavior observations must be taken "under such a diversity of circumstances as to constitute a representative sampling of the child's daily life."

Another limitation of the technique is apparent from the comments above. Simply to record that a child spends 5 sec. in contact with material, or that he has so many contacts with other children during a 5-min. period, leaves out many significant details as to what the child actually is doing. It is important to know something concerning the vigor of his actions (for example, is he merely fingering a small stick or is he handling large blocks and building a house?) and to know whether his contact with others is that of hitting, or joining in play, or the like. Until more is recorded concerning the content of his behavior, the record of his behavior will be quite threadbare. This question as to the validity of the measurements used in her technique is discussed by Arrington. From the observations made in the present study it would seem that unless more of the content of the child's behavior is taken into account the records will not have much significance. On the other hand, as soon as an effort is made to obtain a more adequate description of the child's behavior it is likely that the technique will be less reliable, as measured by agreement between observers. The present study tells more concerning the limitations of the technique that was used than concerning the general problem of the constancy of children's behavior patterns.

SUMMARY

A technique for recording children's overt activity, use of objects, contacts with other children, laughing and talking during free play was used in a study of 11 kindergarten children who had been studied the previous year in the nursery school. Twenty-four 5-min. observations, distributed over several weeks, were made of each child. These data were supplemented by independent observations and case studies.

The quantitative records showed a somewhat low degree of conformity to the relative position of the same children in their scores of the previous year. It appeared that the records obtained during free play in the nursery school gave a measure of adjustments to a particular environment and to particular persons rather than an indication of basic behavior tendencies in the children. Data obtained apart from the formal technique showed that one child had changed his conduct in a decided manner, while several others continued to show characteristics which distinguished them in the nursery school. The study shows that to obtain indications of a child's characteristics, as expressed in his conduct, it is necessary to record the content and scope, rather than simply the duration and frequency, of his activities in a large variety of situations.

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[4] F. L. Goodenough, Inter-relationships in the behavior of young children, *Child Develop.*, 1, 1930, 46.

CHANGE IN AFFECTIVITY WITH REPETITION

By E. M. VERVEER, H. BARRY, JR., and W. A. BOUSFIELD, Tufts College

The experimental evidence relating to the influence of repetition[*] on affective judgments is apparently conflicting. Certain experimenters have found an increase in pleasantness to occur as a result of repeated presentations. Included in this group is Meyer[1] who worked with melodies and harmonies. Martin,[2] when studying the effect of prolonged observation of a picture, found affective value to decrease. Troland[3] states that continued repetition of any piece of music renders it substantially non-affective. Other observers have noted that repetition of a stimulus might either raise or lower the degree of pleasantness (Fechner[4]).

Gilliland and Moore[5] found increased liking for classical music, but very little change with 'jazz.' Ortmann[6] mentions both the change from unpleasant to pleasant with increasing familiarity in certain instances, as well as diminished liking with increased experience in others. Downey[7] found that most of her musical pieces became more pleasant after five repetitions, while one became less pleasant. Washburn[8] mentions that repetition may either raise or lower the pleasantness of a selection. In apparent contrast with the foregoing are the shifts of pleasantness and unpleasantness occurring as a result of the operation of Beebe-Center's Law of Affective Equilibrium.[9]

The present study was initiated in an attempt to discover further data regarding the precise relationship between repetition and affectivity.[10]

PROCEDURE

It is apparent that consideration should be given to the various temporal intervals intervening between the presentations of stimulus patterns. The procedure adopted has the advantage of combining the method of a series of continuous repetitions at one sitting with the method which allows a relatively long intervening period of a number of days.

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[*] Accepted for publication November 1, 1931.

[1] Max Meyer, Experimental studies in the psychology of music, this JOURNAL, 14, 1903, 474-475.

[2] L. J. Martin, An experimental study of Fechner's principles of aesthetics, *Psychol. Rev.*, 13, 1906, 182-186.

[3] L. T. Troland, *Fundamentals of Human Motivation*, 1928, 285-296.

[4] G. T. Fechner, *Vorschule der Aesthetic*, II, 1876, 240-243.

[5] A. R. Gilliland and H. T. Moore, The immediate and long-time effects of classical and popular phonograph selections, *J. Appl. Psychol.*, 8, 1924, 309 ff.

[6] O. Ortmann, *The Effects of Music*, 1927, 44-66.

[7] J. E. Downey and G. E. Knapp, *The Effects of Music*, 1927, 227-229.

[8] M. F. Washburn *et al.*, *The Effects of Music*, 1927, 203.

[9] J. G. Beebe-Center, The law of affective equilibrium, this JOURNAL, 41, 1929, 64.

[10] The writers wish to acknowledge their indebtedness to Dr. Beebe-Center for suggestions and advice in the preparation of this report.

The experiment consisted in the playing of two 'jazz' selections[11] on a victrola to groups of not over 7 *Ss* at one time, all undergraduate students in psychology, who were placed so that there were no less than three intervening seats between them. The music was clearly audible to all. At the beginning of each series of observations, *E* read the following instruction: "I am going to play a few records for you. While each piece is being played, kindly keep your eyes closed and try to concentrate on the music as much as possible. At the close you will rate each piece on a +10 to —10 basis, according to the degree of pleasantness the piece has for you." At the conclusion of each selection, introspections were recorded and then the same selection (or a control selection) immediately followed. For the first group of *Ss*, selection No. 1 was played repeatedly, with selection No. 2 inserted after three or more repetitions as a control. After the control piece had been rated as to pleasantness, the cycle was repeated until the conclusion of the experimental period, which lasted over an hour. The experiment was continued with the same *Ss* and same procedure after an interval of one week.

For the second group, the procedure was similar, except that selection No. 2 was played repeatedly with selection No. 1 used as a control. Only one *S* had previously heard selection No. 1, while everyone knew selection No. 2. Both pieces were 'jazz' music and piece No. 1 was chosen for the amount of internal repetition which it contained.[12]

RESULTS

A statement of the results is given in Table I. The successive presentations of the selections are represented by the figures 1 to 8 for the first and second weeks respectively. The absolute judgments of affective value are represented as deviations from the first trials. Values for the controls are also given in terms of deviations from the ratings of the first controls (Table II). It is plain from Tables I and II that affective value tends first to increase to a maximum and then to decrease to a value far below the initial value. If these data are plotted, the curves evidently are inflected. These changes in direction or affective 'inflections' characterize the data for the two selections with their controls.

The data of Table I show clearly the change in affective value for the two continuous series, also the changes resulting from the intervening interval of one week. The values for the controls over the same periods are shown in Table II.

DISCUSSION

The experimental data here presented appear to confirm the following observations of Fechner: "With a continuous (or repeated) stimulus, the affective value increases up to a certain limit which can be designated as the ascending effect. . . . If the stimulus is continued or repeated past the point of optimum affectivity, its value is lessened, . . . unless there is an intervening time interval."[13] In other words, Fechner clearly distinguishes between the effects of continuous presentations and the effects of a relatively long intervening time interval. The results mentioned for Meyer,[14] Martin,[15] Downey,[16] and others seem reconcilable to Fechner's principle of the ascending effect. Since stimuli may be represented as patterns of varying degrees of complexity, Washburn's results,[17] which indicate that the optimal degree of pleasantness occurs more quickly with popular than with classical music, and in general more quickly with musical than nonmusical observers, may be similarly interpreted. Gilliland and Moore[18] corroborate Washburn with respect to the influence of repetitions on classical music and jazz; Schoen and Gatewood[19] also make similar observations. Ortmann[20] finds both short (0.1 sec.) and long (15 sec.) tones are less pleasant than those of intermediate length (3 sec.). He also states that chords which are at first decidedly unpleasant grow less unpleasant as they are heard repeatedly, and that in time musicians frequently change their feeling toward modern harmony from extreme unpleasantness to pleasantness.[21]

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[11] No. 1: *Lazy Levee Loungers,* Columbia, 14555-D (150660); No. 2: *What's the Use,* Brunswick, 4810.

[12] These results have been confirmed with a different set of records.

[13] Fechner, *op. cit.,* 240-243.

[14] Meyer, *op. cit.,* 474-475.

[15] Martin, *op. cit.,* 182-186.

[16] Downey and Knapp, *op. cit.,* 227-229.

TABLE I

Affective Value of Musical Selections with Repetition

(Average degree of pleasantness +10 to −10 as a basis)

Selection:	No. 1 (8 Ss)	No. 2 (8 Ss)	No. 1 Extremes (3 Ss)	All Ss
Av. Trial No. 1	−1.63	4.38	3.67	1.74
Trial No.		Change from Trial No. 1		
1	0	0	0	0
2	.75	.25	0	.42
3	−.13	−.25	−1.67	−.42
4	−.63	−1.13	−2.67	−1.16
Control 1				
Rest				
5	−.38	−.38	−6.33	−1.32
6	−.75	−2.13	−8.00	−2.48
7	−1.38	−3.63	−10.33	−3.74
8	−.38	−4.75	−12.00	−4.05
Control 2				
2nd week				
1	1.75	−.25	.33	.68
2	1.63	−.63	−.67	.32
3	.88	−1.75	−2.33	−.73
4	1.00	−2.00	−2.67	−.84
Control 3				
Rest				
5	.38	−1.75	−3.67	−1.16
6	−.13	−2.88	−2.67	−1.69
7	−.75	−4.50	−3.67	−2.79
8	−1.75	−6.75	−3.00	−4.05
Control 4				

From the foregoing remarks it may be noted that (1) both selections indicate a change in affective value for a small number of repetitions up to an affective peak; that (2) following the affective peak, pleasantness diminishes progressively with further repetition,[22] and that (3) there is a tendency for the pleasantness of a selection to increase after a period of rest.

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[17] Washburn, *et al., op. cit.,* 205.
[18] Gilliland and Moore, *op. cit.,* 309 ff.
[19] M. S. Schoen and E. L. Gatewood, *The Effects of Music,* 1927, 171, 179-181.
[20] Ortmann, *op. cit.,* 44.
[21] *Ibid.,* 44-66.
[22] The experimenter, who listened to the selections the largest number of times finally developed a headache whenever the selections were played.

It may also be seen that many of the experimental results which show a decrease in pleasantness due to repetition may be interpreted in terms of the second of the observations of Fechner quoted above. Mention may be made in this connection of the fall in pleasantness of familiar or relatively simple pieces mentioned by various experimenters, like Ortmann,[23] who observes that many musicians have seen the charm of their favorite pieces grow less with experience. It is possible that the apparent discrepancies noted by certain investigators may be due to a different time interval at which the affective peak is reached with varying complexity of stimulus pattern. There still remain to be reconciled the cases coming under Beebe-Center's Law of Affective Equilibrium,[24] supported by experiments performed by him, Herring,[25] and Pepper,[26] all showing a general tendency for both pleasant and unpleasant stimuli to become relatively neutral or indifferent. Apparently the principle implied in Beebe-Center's law of affective equilibrium and Harris' law of contrast (a principle subsumable under the law of affective equilibrium) may have superimposed upon it the principle of affective 'inflection' which is presented in this paper.

TABLE II

		CONTROLS		
Selection:	No. 2	No. 1	No. 2	All Ss
Av. Control No. 1	4.13	−2.75	4.00	1.21
Trial No.		Change from Control No. 1		
1	0	0	0	0
2	.38	− .25	−6.00	−.89
3	.88	− .88	2.00	.32
4	2.00	−1.63	.33	.21

If we wish to identify temporal repetition as a special case of increased intensity (with the addition of a temporal factor), the principle mentioned might be related to Troland's principle of the hedonic calculus,[27] for which the parts relevant to this discussion appear to have some experimental verification in the work of Ortmann[28] that shows that tones of moderate intensity are more pleasant than those which are extremely soft or extremely loud. Troland[29] and Engel[30] observe that various gustatory stimuli have optimum pleasantness when of moderate intensity. If the influence of increased intensity is similar to that of repetition, the principle of affective 'inflection' seems also to have been anticipated by Wundt[31] who stated that with increasing intensity of sensation, feeling tone becomes at first more pleasant until an optimal intensity of sensation is reached, after which the feeling tone becomes progressively more unpleasant. His curve, which was systematized from casual observation (since he says feeling cannot be measured precisely), resembles those which may be plotted from our data.

In support of the general thesis of this paper, attention may be called to the following well-known phenomena: (1) the liking of children for the same familiar story, always in the same identical words; (2) the extreme unpleasantness of a once 'popular' tune after too extensive repetition; (3) the usual rise and decay of various fashions and styles; (4) the progress of musical preferences in the race and the individual from the simple to the more complex; (5) certain instances in which the economic law of diminishing returns are based on a purely psychological foundation.

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[23] Ortmann, *op. cit.*, 66.

[24] Beebe-Center, *op. cit.*, 64.

[25] J. P. Herring, The measurement of liking and disliking, *J. Educ. Psychol.*, 21, 1930, 159.

[26] S. C. Pepper, Changes of appreciation for color combinations, *Psychol. Rev.*, 26, 1919, 389-396.

[27] Troland, *op. cit.*, 308-314.

[28] Ortmann, *op. cit.*, 44.

[29] Troland, *op. cit.*, 308-314.

[30] R. Engel, *Psychology*, 1928, 1-36.

[31] W. Wundt, *Grundzüge der physiologischen Psychologie*, 5th ed. II, 1902, 311-312.

CONCLUSIONS

(1) Fechner's observations on affective change with continuous repetition are confirmed.

(2) With closely spaced repetition, pleasantness tends to increase to an affective peak at an optimal degree of familiarity.

(3) Further repetition tends to make the experience progressively less pleasant.

(4) An intervening time interval, however, tends to enhance the pleasantness of subsequent repetitions.

(5) Apparently discrepant results of different investigators may be harmonized, at least in part, in terms of the contrary effects of continuous repetition and repetition at intervals. It has been demonstrated that Fechner's law of repetition applies—as was stated by Fechner—to cases involving continuous repetition. It is likely that Beebe-Center's law of affective equilibrium, or the habituation effect described by Herring, apply to cases involving repetition at intervals.

APPARATUS

A DEMONSTRATIONAL TEST OF VISION

By M. Luckiesh and Frank K. Moss, Nela Park, Cleveland, Ohio

It is apparent to those engaged in research in the field of physiological optics that human vision does not benefit as it very well might from the knowledge thus gained. Due to the vagaries of human behavior, it is exceedingly difficult, *e.g.* to demonstrate the beneficence of more light or better lighting in a practical and convincing manner.

After a number of years of continuous experimentation, the authors have been able to present for the first time a simple and reliable method for demonstrating the influence of illumination-intensity upon visual efficiency.[1] Although the test described here was designed primarily to serve as a practical demonstration of the beneficence of better lighting, it has also proved to be an excellent research device, comparable in laboratory usefulness to far more complicated apparatus and experimental technique. It is applicable to, and has been successfully used in, a number of psychological researches.

A fundamental requisite of such a test demands that it respond significantly to relatively small changes in illumination-intensity in comparison with the enormous range of intensities under which the visual sense may function. Furthermore, it must be responsive to changes in illumination-intensity which are within the range actually encountered in lighting practice. Finally, any test-method which will be useful as a demonstration must be simple in operation and must yield positive results in all or nearly all cases in which it is tried.

In developing our instrument the experience gained in many behavioristic experiments was invaluable in providing certain limitations and suggesting certain methods of approach.[2] This experience indicated (1) that the instrument must be of the integrating rather than of the indicating type since a single observation on human behavior has little or no reliability as a quantitative datum. It was also recognized (2) that the test must operate at or near the differential threshold of visual ability in order to isolate the effects of small changes in seeing conditions by a test of short duration. A third fundamental requirement (3) necessitates some means for maintaining the effort expended under test as nearly constant as possible for various changes in the experimental variable. Without this control, the effect to be determined is often obscured, principally on account of the facility of the subjects in adapting themselves to widely different conditions, even at the expense of greater strain, effort, and fatigue. The fulfillment of this last fundamental requirement was the most difficult.

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[1] From the Lighting Research Laboratory, General Electric Company.

[2] See the volume *Seeing; A Partnership of Lighting and Vision,* 1931, by the present writers, which contains the complete demonstrational test herein briefly described.

Description. The total time required to perform a series of exacting visual tasks was selected as the criterion of visual efficiency. The individual test-objects required the recognition of a capital letter formed by small breaks in parallel diagonal lines printed in dark gray upon a lighter gray background, as illustrated in Fig. 1. (In the illustration the pattern has been reproduced on a white instead of a gray background in order to show the test-object more clearly.)

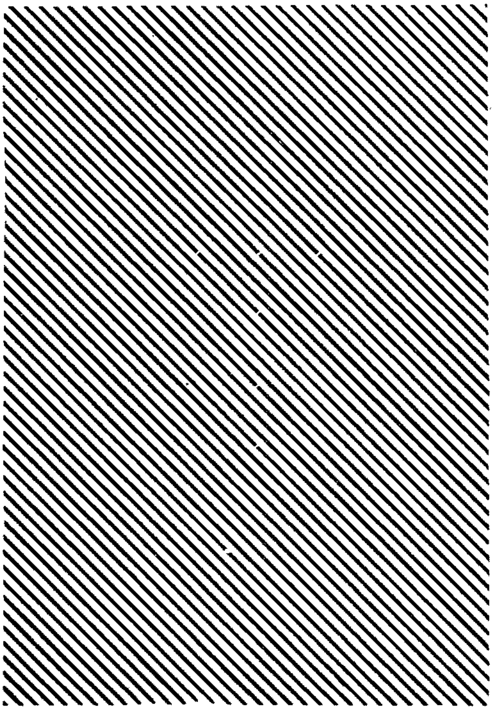

FIG. 1. THE TEST-OBJECT

The test-object is formed by breaks in the parallel-diagonal lines. On this pattern is the letter "T." For this illustration the visibility of the test-object has been increased by the use of a white instead of a gray background of 16.3% reflection-factor as used in the research form of the test.

The problem of producing a test-object of approximate liminal difficulty was accomplished by a combination of two methods. The first step consisted of making the breaks in the parallel diagonal lines fairly small. These breaks are approximately 0.3 mm. in width and subtend an angle of nearly 3 min. at the normal reading distance of 14 in. Due to the wide differences in visual acuity among various subjects, it was not considered advisable to approach nearer the actual visual threshold by a further reduction in the size of the small breaks which formed the letter to be recognized.

The actual threshold of visibility was approached by reducing the contrast between the parallel diagonal lines and the background. The reflection-factors of the lines and background are 10.4% and 16.3%, respectively. Therefore, the contrast between lines and background is 36%, as computed by the formula: Contrast % = 100[(B.B. — O.B.)/B.B.], in which "B.B." is the background brightness, and "O.B." is the object brightness. The visual task thus presented to the subjects was of sufficient difficulty to produce significant changes in the time required to recognize a series of 52 such test-objects for relatively small changes in illumination.

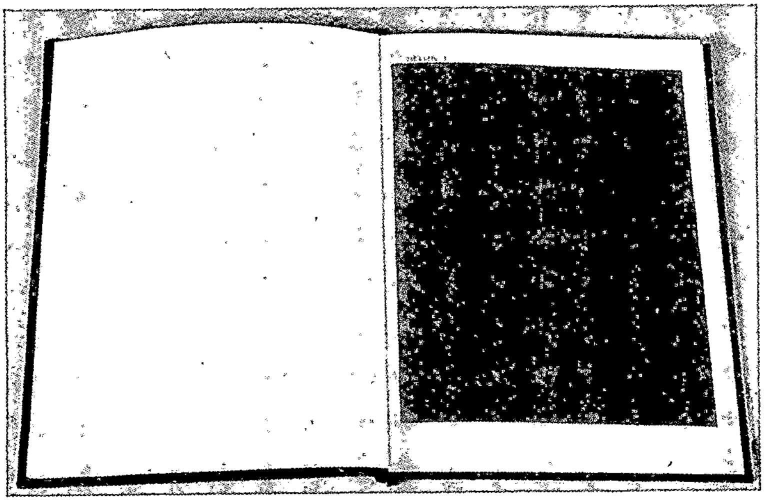

FIG. 2. TEST-BOOK OPENED AT FIRST PLATE
One of the 17 plates composing the series of test-objects. Each plate is composed of 4 patterns similar to that shown in Fig. 1.

The purpose of the pattern of parallel diagonal lines is not to present a maze in which the test-object (the capital letter) is hidden. The letter has purposely been made large and as simple as possible while the factors which tend to make its recognition difficult (excepting the visual factor) have been minimized. The uniformity of the parallel lines makes it quite difficult for the eyes to remain fixated on a definite part of the pattern. As a result, normal eye-movements are greatly accelerated. This fact will be readily appreciated if Fig. 1 is viewed steadily for a few moments. The significant fact is that these eye movements are performed involuntarily and, since the subject has so little control over the amount of visual effort expended, changes in the experimental variable produce corresponding changes in the rate of working.

It will also be noted from viewing Fig. 1 steadily that definite color-effects are produced. It seems reasonable to expect that these color-effects are produced as a result of the rapid eye-movements over the stationary black and white pattern.

This effect may be analogous to the color-effects produced by a rotating Benham disk.

Performance. The task is to recognize the capital letter in the center of each pattern, of which there are 4 on each page (see Fig. 2) and to name it aloud to insure careful performance of the test. The first series of patterns begins on a page marked "Begin 1" and is concluded on the page marked "Stop 1." The series includes 52 patterns containing 13 different letters between these designations. The time required to perform this series is recorded. The experimental variable is then changed and the experiment repeated, beginning the second series on the page marked "Begin 2" and concluding it on the page marked "Stop 2." A third test should be performed under the same conditions as the first and the time required to complete the first and third tests averaged and compared with that of the second series.

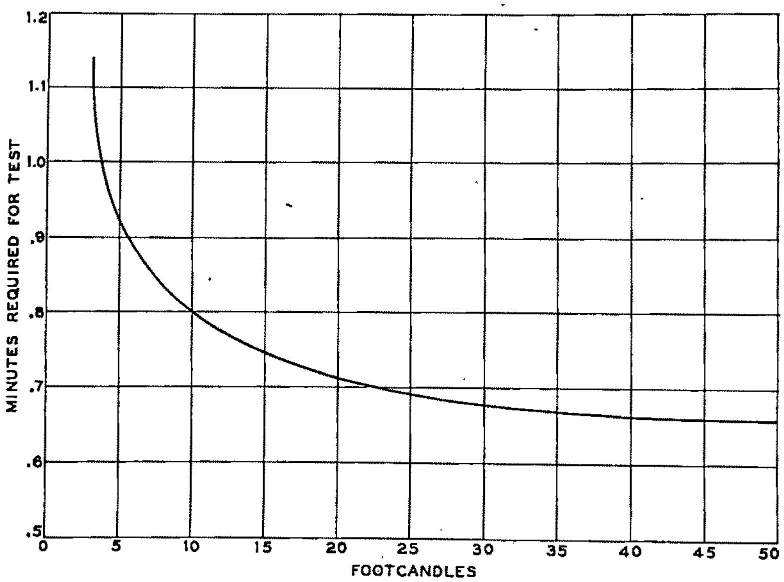

FIG. 3. TIME REQUIRED TO PERFORM THE TEST CORRELATED WITH INTENSITY OF ILLUMINATION

The average results of a large number of tests in which illumination-intensity was the experimental variable are plotted in Fig. 3. It will be noted that the test is satisfactory for the range in level of illumination for which it was designed.

The influence on human behavior of other experimental variables may be readily determined by using this test-method as a criterion. For example, the test was conducted repeatedly under two different conditions: (1) in a quiet room and (2) in a room in which several small motor-generator sets were operating. The visual work was done in a booth and at a level of illumination of 5 foot candles in both situations. Hence the distracting noise of the machines is the only known variable in the experiment. The results follow:

Quiet room	0.77 min. required per test
Noisy room	0.82 min. required per test

These results indicate that the distracting noise of the machines increased the average time required for the test by about 6%, a difference more than 6 times the probable error of the measurements.[3]

SPECIAL APPARATUS IN THE LABORATORY OF EDUCATIONAL PSYCHOLOGY, UNIVERSITY OF TEXAS

By B. F. HOLLAND, University of Texas

EYE-MOVEMENT CAMERA

The eye-movement camera was built by C. T. Gray in 1915. Its essential features are described in his *Deficiencies in Reading* (179-182). In recent years the following improvements have been made by the writer, the essential features of the camera being left intact.

(1) A set of photographic lenses of 12-in. focal length was added to replace a set of lenses of 8-in. focal length. These lenses are known as the Wollensak Velostigmat, Series IV, f. 6.3 Anastigmat. They function best when the eye is about 14 in. from the front of the lenses. The particular advantage of these lenses over those of shorter focal length is that they make possible the use of longer lines and longer selections of materials.

(2) A metal head-rest, attached to the camera table by means of clamps, gives greater rigidity than Gray's wooden piece.

(3) The arc-light used by Gray was replaced with a Bausch and Lomb illuminating unit equipped with a ribbon filament lamp of the 6-volt, 108-watt type, and a spherical condenser with rack-and-pinion focusing mount. The light produced by this unit is satisfactory for photographic purposes. After the unit is properly adjusted, it requires little or no further attention.

(4) A lighting device was designed to illuminate the reading material. This mechanism includes a regular automobile dash-light unit and a sheet-metal projector. The projector is a rectangular tapering inclosure attached to the lighting unit, being soldered to a metal cap which covers the dash-light bulb. Its dimensions at the source of light are ½ in. × ⅜ in. and at the large end from which the light emerges 2 in. × 3½ in. The total length is 7 in. The entire unit is attached to the upper cross-bar of the head-rest by means of a clamp. It can be adjusted to illuminate a small or a large amount of reading material. The lamp receives current from the city 110-volt line through a transformer.

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[3] The printing of this visual test was in itself a research-problem requiring photometric measurements and necessary alterations in the reflection-factors of the inks at each of the several stages in printing. For this reason it is obviously impossible to reproduce the test-pages in this publication. Copies of the test to be used for research purposes will be supplied at a nominal price by Lighting Research Laboratory, General Electric Company, Nela Park, Cleveland.

(5) A film mechanism (see Fig. 1) constructed to carry standard moving-picture film replaced one suitable for kodak film only. The mechanism was designed by the writer and built by W. L. Benson, University mechanician in Chemistry. Four film-boxes are arranged in opposite pairs, the boxes at the top and left holding a supply of unused film, the other two receiving film as it is exposed. The film is pulled from the supply-box by means of a sprocket which operates in the inclosure just above the lower film-box. This sprocket is operated on a shaft shown just under the left film-box, the shaft being turned by a pulley belted to a speed-reducing unit which is operated by an electric motor. Another pulley on this shaft is connected to a pulley which operates a core in the film-box on which the film is wound as it comes from the supply-box. These pulleys are articulated by means of a steel-spring belt.

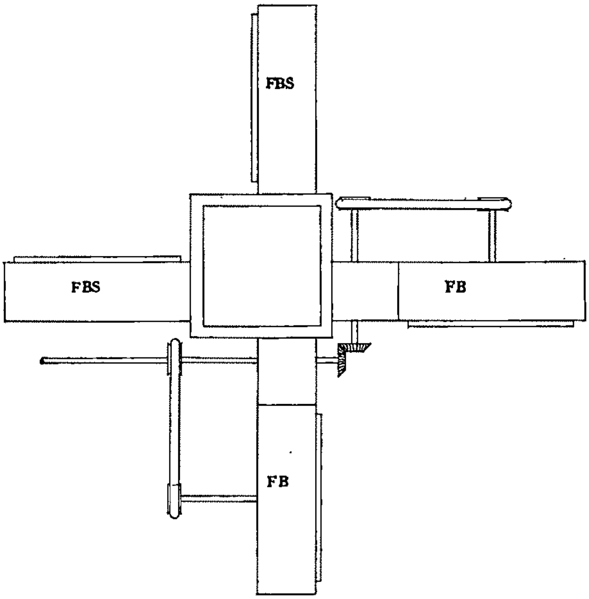

FIG. 1. BACK VIEW OF EYE-MOVEMENT CAMERA FILM MECHANISM

Fig. 2 is a sketch of the mechanism as seen from the left side. (FBS) is the film-supply box, (F) the film, and (S) the traction-sprocket. The unexposed film is placed in (FBS,) pulled through a narrow slit in the film-box, run across the exposure-box (LB), and drawn around the sprocket (S) from which it goes to the core (C) in the film-box below. As it turns the sprocket pulls the film across the exposure box, being held in place by a system of rollers (R). Behind the film in the exposure-box is a plate of ground glass (G) upon which the light from the subject's eye is brought to a focus to be seen and regulated by using pulleys of various sizes on the motor or the speed-reducer. The film moves about one foot in 3 sec.

The entire film-mechanism is mounted on a structure which connects with the camera tube by means of a photographer's bellows. This bellows enables the operator to disengage at will the mechanism from the power unit. Thus it is possible to present a subject with several practice selections with all of the moving parts in operation except the film-mechanism, the subject not realizing that his eyes are not being photographed. When the experimental selection is ready to present, the operator moves the mechanism backward until, the belt becomes taut and the film is set in motion. In actual experimental procedure, this feature of the set-up has distinct merit.

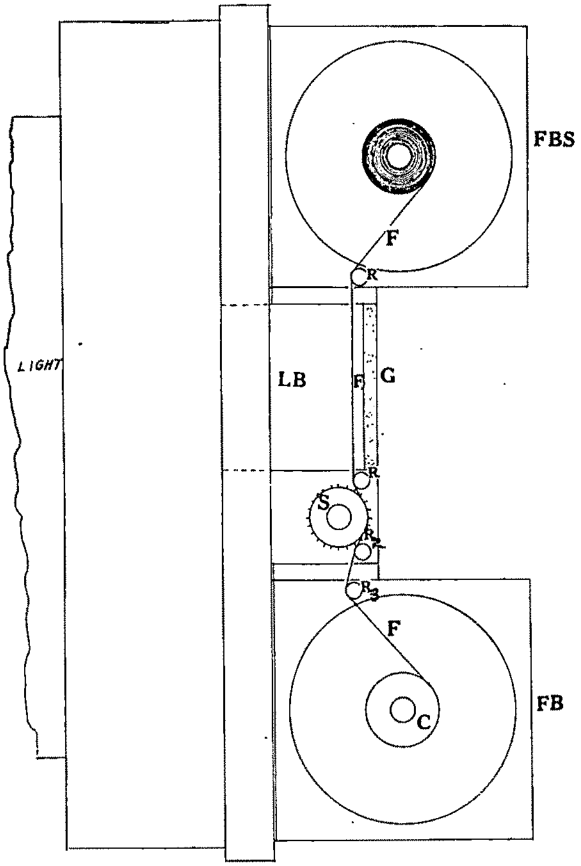

Fig. 2. Side View of Film Mechanism

An Exposure Apparatus

The exposure apparatus was built about a screen manufactured by the Error-No Company, Inc. of Rochester, N.Y., to hold a stenographer's copy while working at a typewriter. To convert the mechanism into an exposure apparatus for experimental or laboratory work, it was necessary to make several changes.

Fig. 3 shows front, back, and side views. The carrier-plate (A) to which the copy of material is attached by the clamp (B) is raised intermittently by the wheel (C). A metal tape, attached at one end of wheel (C) and drawn over the stationary pulley (E) to the lower edge of carrier (A), where it is fastened, is wound about the wheel (C) as the wheel rotates and thus raises the carrier-plate. Wheel (C) is turned by the friction-drive-wheel (G) in mesh with (C). Through the drive-wheel (G) is a shaft (H) to which is attached a ratchet (I) that is in turn articulated with a shaft fastened eccentrically to a pulley on the shaft of a chain of gears (K). The gears serve as a speed-reducing unit between the motor (M), being connected with M by means of a round belt and pulleys, and the carrier-plate. The pulleys on the speed-reducing unit, as well as those on the motor are of various sizes, so that the carrier-plate can be lifted at various speeds. This provides for a large range of exposure-times for material on the carrier.

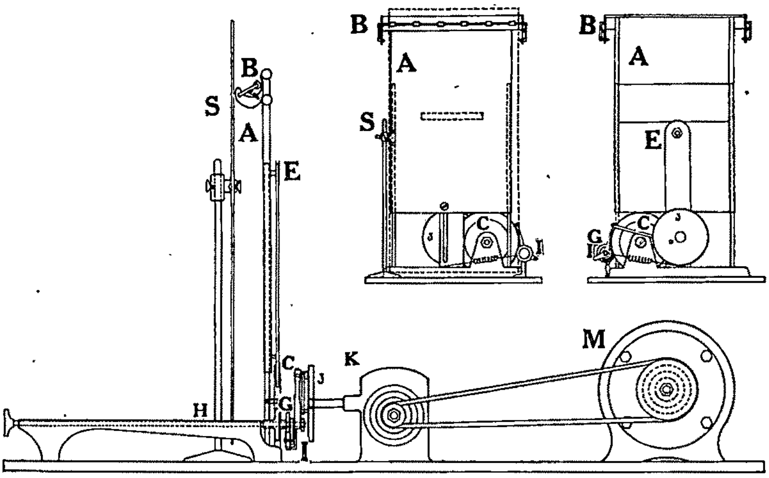

FIG. 3. FRONT, BACK, AND SIDE VIEWS OF EXPOSURE APPARATUS

Materials are exposed through a slit in the black screen S, cut at any desired length and height. The eccentric device is adjustable so that the amount of lift given the carrier can be varied. The adjustment enables the operator to space the lines to be exposed on a sheet with one, two, or three vertical spaces between. By using various lengths of slit any desired length of line up to eight inches is exposed. The carrier is lowered when it reaches the upper end of its lift by releasing the friction-drive-wheel (G) from wheel (C). The carrier drops by gravity and 'starts up' again as soon as the wheels are enmeshed. The two wheels are held in mesh by a taut coil spring.

The changes in the Error-No necessary to convert it into the exposure apparatus involved moving the ratchet device from the front·end of shaft (H) to the back end, as shown in Fig. 3. The shaft had to be adapted to this change. By using a synchronous motor, it is possible to expose materials at regular intervals of equal length, either simultaneously or successively. The apparatus has been used in elaborate experiments by Dr. Helen L. Koch and for class learning experiments. The entire mechanism is mounted on a stand 16 in. × 36 in.

APPARATUS FOR PHOTOGRAPHING FINGER MOVEMENTS

A mechanism used to record the finger movements of blind persons in reading Braille or Point includes the following assembly: a moving-picture camera, a speed-reducing unit, a synchronous motor, lighting and projecting devices and supporting tables (Fig. 4). The camera (A) is known as the Model A Universal equipped with a lens (B) of the constant-focus type. The camera is mounted on a platform (C) supported by four 1-in. pipes and flanges (D) to the top of a table (E). The lens points downward through an opening in the platform and is so arranged that the subject's fingers can be photographed while reading material placed on the reading desk (F) fastened to the table top. The camera is driven by a Westinghouse synchronous motor (G) which operates at a constant speed of 1800 r.p.m. Between the motor and the camera is a speed-reducing unit of the noiseless, run-in-oil type (H), made by the Boston Gear Works. This unit is driven by the motor by means of pulleys and belt (I). The camera mechanism operates off the slow shaft of the speed-reducer by means of a sprocket-and-chain assembly (J) which takes the place of the hand-crank with which the camera is equipped. Light

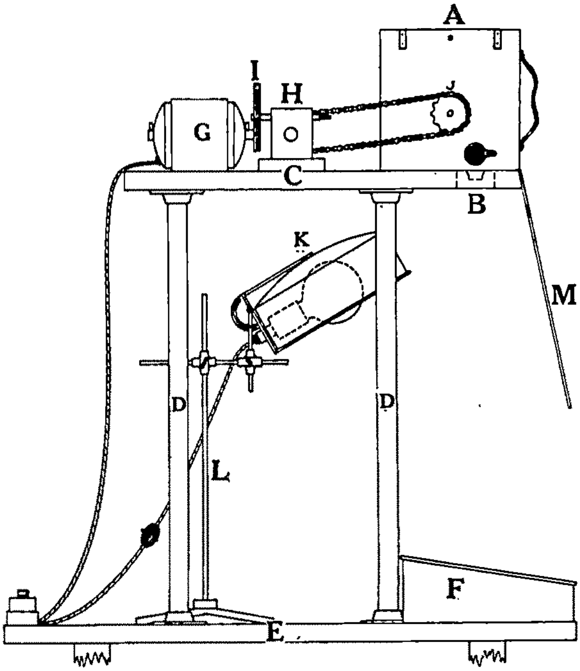

FIG. 4. SET-UP FOR PHOTOGRAPHING FINGER MOVEMENTS OF BLIND READERS

is furnished by a 1000-watt "Fotolite" (K) mounted on a stand (L) directly in front of the subject. A card-board screen (M) protects the subject from the glare and heat from the lamp.

In photographing finger movements, it is necessary to run the camera at such a speed that the subject can make no significant movements between exposures as he moves his fingers along the line of print. For ordinary purposes, 1/5-sec. exposures seem to meet this need. Since the constant-speed motor insures a regular exposure-time and a constant interval between exposures, the rate of reading by a given subject is determined by counting the number of exposures per unit of material (as word or line). By projecting the record on the material read, the position of the fingers at any point in the performance can be determined.

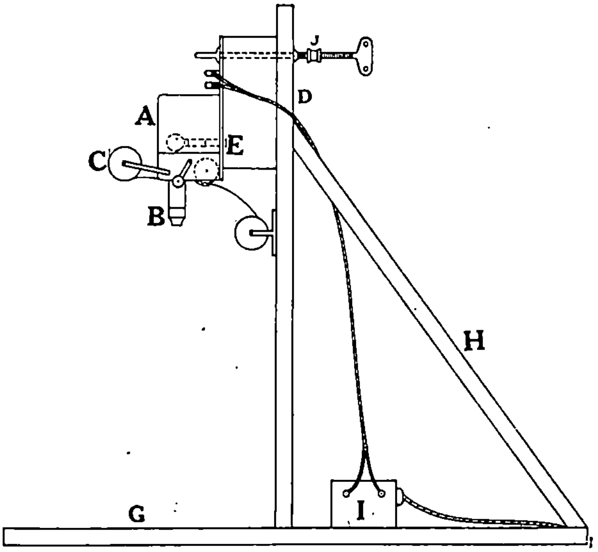

FIG. 5. DEVICE USED IN PROJECTING MOVING-PICTURE FILM RECORDS

PLOTTING DEVICE

The records obtained by the eye-movement camera or by the moving picture camera can be superimposed upon the material read by means of any projection lantern. The particular projector used at the University of Texas for this purpose is illustrated in Fig. 5. A light box 4 in. × 6 in. (A) contains a lens (B) and film mechanism (C) and is mounted on an upright (D) by means of the clamp (J). The upright piece is mounted on a board and held in rigid position by the support (H). The light-box contains a 32-candle power automobile light globe lighted by a 110-volt current through a transformer. By moving the light box up or down on the upright piece, the film record can be accurately focused and superimposed upon the material placed at (G). The block (E) holds the lens out from the upright in order to provide space for the sheet of paper containing the material.

The records of finger-movements are relatively easy to project if one observes the following procedure. First, the Braille characters used as experimental material are colored with black India ink. In the film-record the dots thus appear white; secondly, photostatic copies are made of the material with lines spaced about ¾ in. apart. These photostatic copies then contain black dots upon which the white dots on the film are projected. The film is moved one exposure at a time and a record is made of the position of the finger in that particular exposure. Thus one gets the number of exposures, or time, and the location of the fingers during each exposure.

SOME NEW ELECTRONIC DEVICES FOR THE PSYCHOLOGICAL LABORATORY

By David P. Boder, Lewis Institute

I. A Photo-Electrical Reaction Key

The device presented on Fig. 1 is a photo-electrical reaction key. (C) is a glass tube, ½ in. in diam., supported by metal ends such as are used for towel bars. Half-inch holes are drilled in the back of ends so that rod (E) can be introduced into the tube and also for purposes of ventilation. (D) is a 6-volt radio pilot light screwed into a socket, soldered to rod (E). The light from (D) activates the photo-electric cell (PE) which is covered by a cardboard pill box with a 1 × 1½ in. opening for the entrance of the light and is connected with a DeVry Photo-electric Kit. When the finger, placed at (F), is moved to the left, the light beam directed from (D) into the photo-electric cell (PE) is interrupted; this produces a break into the (PE) relay, and so breaks the chronoscope circuit, bringing the chronoscope handle to a stop. The reactions so far obtained compare favorably with, and are by no means longer than, those obtained with other chronoscope reaction keys, and the necessary number of practice trials for naïve subjects appears with this device significantly less than when other keys are used.

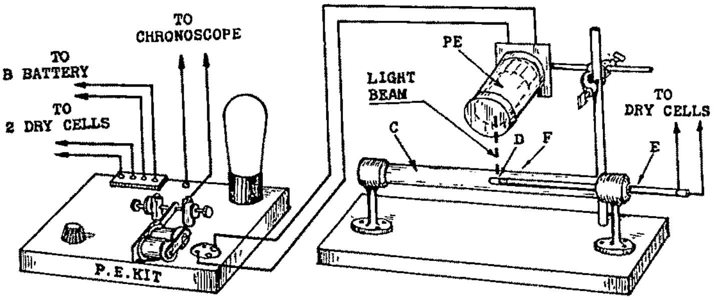

Fig. 1. A Photo-Electrical Reaction Key

II. A Voice Key and Speaker Relay

A microphone is connected to an amplifier or to an ordinary radio set.[1] (A) and (B), Fig. 2, are two magnetic loud speaker units, viz. a Baldwin and a Temple unit. To the armature of the speaker (A) is soldered a fine "jeweler's blade" (A_1), and to the rod at speaker (B) is soldered the brass attachment (B_1). The adjustment is made in such a way that (A_1) and (B_1) make contact in spite of slight impulses coming from the amplifier (or radio set). The leads from both speakers, in parallel or series, are connected with the output of the amplifier. When the accentuated syllable is spoken into the microphone both speaker units respond to an extent that (A_1) and (B_1) throw each other apart and the circuit of the chronoscope is broken. This same circuit-breaking device can be used to activate a signal magnet and to record on a kymograph or polygraph each accentuated syllable spoken into the microphone. Some disadvantages lie in the fact that the speakers are rather noisy and proper measures for damping the noise through rheostats or soundproof casing is desirable. It seems that unit (A) alone, with the blade (A_1), could be used for photographic recording of the sound impulses from the microphone, although for these purposes the device described below may be more efficient.

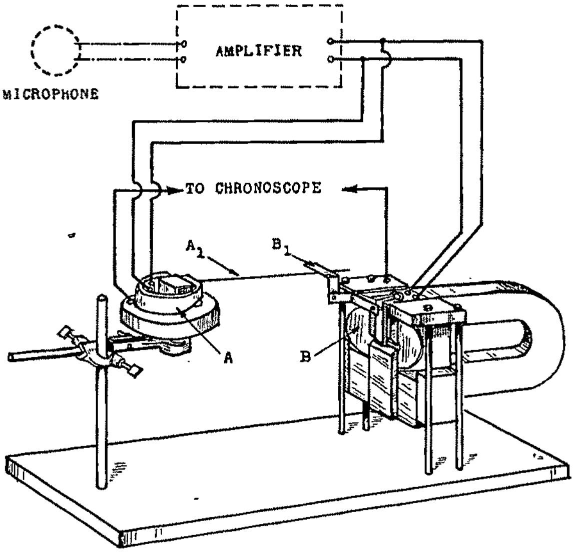

Fig. 2. A Voice Key and Speaker Relay

III. A Photo-Electric Word Counter

To eliminate noise and other possible difficulties which presented themselves with the speaker relay, the apparatus shown in Fig. 3 was built. A neon television lamp

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[1] Dr. Arthur Bills of the University of Chicago coöperated in the construction of this and the succeeding piece of apparatus.

(A) with a one-inch square screen is hooked up with the amplifier in place of the speaker, or in series with a magnetic speaker. The television lamp flashes with every accentuated syllable and so activates the photo-electric cell (PE) connected with the DeVry Photo-electric Cell Kit. The relay (R) of the Kit makes or breaks the circuit of the signal magnet (S) or any other writing device, offering the possibility for a study of temporal relationships between a number of syllables spoken in succession.

OTHER USES OF THE EQUIPMENT

(1) A television lamp of larger size such as manufactured by the National Carbon Co. (Everready) substitutes admirably, at least for demonstration purposes, the acetylene flame for the Scripture disk, and promises simplification of stroboscopic studies such as with the Seashore Tonoscope.

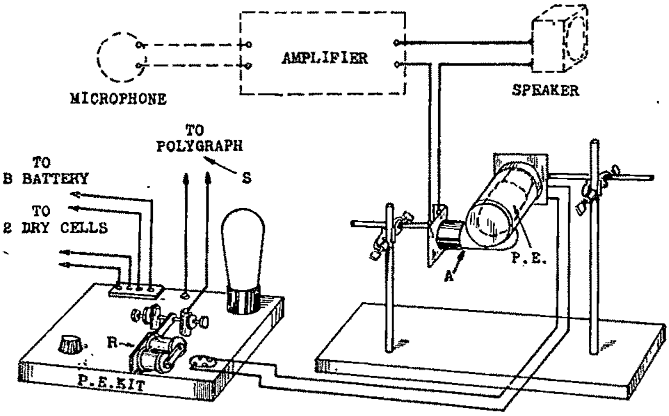

FIG. 3. A PHOTO-ELECTRIC WORD COUNTER

(2) A photo-electric timer for ½ or ¼ sec. is easily constructed by adjusting windmill wings to the shaft of the starting gear in the back of an inexpensive electric clock. This clock is placed in such a way that the wings interrupt the beam of light directed from a flashlight bulb into the (PE) cell—arranged as in Fig. 3, substituting flashlight for (A). The choice of the number of interruptions appears to depend mostly on the sensitivity of the signal magnet of the writing device. At the Lewis Laboratories up to 6 interruptions per sec. were obtained, but it would be safer to use it for not more than 4 per sec.

(3) A constant gramophone motor can successfully be used instead of the clock. Cardboard strips about 5 in. wide protruding about 4 in. outside the revolving disk serve as excellent beam interrupters—arranged as in Fig. 1—the whole device makes an excellent time-marker free from mercury or other mechanical contacts, and operates almost noiselessly. The whole equipment is relatively inexpensive.

A SIMPLE CAMPIMETER

By Forrest Lee Dimmick, Hobart College

The ordinary type of quadrant perimeter has several disadvantages for use in the elementary laboratory. The simpler forms require steadiness on the part of the manipulator and are correspondingly inaccurate; the better instruments are expensive; and all necessitate duplication of the observations if each student is to understand the experiment. A campimeter described by Titchener avoids some but not all of these difficulties.[1] Following it in principle, we worked out a device in the Hobart Laboratory that has been most satisfactory for the elementary laboratory experiment.

The apparatus (see Fig. 1) consists of a tripod head-rest which rests upon a table in front of *O* and holds his head horizontal 20 cm. above the campimeter field.

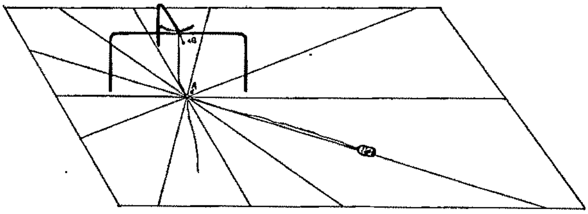

Fig. 1. The Hobart Campimeter

The head-rest has two eyelets, one (A) that just clears the table, the other (B) directly over the first and 1 cm. beneath the right eye, so that when the head is in position *O* fixates the lower eyelet through the upper. The stimulus, a disk of colored paper 1 cm. in diam., is carried on a slide made of thin metal (1 cm. by 2 cm.) painted gray. A thread is attached to the slide and runs through the lower eyelet. The student prepares a campimeter about 50 cm. by 75 cm. in size with lines 30° apart radiating from a point 25 cm. from the top and left end of the paper. The head-rest is placed with the lower eyelet (A) directly over the center of the radiating lines. *O* blindfolds his left eye and puts his head on the head-rest so that with his right eye he fixates through the two eyelets upon the focus of the lines. The stimulus-carrier upon which a disk of the color to be studied has been placed is set at the extremity of one of the lines on the chart, and *O* pulls on the free end of the thread bringing the stimulus slowly in along the radius upon which it is set until the color is seen. When this point is reached, he stops pulling and marks the position of the stimulus. Repeating this procedure for every one of the radiating lines he obtains a fairly accurate map of his field of vision for that color.

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[1] E. B. Titchener, *Experimental Psychology, Student's Manual, Qualitative,* 1901, 10.

APPARATUS NOTES

TABLES FOR THE CONSTANT METHODS

I have recently had reprinted, for the convenience of students in psychophysics, the various tables that are used for computing and checking in the methods of constant stimuli and constant stimulus-differences.

Urban's table, reprinted from the *Archiv für die gesamte Psychologie,*[1] is reproduced with all the corrections that have been reported to date,[2] and with rules intersticed after every 5 lines so that the reader's eyes may more easily and accurately follow across the double page.

Rich's checking table is reprinted from this JOURNAL.[3] It is reproduced, with 5 corrections,[4] in a single double-paged table. The values for 'x' from —7 to +7 and at the different percentages can be read across and down the table without turning a page. The ease with which the table may be used is thereby greatly increased.

Hoisington's table for graphic checking, also reprinted from this JOURNAL,[5] is reproduced without change.

All the tables were reprinted by line cuts. No type was set; the corrections mentioned above were made in the original tables, which were then photographed and reproduced in zinc. Thus new errors in the reproduction were avoided.

Copies of these tables are available at a slight cost. They may be obtained from the Cornell University Department of Psychology or the Business Office of this JOURNAL.

Cornell University KARL M. DALLENBACH

A USEFUL PERIODICAL ON INSTRUMENTS

Instruments, a monthly magazine, contains much information of interest to experimental psychologists. One of its most valuable sections is the department, "New Instruments," where the latest types of measuring, recording and controlling devices are illustrated and described. Examples of some of the instruments used by psychologists and described in this magazine are the following: Steady electric governed variable speed motors; Silent vacuum contacts; Illuminometers; Small relays; Photocell outfits; Stopwatches; Speed indicators and counters; Electric counters; Electric meters; Color matchers; Recording apparatus; Automatic regulation and control equipment; and Temperature control devices.

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[1] *Op. cit.,* 24, 1912, 240.

[2] Four errors have been reported: two by F. M. Urban (*Praxis der Konstanzmethode,* 1912, 20), and one each by J. G. Rich (this JOURNAL, 29, 1918, 121) and H. H. Long (*ibid.,* 33, 1922, 303).

[3] *Op. cit.,* 29, 1918, 120 f.

[4] A typographical error noted by Rich (this JOURNAL, 29, 1918, 232), and four changes necessitated by Long's correction of Urban's table. Two of these four changes were pointed out by Long (*ibid.,* 33, 1922, 303) and two by H. F. Davies (*ibid.,* 34, 1923, 311). One of Long's corrections, as Rich later pointed out (*ibid.,* 33, 1922, 450), is incorrect; and so also, strangely enough, is one of Davies'. The value for $p = .27$ and $x = +2$ should be 4.9665, instead of 2.9665 as given by Davies.

[5] *Op. cit.,* 33, 1922, 245.

In each issue there are two or three articles on different phases of instrumentation and several pages of abstracts of articles on instruments in American, English and German publications.

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Massachusetts State College HARRY R. DESILVA

A SIMPLE TACHISTOSCOPE OF MANY USES

Modern experimentation not infrequently demands that stimulus-objects of various sizes be exposed for brief and definitely determined lengths of time, and that the observer be maintained in a condition of light-adaptation between exposures. Since the ordinary tachistoscope is unsatisfactory in at least two of these respects, the writer has employed a modified mirror-tachistoscope with a condenser-charge control which has proved very efficient. The tachistoscope, based upon a general principle which Dodge (1907) cleverly adapted from a spectacular illusion of stage magic, is constructed of two chambers, one of which is set at right angles to the other. At the junction of the two chambers, a sheet of good glass is set at an angle of 45° to the line of regard. When the chamber beyond the glass and along the line of regard is dark, and the chamber at right angles to the line of regard is brightly illuminated, the glass serves as a mirror and reflects in excellent fashion whatever object (frequently a fixation-point) is put in this 'fixation' chamber. When the illumination is reversed with the fixation chamber dark and the exposure chamber illuminated, the observer is, of course, able to see directly through the glass which does not interfere at all with his view of the stimulus-object.

The writer will presently publish a wiring diagram of the condenser-charge apparatus designed by a local electrical engineer for the express purpose of the reciprocal operation of the lights in the two chambers. For the moment it may be enough to say that a condenser is charged through the grid of a tube, the plate of which is in series with the windings of a make-and-break telephone relay operating on 90 volts. The capacity of the condenser being held constant, its time of charge is a function of a resistance employed as a leak in the grid circuit, and the time may be varied without difficulty simply by varying the resistance. This is done in the present case by having available a series of grid leaks (1 to 20 megohms) which may be inserted in the circuit as they are needed. The apparatus is stable and is capable of operating for any length of exposure from several minutes down to a few sigma (the lower limit being set by the relay). Another virtue is that it may be constructed for almost any separation of observer and stimulus-object, as well as for any size of exposed object. The flexibility of the apparatus as to size, distance from fixation-point, and length of exposure, has made it a useful addition to the equipment of the Cornell laboratories.

Cornell University JOHN G. JENKINS

NOTES AND DISCUSSIONS

GESTALT PSYCHOLOGY AND BEHAVIORISM

Modern experimental psychology, having passed through two stages of development, seems about to enter a third. Unlike the so-called "natural" sciences, psychology has been unable to build a science of facts wherein each fact indicates the interpretation that should be put upon it, because the question as to what a psychological fact is still remains open. According to the Wundtian tradition a sensation is a psychological fact, and the various sensations can be classified with respect to their qualities and other variables. According to the Brentanian tradition it is not the content of experience but the experiencing, not the sensation but the sensing, that is factual. Thus a structural psychology differentiates itself from a functional psychology, the one finding its facts in the *contents* of experience and the other in the *acts* of an experiencing individual.

For purpose of experimentation the Wundtian position proved more fruitful than the Brentanian. One could study sensation in terms of the physical and physiological conditions of the sense-organ involved; whereas sensing is so general an activity that one can do little more than point to the thing sensed as being either a physical object or the result of some previous sensing of a physical object.

English psychology, which was slow to adopt the experimental method, shows the influence of Brentano, notably in the writings of Stout and Lloyd Morgan. American psychology, however, adopted the experiment at once, and laboratories were established in every college. But the experimental method employed in these laboratories was in many cases modified in the direction of a functionalism suggested by James, in which both contents and acts, things and their relations, existential data of consciousness and the relations of these data with one another, were accepted as two kinds of facts with which psychology might deal.

Thus the stage was set in America for behaviorism at the same time that Gestalt psychology was emerging from the traditions of Wundt and Brentano in Germany.

The American revolt against the Wundtian tradition of existential psychology and the functionalism of experienced things and their relations, was a revolt against the private character of experience as such. The German revolt against contents and acts was directed against the static character of a psychology in terms of sensations, affectations, images, and thought-contents such as *Bewusstseinslagen* and *Bewusstheiten*. American behaviorism began by translating sensation and sensing into the objective terms of stimulus and response, and obtained its objective character by the simple expedient of correlating stimuli which could be defined physically with reactions which in principle reduced to muscular contractions and glandular secretions. The living body, with its sense-receptors connected by nerves through spinal cord and brain with muscles and glands, was supposed to supply a framework within which varied combinations of these connections by use and disuse would account for development and learning. Behavior, for this kind of behaviorist, was a more or less arbitrary combination of responses, each of a "can't-be-helped" type and thoroughly mechanical in its operation.

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* This paper was read at the Tenth International Congress of Psychology in Copenhagen, August 23, 1932.

Gestalt psychology began by criticizing the structure of experience. Instead of denying consciousness as a means or end of psychological investigation, it pointed to the inadequacy of sensation and its derivatives, including the relations between contents, as a scientific description of experienced events. Wertheimer's "phi-phenomenon" of movement was a challenge to the existentialist because the phenomenon could not be reduced to the elements of experience with which existential psychology operated. While motor psychologists accepted it as evidence of a neurological short-circuit vaguely reflected in consciousness as a kinaesthetic experience that could not be precisely defined, Titchener set one of his students to study it *sub specie existentialis* with the result that it was defined as a *gray flash*.[1] But though all seen-movements might be "gray flashes," it was not apparent that all gray flashes were seen-movements. In other words, the meaning of the phenomenon was intentionally excluded from the existentialist's description. The issue here was one of principle. Titchener contended that there were no meanings in science. The descriptions of science, he said, deal with bare facts. Implications are logical and referential. But where do these implications arise if they have no basis in fact?

Gestalt psychology maintained, as James had done, that meanings are implicit in every phenomenon; that they are fundamental conditions of the experienced phenomena. Thus no phenomenon is significant *per se*, but is always a phenomenon of something else. The something else is a behavior, or a part of a behavior.

Thus Gestalt psychology became a behaviorism,[2] but it reached this conclusion by a different route and with a different orientation from those of American behaviorism.

The Gestalt psychologist, unlike the behaviorist, was not at first disposed to deny consciousness, but to reinterpret it. Instead of questioning the existentiality of consciousness as a fact, he questioned the existentiality of the elements into which consciousness had been analyzed.[3] Instead of "complexes" and "constellations" of elements with their attributive variables: quality, intensity, extensity, duration, etc., he found *Gestalten*, each of which was an unanalyzable whole—a phenomenon which analysis utterly destroyed, leaving in its place fragments which are no more the constituent parts of the original whole than fragments of broken glass are the true parts of a bottle to which they once belonged.

But this criticism of the "constancy-hypothesis" of the existentialist was only a beginning, which gradually led to a new psychology of perception. Hitherto perception had been a certain complex of sensations, tied imagery, and affections; now it became something over and above these elements in combination. A new content had been produced which was something more and other than these elements and their attributes. To the Austrian School of Psychology a *Gestalt* quality was a synthetic product. For the members of this school the elements remained ultimate facts, but the qualities of form, geometrical, musical and so on, were seen to depend not on their elements but upon their relationships. Elements *exist,* but the relations which constitute a melody and permit its transposition on another key *subsist.* Analysis of a melody breaks the subsistent relations of its notes, and thus destroys it utterly, leaving only the elemental discrete sounds as fragments.

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[1] F. L. Dimmick, An experimental study of visual movement and the phi phenomenon, this JOURNAL, 31, 1920, 317-332.

[2] M. Scheerer, *Die Lehre von der Gestalt*, 1931.

[3] R. M. Ogden, Are there any sensations? This JOURNAL, 33, 1922, 247 ff.

But what is the activity which thus binds elements together by creating subsistent relations between them? Science is a knowledge of facts. Agencies that bring things together and tear them apart must be known as facts before they can be accepted. Even though they are confined to one's own private consciousness, sensations, images, and feelings are in some sense facts; but the relation of two or more of these elements as they belong or do not belong together is nothing one can describe in terms of itself. The relation of one thing to another does not exist, it subsists between the two. The analytic facts are these two things, but their relationship is something more and other than the two facts taken separately. Consider a perception of two things, one greater than the other, without analysis. Is it possible to regard this perception as one whole in which the analytic facts of the two separate things have not yet emerged? Köhler's choice-training experiments with hens,[4] seemed to demonstrate that at a low level of discrimination the factual situation is not two things, one greater than the other, but a gradient of direction from less to more, or *vice versa.* At this level perception is not a complex of factual things; it is a direction of behavior which includes not only the two things as terminals of the direction—more or less than—but also the hen which wants food that can be taken from one terminal of this gradient. From such experiments we learn that perception is more than a complex of conscious facts with relations somehow added to make them wholes; it is a dynamic movement of the organism towards or away from certain features of the environment that attract or repel it.[5]

Thus Gestalt psychology, beginning with existential phenomena, was led to a behaviorism which denies the reality of these phenomena; not, as the behaviorist said, because they are private—mine and yours—but because they are at best criteria of behavior and not facts in themselves.

Meanwhile, the development of behaviorism has been moving in the same direction. The investigations of Lashley have shown the inadequacy of any theory of behavior which regards external stimuli as the motivating occasion for the reactions of an organism.[6] The frame-work of fixed connections in its nervous system gives but a partial account of the organism's behavior. In addition to the fixed connections which account for reflex action, a self-regulation of the organism from the very beginning of the individual's life, and throughout its embryonic development, has been amply demonstrated by the anatomical researches of Coghill.[7] The organism behaves, to begin with, as a fully integrated unit and proceeds in its development to throw off partial patterns of behavior by means of special organs which are individuated both structurally and functionally in response to organic needs.

The behavior of a living creature is neither predetermined by its inherited structure nor is it a result of chance-connections determined by environmental influences. The old problems of nature and nurture involving the major influences of heredity and environment are pseudo-problems. What the organism does at any moment of its existence is a result of such a structure as it may possess when animated by a want. This want finds its expression in the attractive and repellent features of its environment. Without the presence of a goal towards which the organism can strive, behavior lacks definition and is aimless. But the very aimlessness of a restless behavior alters the environment in which the organism finds itself, and thus creates the possibility of accents or foci of attraction and repulsion which give pattern to its behavior.

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[4] W. Köhler, Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, *Abh. d. Preus. Akad. d. Wiss., Phys.-math. Kl.,* 1918, 101.

[5] Cf. the investigations of K. Lewin. Summarized in *Die Entwicklung der experimentellen Willens-psychologie und die Psychotherapie,* 1929; and in *Psychologische Situation bei Lohn und Strafe,* 1931.

[6] Cf. *Brain Mechanisms and Intelligence,* 1929.

[7] Cf. G. E. Coghill, The structural basis of the integration of behavior, *Proc. Nat. Acad. Sci.,* 16, 1930, 637 ff.

Thus Gestalt psychology and behaviorism converge upon the same problem with the same outlook. Many of us are no longer arguing whether we shall or shall not include the phenomena of consciousness in psychology, because we are agreed that the facts of psychology are facts of behavior. But we need not rule out the conscious criteria of behavior because they are private. They are that only so long as we regard them as facts in themselves. So soon as conscious phenomena are accepted as criteria of behavioral adjustments of the organism to its environment the nature of these phenomena no longer interests us as a structural fact but only as a functional index of the behavior that is taking place.

The behaviorist's first statement of his problem, with consciousness excluded, left him to pursue a generalized physiology of the whole man; a pursuit which did not clearly differentiate psychology from physiology. But psychology is something other than general physiology, its special province being the employment of conscious phenomena as criteria of behavior.

The step which modern psychology has at last taken, and which bids fair to define it as a natural science, is the frank admission that experience, mysterious though it may be as a phenomenon, is always an experience of behavior, or some aspect of behavior, and never an experience *per se.*

In accepting perception as the starting-point of any truly psychological investigation, we are dealing, not with a panorama of sensory phenomena, but with an adjustment of the living creature to its environment. This adjustment involves conditions which point in two directions: backwards and forwards. The backward direction leads us to investigate pre-perceptual adjustments of an instinctive character to which no special heed was given. Instinctive behavior is "blind" because its process of "closure," or achievement, does not permit the differentiation of the self which wants something, from the goal which satisfies this want. Whenever an attitude of receptivity or denial emerges as a partial pattern within the total behavior of being attracted or repelled by an environmental thing or event, we have perception. The creature then heeds its own actions in terms of this differentiation of its behavior pattern.

Having once felt this differentiation of its own wants from an object or goal that will satisfy these wants, the perceiving organism can proceed to gain insight into its behavior by a further process of individuation. The criterion of insight is a discernment of these members of the behavior-pattern, and is clearly evinced in those cases where the closure of subjective want and objective goal is effected by some definite means. The forward direction which investigation takes goes beyond the bare feeling of perception in discerning an attitude of receptivity, an object for which one is receptive, and a means whereby this object is attained. Yet in the employment of any and all of these means of adjusting the creature more adequately and precisely to its environment, the fundamental motivation of satisfying felt wants is maintained. Originally, one grasps things with hand and mouth. Later one grasps them with eyes and ears. Apprehension, however, is always more than a combination or constellation of phenomena revealed, as it were, upon some inner mirror of the mind, for these phenomena are themselves indications of what is being done at the moment.

The negative side of the pattern of insight is provided by obstacles in the way of attainment and by barriers or repelling forces. When these become dominant the creature's want is overcome and is superseded by the behavior of escape from the field in which the attractive goal is situated. There is then a marked tendency to resort to instinctive behavior without insight and without perception. The creature rushes blindly in the opposite direction, and beats aimlessly against whatever barriers may prevent its escape.

Yet granted the capacities of perception and insight, a barrier to escape in one direction may lead to ingenious devices for escape in other directions. Thus instinctive anger, fear, and disgust are not the only expressions of conflict. Substitutional forms of behavior, in which one does what one can, though not what one would do, offer temporary satisfactions. With abstract as well as concrete tools to work with, one may raise himself theoretically above the field of actuality on to a plane of contemplation where by recourse to ideas and plans one's best hopes are no longer thwarted, and one gains in thought and imagination what one cannot have in reality.

All these substitutional means to ends are derived from actual means to ends, and the behavior of thought and contemplation is itself no less actual than perception. While no longer overt, the behavior is still real; the attitudinal and postural resources of the self have merely acquired gestural, verbal, numerical, and graphic means of representing actions and fulfilments unhindered by the barriers and obstacles of a physical environment.

In order to be able to deal with these inner conflicts and achievements one must have recourse to the experience of them. No purely objective research, such as a biologist is wont to pursue, can ever reveal the minute details of these patterns. They must first be known as they are experienced. Then arises the biological problem of determining the physical status of a process which is so largely completed within the organism.

Even if we knew the physical basis of thinking, it would be absurd to demand an instrumental record that could be read from a scale, in lieu of the direct account that can be so much more readily given by the thinking person. Important as scale-readings may be as direct evidences of what is going on in the brain and other parts of the organism, it will remain the task of the psychologist to set the neurologist's problem for him; because only the psychologist has direct insight into what the organism is doing with itself. This view of behavior from the inside is the special province of psychology, and the problem is no less one of behavior because the indications of it are in a true and unique sense psychological rather than biological or physical in nature.

Cornell University R. M. Ogden

ATTENSITY, CLEARNESS, AND ATTENTION

The greatest obstacle which at present hinders the student of attention is not that of technique. Rather, the dilemma of systematic premise increasingly embarrasses him, for the outstanding fact of the psychology of attention in the last decade is that of a sharper and sharper division into two doctrinal parties.

The first of these schools sees in the phenomenon of attention the manifestation of *sensory* clearness exclusively, in the Titchenerian sense of that adjective. The school enjoys the vigorous leadership of Professor Dallenbach, from whose 1920 paper on "Attributive *vs.* Cognitive Clearness"[1] the whole controversy as an explicit clash of doctrine proceeds; and the subsequent defense of sensory clearness in relation to attention has been conducted for the greatest part by him and his students. Professor Dallenbach's position is consistent with Titchener's and may be epitomized in these terms: the problem of attention is the problem of the pattern of sensory (*i.e.* attributive) clearness as distinguished from all other clearnesses (*e.g.* cognitive clearness). He was thus led in 1920 to reject from the field of attention the classical experiments on range; for "the experiments hitherto made upon 'range of attention' are perceptual experiments, in which the end-result is the range, not of attention, but of cognition," since "in experiments on the range of attention, range has always been determined by the number of objects—lines, letters, words, numbers, figures, etc.,—which can be named or reproduced after a momentary exposure; that is, by the number cognised."[2] And again, in 1926[3] and 1930,[4] he includes in his reviews of work on attention only with reluctance experiments dealing with cognitive clearness.

The second school—embracing those who heterodoxically erect the phenomenon of attention upon *cognitive* clearness—seems to me to be without definite leadership. It is revealed rather by a number of independent investigators who have at one time or another come into conflict with Professor Dallenbach; and it derives unity largely from a common rejection of his doctrine. Professor Dallenbach has himself provided complete references to his critics' papers;[5] they need not be recited here.

It is my opinion that this state of sharp disagreement cannot but retard experimental progress. For the individual investigator, however enthusiastically he may set to work, soon runs against the unhappy necessity of choosing whether to side with a conception of attention which discards with one gesture what has come to be regarded as classical research, or, taking the other side, to risk the demonstration when he is through that he has not, *ex hypothesi,* dealt with attention at all. If one takes the first alternative, one must subscribe to the unattractive persuasion that very nearly all the experiments on attention, as far back as Hamilton, have really contributed nothing to the field. Or, accepting the second, one must perforce take issue with a doctrine that has behind it the considerable prestige of the Titchenerian psychology—the very system that, preëminently, urged the implicative importance of attention. Can either choice quickly tempt the student who recollects the splendidly confident pronouncement that "the doctrine of attention is the nerve of the whole psychological system?"

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[1] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.,* 3, 1920, 184-230.

[2] *Ibid.,* 228.

[3] *Idem,* Attention, *Psychol. Bull.,* 23, 1926, 1-18.

[4] *Idem,* Attention, *Psychol. Bull.,* 27, 1930, 497-513.

[5] *Idem,* this JOURNAL, 40, 1928, 337 f. (footnote 3).

And yet we must choose!—else we shall never come to the empirical knowledge of attention that a complete science demands. The quandary is intolerable precisely because the problems that generate it are fundamental.

The burden of this paper is an essay toward such a choice. Its fundamental contention is this: Granted (as will be apparent) that the question whether or not to consider attention as coterminous with attributive clearness is one of sheer definition, and that with respect to definition there is no *a priori* imperative to define one way or another—still, once a definition has been accomplished, its logical and practical consequences must be such as to justify its continued acceptance. And I intend to maintain that the theory of attention in terms of cognitive clearness is superior to that in terms of sensory clearness in those respects.

First, however, we must understand what is meant by attributive (sensory) clearness, or *attensity,* as we shall henceforth call it for simplicity's sake. Our search for definition carries us back to Titchener; and the following quotations are presented as appearing to me to be at once sufficiently concise and representative:

> Clearness [attensity] is an attribute of sensation, which, within certain limits, may be varied independently of the other concurrent attributes.[6]

> Clearness [attensity] . . . is the attribute which gives a sensation its particular place in a consciousness: the clearer sensation is dominant, independent, outstanding, the less clear sensation is subordinate, undistinguished in the background of consciousness.[7]

> We may group all the attributes of sensation under two headings, qualitative and intensive. I should, for instance, rank as intensive attributes, in the broad sense, degree or intensity proper, duration, extension, and clearness [attensity] . . . Clearness . . . varies between a liminal value and the maximum set by the limit of attentional concentration.[8]

> Clearness [attensity], our fourth intensive attribute, is no more definable than its fellows. It is the attribute which distinguishes the 'focal' from the 'marginal' sensation; it is the attribute whose variation reflects the 'distribution of attention.'[9]

We must, of course, amend these definitions in one respect, for, if we are to retain a structuralistic concept of attensity, we must recognize that latter-day structuralism was forced by internal criticism away from the notion of attributes of sensation to the notion of dimensions of sensory experience.[10] But, with the inclusion of this significant change of terminology, a sufficiently faithful characterization of attensity would appear to be something of this sort:

Attensity is an independently variable, intensive dimension of experience, revealed typically in attentional structure, but so primitive as to be otherwise indefinable.

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[6] E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention,* 1908, 211.

[7] *Idem, A Text-Book of Psychology,* 1910, 53.

[8] *Idem, Feeling and Attention,* 10.

[9] *Ibid.,* 26.

[10] Cf. Titchener, this JOURNAL, 35, 1924, 156.

As distinct from attensity, we now wish to define or characterize cognitive clearness—or simply *clearness* (as, again, we shall most conveniently call it.) Professor Dallenbach refers us to Titchener for that distinction. The passage is as follows:

> When a tachistoscopic field is exposed for the first time to an unpractised observer, he will very probably fail to 'make out' anything at all; the lines or letters or geometrical figures are seen as a general impression, without discrimination of detail. Was, then, the field obscure? Surely not! The observer was 'attending' with all the concentration he could summon; the field was the clearest thing in his consciousness. What he failed to do was to *cognize.* Cognition is not clearness [attensity]; it is an associative process of the assimilative kind. Apperception and cognition are so usually conjoined, in our adult experience, that we may sometimes forget to separate them; but psychologically they are different things. When, then, the practiced observer tells us that some of the details in the exposure-field are 'clear' and others 'half obscure,' he means that he has cognized the former and failed to cognize the latter.[11]

Here again, as with attensity, the experiential characteristic is easier to demonstrate than to define. Nevertheless, certain descriptive terms at once suggest themselves. Cognizability is, naturally enough, the truest synonym of this clearness. And cognizability implies immediately reportability—a point with which Professor Dallenbach no doubt agrees; since, in refutation of Wever, he labels the report of one of the latter's observers to the effect that "a thing is clear in proportion as it can be reported upon" as a characterization of cognitive clearness.[12]

Reportability, in turn, implies adequacy of content in respect of whatever characteristics the observer is, at the moment, interested in reporting; and this is a fact of no little importance. It was deduced originally by Britz, whose research on clearness[13] motivated Professor Dallenbach's 1920 paper. Britz tells us, concerning the experience of his observers when colored rectangles were presented to them tachistoscopically:

> A vague patch of color occurs as a conscious content. The observer will naturally say to himself that this color should be considered less perfect than other colors which were simultaneously—but not so vaguely—present. Yet, if he were to see this color under other conditions and isolated, he might well feel no inclination to such a judgment; for neither the opportunity nor the intention to relate one content to others is always at hand. The observer knows that the colors represent rectangles, and this knowledge influences him to rate un-contoured colors lower in clearness than contoured ones; for he is quite convinced that the color *could* appear as a rectangle. Under other circumstances he would hardly ever bring the given content into relation with other possibilities.[14]

And Britz comes to the conclusion that clearness is *"ein Urteilsmässiger Vorgang."*[15]

This notice of the *Urteilsmässigkeit* of clearness is of moment for its assertion that clearness is not only a judgmental affair, but that the judgment varies relative to whatever particular aspects of experience the observer happens to be contemplating. This general observation is, indeed, particularized in Titchener's example, given above. There the observer who says the exposure-field is unclear is at once correct and incorrect: correct, because if he expects to be able to see well-defined letters or lines, his experience is inadequate to that expectation, and hence unclear; and incorrect, because, relative to a possible expectation of "a general impression without discrimination of detail," his experience is in truth adequate and clear. We may, with Britz and Professor Dallenbach, regard the various degrees of adequacy of detail with which, *e.g.* a visual impression may occur—from a mere localizable blotch to a determinately bounded and colored figure—as levels of cognition. A given impression is clear, then, relative to the level of cognition to which its experienced properties are adequate, and less clear relative to any higher level. And so, before we can make a judgment as to clearness, we must presuppose an expectation or an *Einstellung.* Adequacy for report-from-such-*Einstellung* constitutes, in consequence, the measure of clearness.

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[11] *Feeling and Attention,* 238, cited by Dallenbach in this JOURNAL, 40, 1928, 339 (footnote 11).

[12] Dallenbach, Dr. Wever on attention and clearness, this JOURNAL, 40, 1928, 342.

[13] C. A. Britz, *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit,* 1913.

[14] *Ibid.,* 67; the translation is mine.

[15] *Ibid.,* 75.

In fine then, we may characterize clearness (in our sense of cognitive clearness) *as determinateness or adequacy of experience in respect of agreed-upon properties, and measurable by the reportability* (for example) *of such properties.*

With the natures of attensity and (cognitive) clearness, respectively, now in mind, I wish to offer my objections to a doctrine that bases the attentional process on the first of these affairs exclusively.

The first criticism which comes to mind rests upon the evident disparity between an attention of attensity and the large pre-psychological problem of attention which scientific psychology adopted. Attention—even restrictedly as a mode of structure of experience—is no recondite or academic phenomenon. Its observation lies close at hand and is accomplished frequently and independently of instruction by the naïvest individuals. Despite the popular confinement of the term *attention* to what we should call *secondary attention,* the student who is introduced in class to the law of the selectiveness of conscious representation as a sheer general fact recognizes in that fact something strongly familiar, from the recollection of numberless instances. He knows already that he can 'pay attention' to one thing only at the cost of the vididness of other simultaneous experiences; if he attends to one of a number of persons talking in his presence, the others begin immediately to utter mere nonsense sounds; and where his interest is absorbed by one event, his impression of concurrent happenings (which he is astonished not to have noticed) is often inadequate both at the moment and for recall. We may agree with James that "everyone knows what attention is. It is the taking possession by the mind, in clear and vivid form, of one out of what seem several simultaneously possible objects or trains of thought. Focalization, concentration, of consciousness are of its essence. It implies withdrawal from some things in order to deal effectively with others."[16]

There is inherent in pre-psychological descriptions of attention the variously expressed notion of equivalence between high degree of cognitive clearness and focality for experience. The allusion to it by way of James' statement of effective dealing with parts of consciousness is patent; the common remark—"I didn't pay enough attention to understand it"—reveals the same thought; and Titchener's untrained observer, who falls so easily into judgments of clearness rather than attensity, thus gives expression to his untutored belief.

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[16] W. James, *The Principles of Psychology,* 1890, I, 403 f.

Our natural expectation would, therefore, be to find the psychologist subsuming the phenomenon of attention under scientifically derived laws of cognitive clearness. The layman (and conceivably a number of psychologists!) would be, presumably, well satisfied with the statement that attention is a matter of clearness. Curiously, however, the structuralists have resisted the idea.

The resistance, as we have said, is evidently not predictable from the mere common-sense of attention. It ought therefore to have behind it some very solid argument of a high level of sophistication. But to look for this argument in the precise place where one would expect to find it is to invite disappointment. Recall, for a moment, how the marriage of attention and attensity is carried off in *Feeling and Attention:* "It seems to me beyond question," Titchener writes, "that the problem of attention centres in the fact of sensible clearness. Let me call my witnesses. . . ."[17] There follow five pages of quotation from Wundt, Pillsbury, Ribot, Ladd, Stumpf, Stout, Baldwin, Bradley, and Ferrier. All are agreed that attention is characterized by the clearness of certain contents, or at least by a condition reasonably synonymous with clearness; so that Titchener is quite right in his statement that "wherever you look, you find some form of reference to clearness." But none of these witnesses has the slightest contribution to make toward an identification of clearness with *attensity*—least of all, Wundt! So that, when Titchener concludes—"I have been obliged to turn up a small library of references, in order to make sure that my position is well taken. With that assurance gained, let us proceed to a study of clearness as an attribute of sensation"[18]—we may well take exception to this gratuitous feeling of security.

Beyond its *non-sequitur* from independent authority, the Titchenerian doctrine is furthermore essentially circular in logic. On the one hand, we have the definition of attensity as "the attribute whose variation reflects the distribution of attention," and subsequently we come upon the idea that "attention centres in the fact of sensible clearness." The one definition seems always to imply and employ the other, in whatever order one proceeds. Titchener's answer to this is that "you cannot say much about a thing you regard as ultimate to your science."[19] And if attensity, or attention, are both or either ultimate, they may thus forever escape definition. But the fact remains that, for this very reason, the definition of attention in terms of attensity can never of itself compel acceptance.

If attensity is really indefinable and only demonstrable, perhaps the argument from attensity to attention would be apparent from Titchener's examples of the phenomenon. But, with respect to many such illustrations, the logical sequence is no better. For instance, no one can have read the exposition of attention in the *Textbook* without recalling the space there devoted to the picture puzzle as evidence of the shift of attention. The particular puzzle is a drawing of the human brain, the convolutions of which, on inspection, turn out to be cleverly designed little babies.

> Look at it, and try to find what it conceals. While you are searching, the whole drawing is at the upper level of consciousness, and the rest of your experience is at the lower level. Suddenly you find what you are looking for: and what happens? Why the moment you do so, the picture of the brain drops clean away from the

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[17] *Feeling and Attention,* 182.

[18] *Ibid.,* 188.

[19] Titchener, The psychological concept of clearness, *Psychol. Rev.,* 24, 1917, 55.

upper level: the concealed outlines stand out with all imaginable clearness, and the form of the brain is no clearer than the feel of the book in your hand.[20]

The Gestalt-theorists have already dealt adversely with the hypothesis that such shifts in equivocal perceptions are shifts of attensity.[21] Their criticism rests on the following very reasonable point. In the case of the brain-drawing, for example, the original perception is not one of clear brain-form and unclear baby-form. The baby-form is simply not there to begin with, and hence it cannot be assigned any degree of attensity except zero—which is synonymous with non-existence. The assertion that "concealed" forms are usually really non-existent is supported empirically by evidence showing that when they are no longer "concealed," there is a distinct break for recognition in the identity of the stimulus.[22] Unless the structuralist can reply with contrary experimental evidence, the configurational argument removes equivocal perceptions from the domain of attention-*qua*-attensity.

Or, again, Titchener treated of range of attention in the *Text-book*. We measure range, he tells us, by presenting visually, *e.g.* "a number of dots, lines, letters, numerals, simple geometrical figures, bands of colors, *etc.*"[23] But Professor Dallenbach has already pointed out the inconsistency between the measurement of attention where such stimuli as these must be cognized, and the non-cognitive nature of attensity. Thus it is that, although we owe the formulation of the theory of attention and attensity to Titchener, his own treatment of the subject varies extraordinarily between what is consistent with that doctrine and what is the proper study of cognitive clearness.

I have criticized the Titchenerian dogma on the scores of its disregard of the pre-psychological formulation of attention and of independent psychological authority, of its internal logical circularity, and of its inconsistence with purported examples and measurement of attention. Let us now occupy ourselves with an examination of the fruits of this doctrine.

On the side of experimental productivity, the consequences have been negligible. More than twenty years of opportunity for research since the firm establishment of the doctrine have produced almost no new knowledge about attention-*qua*-attensity beyond the laws originally stated by Titchener. The exceptions seem to me to be only three in number.[24] The first is the evidence of Geissler[25] for the multiple-level structure of the attentive consciousness (in, perhaps, the case of only certain observers); the second is Professor Dallenbach's contrary evidence for a dual-level structure[26] (again, perhaps, for certain observers); and the third is Professor Dallenbach's evidence for extending the constant of the range of attention to a much larger number than the traditional six.[27] And, as respects the security of these three demonstrations, the first and second are obviously in conflict, and the third has been shown by Fernberger (as we shall later observe) to involve a method of measurement which defeats its own ends. It would be distinctly unfair not to mention the series of *Cornell Studies on Conditions of Attention*[28] now in progress; but these are rather more supplements and refinements of Titchener's original laws than demonstrations of new principles.

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[20] *Text-book*, 277.

[21] This objection has been reëmphasized lately by H. Helson, Studies in the theory of perception. I. The clearness-context theory, *Psychol. Rev.*, 39, 1932, 44-72.

[22] Cf. W. Köhler, *Gestalt Psychology*, 1929, 205-215, and E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 28.

[23] *Text-book*, 287.

[24] I wish to stress my belief that these are the only important studies whose contributions to the theory of attention-*qua*-attensity might not equally well be regarded as contributions to a theory of attention-*qua*-cognitive-clearness.

[25] L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 524ff.

[26] K. M. Dallenbach, Attributive *vs.* cognitive clearness (note 1 *supra*).

Or, again, the theoretical consequences of the Titchenerian doctrine have lagged. The forceful utterance of a doctrine in psychology usually prepares the way for a series of attempts to correlate the variables of that theory with physiological variables. Of the original five dimensions of experience, considerable progress has been made in the tying down of four to cerebral and recepto-neural events.[29] And, if our knowledge of the physiology of intensity, quality, extensity, and duration is still defective, the lacunae are at least subject to competent and enthusiastic speculation. But attensity alone of the equally primitive dimensions seems not to have aroused the discussion or creation of physiological hypotheses that its alleged importance would warrant. The student who inquires after the possible cerebral basis of attensity may quite safely be sent back to Titchener to find that attensity has, perhaps, something to do with "reënforcement" and "inhibition."[30] He will not be able to discover why the results of reënforcement and inhibition are *at*tensive changes, and not, as would seem more likely, *in*tensive changes. But he will at least have consulted the latest word on the matter!

A particularly unhappy development is the apparent impasse which has been reached in the measurement of the range of attention. Strict Titchenerian tenets required, of course, that the older methods involving calculations of the number of objects which could be cognized should be discarded; and it was necessary, therefore, to devise some method whereby it could be determined how many conscious contents can simultaneously share high attensity, irrespective of their clearness. Gill and Dallenbach[31] selected for this purpose the method of constant stimuli. Groups of simple stimuli of various numbers were presented to the observer, and he was required to report whether all, or only some, of the resulting impressions were at the upper level of attensity; from the data the upper limen (in terms of number of stimuli) for the uni-level effect was calculated, and employed as a measure of the range of attention.

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[27] N. F. Gill and Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 247-256; A. D. Glanville and Dallenbach, The range of attention, this JOURNAL, 41, 1929, 207-236.

[28] A complete list of these Studies will be found in A. M. White and Dallenbach, this JOURNAL, 44, 1932, 175.

[29] Summaries of this progress have been provided most recently by E. G. Boring, The physiology of consciousness, *Science*, 75, 1932, 32-39, and L. T. Troland, *The Principles of Psychophysiology*, 1931-32, especially vols. I (Sensation) and III (Cerebration). It seems to me very much to the point that Troland has, furthermore, a good deal of much interest to say about the possible physiology of cognitive clearness, around which his discussion of attention is built (Vol. III, 137-154), whereas he has not similarly speculated on the physiology of Titchenerian attensity.

[30] The most explicit of Titchener's treatments of the physiological basis of clearness and attention is in his article, Attention, *New International Encyclopedia*, 2nd ed., 1923, II, 342.

[31] *Op. cit.*

Ingenious as this method is, it failed of its purpose. For, as Fernberger has pointed out, what we are here measuring is simply the number of stimuli which statistically is adequate to a uni-level impression, and not the number of *conscious contents* which can concurrently occupy the upper level—whereas the latter measure is the one with which questions of range are concerned. "What assurance," Fernberger asks, "did Gill and Dallenbach have that the *O*s *saw* all of the elements on a given card? This is apparently assumed. It has been our experience that, with exposures as rapid as 60 sigma, many elements may be on a card which are not seen at all. My own and Oberly's findings indicate that this happens when stimulus cards contain only 3 or 4 elements."[32]

The essence of Professor Dallenbach's reply to this criticism is: "Truly we have no such assurance, and what is more to the point we did not care whether our *O*s saw all the elements, half of them, or only a small fraction of them. All the elements had the same opportunity of being seen; beyond that our interest did not go. The question whether all the items were seen no more concerned us than the question whether all the grams in a 100-grm. weight were sensed would concern Fernberger in a lifted-weight experiment. The method of constant stimuli, as Fernberger knows, is adequate to such subjective variables. What we were solely interested in was to discover, by the method of constant stimuli, the number of dots that would correlate 50% of the time with one level of clearness."[33]

This reply seems clearly unsatisfactory; the analogy suggested is hardly proper, and the whole statement does not make of the experiment the measure of range which Fernberger demands. The most unfortunate aspect of the matter is not that this particular experiment failed to measure the range of attention-*qua*-attensity, but that it demonstrates the strong likelihood (also noted by Fernberger) that such a measure can *never* be obtained by the known psychophysical methods. The reason for this is that in order to determine the answer to the question of how many conscious items may occupy the upper level, the observer must count or otherwise identify the items—and hence the measure will involve cognitive clearness. Thus the very mechanism of measurement substitutes clearness for attensity.

In fact, this point is capable of significant extension beyond the particular case of a quantitative psychophysical experiment. *Any sort of report whatsoever on attensity is demonstrably subject to the limitations of cognitive clearness.* For there is ultimately no difference in this respect between a report which says "this item of experience is circular, or square, or a letter of the alphabet" and a report which says "this item of experience is attense." The first report involves cognition, but so does the second—the cognition of attensity. Whoever would maintain that, for the purposes of psychology, certain aspects of experience are handled free of cognitive interpretation is at variance with the—almost painfully learned—consensus of contemporary science. And attensity is naturally no exception. It may have been thought an exception in the hey-day of the sensation; but with the realization that, as Koffka puts it,[34] the sensation is not prior to meaningful organization, but actually a product of one such organization, not even the attributes of sensation can escape cognitive interpretations. Professor Dallenbach regards attensity as one of the conditions of (cognitive) clearness.[35] That is very likely; but we now need to add that for practical purposes (that is to say, for the purposes of empirical psychology), clearness is a necessary condition of the reportable knowledge of attensity. And, consequently, he who would discuss attensity with no thought to cognition may well take pause.

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[32] S. W. Fernberger, The 'range of attention' experiment, this JOURNAL, 38, 1927, 478.

[33] K. M. Dallenbach, Dr. Fernberger on the 'range of attention' experiment, this JOURNAL, 38, 1927, 481.

[34] K. Koffka, Zur Theorie der Erlebnis-Wahrnehmung, *Annal. d. Philos.*, 3, 1923, 396: "Dann ist, in weiterem Sinn, die Analyse auch ein Gestaltungsvorgang. Denn

Another difficulty at which the doctrine of attention-in-terms-of-attensity has arrived is that of sharp divergences in the interpretation of published experimental results; the peculiar idefinability of attensity has not made it a happy topic of discussion. An example will suffice: namely, the controversy between Professor Dallenbach and Wever over the latter's experiment on the figure-ground perception. Of this controversy the salient point is Professor Dallenbach's conviction that Wever "does not know his clearnesses"[36] because his protocols are inadequate to a demonstration of the difference between cognitive and attributive clearness. The accusation might also be levelled against Wever's observers with equal justification. The inescapable conclusion is that some observers can reliably observe attensity and others can not. Professor Dallenbach's observers seem to fall into the first class and Wever's into the second; although it must be pointed out that even Professor Dallenbach's observers in 1920 failed to report attensity in almost one-fifth of their observations, despite instructions.[37] A phenomenon which varies thus at the hands of observers of different laboratories makes inevitably for anything but scientific accord and security. One cannot read without a certain sympathy Crosland's recent lament[38] over the possibilities for almost personal misunderstanding which lie very near the surface of this field of discussion.

Finally, it may be asked of the doctrine of attention and attensity, whether it has in the years since its conception shown any really essential relevance to the rest of psychology—such relevance, for instance, as would justify Titchener's notion of its fundamental importance. The answer here is strongly in the negative. We can demonstrate this truth by considering the present status of two fields of psychology in which, according to Titchener, attention was of large importance. The first field is that of psychological analysis; and here we have already remarked that the products of analysis are now generally considered to be particular meaningful *Gestaltungen*, and, therefore, in a strong sense, akin to the phenomenon of equivocal perception, in that they represent restructurings in the presence of constant stimuli. Here is no indication of the rôle of attensity, any more than in the case of the puzzle-picture. Here is rather a fact of structure and cognition, and, in fine, of clearness as we have defined it—the adequacy of experience to preconceived categories of report. The

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indem wir die Teile aus dem Ganzen Lösen, bekommen sie nun eine eigene Gestalt. Unter diesem Gesichtspunkt ist also die Empfindung nicht etwas dem Gestaltungsprozess vorhergehendes, sondern ein höchstes Erzeugnis einer besonderen Gestaltung."

[35] Dallenbach, this JOURNAL, 40, 1928, 341.

[36] *Ibid.*, 340.

[37] Dallenbach, *op. cit.*, 1920, 200, Table IV. If one excludes the experimenter's own observations from this table, retaining only the two *O*s who worked without knowledge of the problem, the percentage of reports which omit mention of attensity rises to 23.7.

[38] H. R. Crosland, Letter-position effects in the range of attention experiment, etc., *J. Exper. Psychol.*, 14, 1931, footnote 17 on pp. 501ff.

second field is that of memory. Focality in attention was, in Titchenerian psychology, an important condition for association. And it still is, if attention be defined in terms of clearness; for that which is cognized as a unit persists as a unit, to which the configurational work on memory bears testimony. Translate 'focality in attention' into 'attenseness,' and where is the evidence for such a law, devoid of cognitive implications? In these two fields the significant relation is to clearness in the cognitive sense. And yet the fields are those in which the Titchenerian doctrine of attention promised great implication.

Our criticism of the attensive theory of attention has now extended over a number of considerations. The positive contribution of the discussion issues from it quite simply. My suggestion is this: that *the study of attention would profit largely if we were to state the phenomenon of attention in terms of cognitive clearness.*

I venture to point out that a doctrine of attention, formulated in this way, would not only receive justification from the nature of the general pre-scientific problem to which it would correspond, but it would avoid the very same difficulties—logical and experimental—that we have seen embarrassing the Titchenerian dogma. More than that, it would embrace the results of much important work that at present is rigidly excluded from the attentional field or whose implications are obscured. We should see, for instance, that the bond between the abstraction-experiment and the theory of attention is of the closest; for the results on abstraction would be sensed as showing the dependence of clearness upon particular *Aufgaben.* We should see, again, that in Spearman's suggestion, for example, of a "constancy of cognitive output"[39] there is a broad hint toward a theory of range. And—what is of no little desirability—we should be able to avoid the confusing and superfluous trichotomizing of the problem of range into ranges of "attention," "cognition," and "apprehension,"[40]—a process which imposes an unnecessary and idle complication of terminology upon what deserves to be an orderly, intact field of investigation.

What I am suggesting is a retrogression—but a retrogression in definition only, not in knowledge. Its goal lies very near to what we can conceive the original problem of attention to have been. The psychologist may naturally be cautious in accepting the counsel of popular introspection. But granting him that caution, there is yet a wealth of historical evidence (as in the present instance) to demonstrate that a provocative problem inherited from lay observation may turn sterile under too artificial cultivation. Does it not appear more than likely that a reformulation of the theory of attention in terms of cognitive clearness would unify a fragmentary field of psychology and endow it with fresh implication?

Harvard University DWIGHT W. CHAPMAN

PORTRAITS USEFUL TO THE PSYCHOLOGIST

The following catholic list of portraits is published for the convenience of teachers and lecturers who may wish, particularly when dealing with the history of psychology or with the contemporary schools, to show pictures of the men who have founded schools, have led movements, or have taken a prominent part in the establishment or furtherance of a point of view. The portraits, besides showing the manner of the man with whom the speaker is dealing, serve to lighten an exposition which is apt to be heavy, and to give visual as well as auditory cues to the principal actors in the story. These advantages, however, are frequently lost because of the difficulty experienced in obtaining pictures of the men desired. Though photographs have been published of most of the important men in psychology, it often requires long and patient search to find the picture of a given individual. This difficulty will, we believe, be reduced to a great extent by the present list.

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[39] C. Spearman, *The Nature of 'Intelligence' and the Principles of Cognition,* 1923, 131.

[40] H. S. Oberly, The range for visual attention, cognition, and apprehension, this JOURNAL, 35, 1924, 332-353; Further results on the range for visual attention, cognition, and apprehension, *ibid.,* 37, 1926, 132-138.

All the portraits found in the books and periodicals of the Psychological Library of Columbia University are given here. Different pictures of the same individual are listed in chronological order and are numbered consecutively; copies are listed after the original without numbers.[1] For example; only three of the five portraits of Alfred Adler are different. These three are numbered and listed chronologically. The second (2) of these was reproduced in two other books. These reproductions are given without numbers and in chronological order after (2)—the original.

Abraham, Karl (1876-1925). *Int. Zsch. f. Psychoanal.,* 12, 1926, frontispiece no. 2.

Adler, Alfred (1870——). (1) G. S. Hall, *Life and Confessions,* 1923, 217. (2) *Wittenberg Symposium,* 1928, 3; W. B. Pillsbury, *The History of Psychology,* 1929, facing 253; *Psychologies of 1930,* xiv. (3) *Int. Zsch. f. indiv. Psychol.,* 8, 1930, frontispiece (with signature).

Angell, James Rowland (1869——). *Scient. Mo.,* 29, 1929, 380.

Arps, George Frederick (1874——). *Encycl. Psychol.,* 1928, 38.

Aveling, Francis (1875——). *Wittenberg Symposium,* 1928, 3.

Bain, Alexander (1818-1903). J. Sully, *My Life and Friends,* 1918, facing 86.

Baldwin, James Mark (1861——). *A History of Psychology in Autobiography,* I, 1930, xiv.

Becher, Erich (1882-1929). *Arch. f. d. ges. Psychol.,* 68, 1929, frontispiece (with signature).

Bekhterev, Vladimir M. (1857-1927). (1) *Festschrift für Bekhterev,* 1926, frontispiece (with signature). (2) L. F. Grote, *Die Medizin im Selbstdarstellungen,* 1927, facing 1 (with signature); *Wittenberg Symposium,* 1928, 3; *Quar. Rev. Biol.,* 3, 1928, facing 505. (3) *Scient. Mo.,* 26, 1928, 444.

Bell, Charles (1774-1843). F. Fearing, *Reflex Action,* 1930, facing 112.

Bentley, Madison (1870——). (1) *Psychologies of 1925,* facing 383; *Ped. Sem.* 33, 1926, following 152. (2) *Wittenberg Symposium,* 1928, 3. (3) *Psychologies of 1930,* xiv.

Bianchi, Leonardo (1848-1927). *Ment. Hygiene,* 14, 1930, suppl. following 462.

Binet, Alfred (1857-1911). (1) *Année psychol.,* 18, 1911, frontispiece (with signature); *Ment. Hygiene,* 14, 1930, suppl. following 462. (2) *Anné psychol.,* 18, 1911, facing 15 (with signature). (3) H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 4.

Blondel, Charles Aimé Alfred (1876——), *Psyche,* 6, 1926, frontispiece no. 24.

Boring, Edwin Garrigues (1886——). (1) *Psychologies of 1930,* xiv. (2) *Scient. Mo.,* 32, 1931, 90.

Bourdon, Benjamin Bienaimé (1860——). *History of Psychology in Autobiography,* II, 1932, xiv.

Braid, James (1795-1860). *Scient. Mo.,* 29, 1929, 157.

Brentano, Franz (1838-1917). *Amer. J. Psychol.,* 36, 1925, facing 469 (with signature).

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Brett, George Sidney (1869——). (1) *Wittenberg Symposium,* 1928, 4; *Psychologies of 1930,* xiv.

Breuer, Joseph (1842-1925). G. S. Hall, *Life and Confessions,* 1923, 217.

Brodmann, Korbinian (1868-1918). *J. f. Psychol. u. Neurol.,* 24, 1919, facing i (with signature).

Bühler, Karl (1879——). *Wittenberg Symposium,* 1928, 4.

Cajal, Santiago Ramón y (1852——). (1) *Clark Univ. Decentennial Celebration,* 1899, facing 311 (with signature); *Mental Hygiene,* 14, 1930, suppl. following 462. (2) *Scient. Mo.,* 31, 1930, 179.

Calkins, Mary Whiton (1863-1930). *A History of Psychology in Autobiography,* I, 1930, xiv.

Cannon, Walter Bradford (1871——). *Wittenberg Symposium,* 1928, 4.

Carr, Harvey A. (1873——). (1) *Wittenberg Symposium,* 1928, 4; *Quart. Rev. Biol.,* 3, 1928, facing 505. (2) *Psychologies of 1930,* xv.

Cattell, James McKeen (1860——). (1) *Wittenberg Symposium,* 1928, 12; H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 198. (2) *Scient. Mo.,* 28, 1929, 25.

Charcot, Jean Martin (1825-1893). (1) G. S. Hall, *Life and Confessions,* 1923, 216; *Scient. Mo.,* 29, 1929, 159. (2) *Ment. Hygiene,* 14, 1930, suppl. following 462.

Claparède, Edouard (1873——). (1) *Wittenberg Symposium,* 1928, 5. (2) *A History of Psychology in Autobiography,* I, 1930, xiv.

Coué, Emile (1857-1926). *Scient. Mo.,* 29, 1929, 159.

Dewey, John (1859——). *Psychologies of 1930,* xv.

De Sanctis, Sante (1863——). *Psyche,* 6, 1926, facing 56, no. 21.

Dodge, Raymond (1871——). *A History of Psychology in Autobiography,* I, 1930, xiv.

Drever, James (1873——). *A History of Psychology in Autobiography,* II, 1932, xiv.

Dunlap, Knight (1875——). (1) *Psychologies of 1925,* facing 309; *Ped. Sem.,* 32, 1925, following 386; *Wittenberg Symposium,* 1928, 5. (2) *Psychologies of 1930,* xv; *A History of Psychology in Autobiography,* II, 1932, xiv.

Ebbinghaus, Hermann (1850-1909). (1) *Zsch. f. Psychol.,* 51, 1909, frontispiece (with signature); H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 50; *Amer. J. Psychol.,* 42, 1930, facing 505 (with signature).

Fechner, Gustav Theodor (1801-1887). (1) G. S. Hall, *Founders of Modern Psychology,* 1912, facing 125; H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 270.

Fernald, Walter Elmore (1859-1924). (1) *Ment. Hygiene,* 9, 1925, frontispiece; *J. Psycho-Asthenics,* 30, 1925, facing 16; *Ment. Hygiene,* 14, 1930, suppl. following 462; S. P. Davies, *Social Control of the Mentally Deficient,* 1930, facing 24.

Ferrari, Giulio Cesare (1868——). (1) *Ment. Hygiene,* 14, 1930, suppl. following 462; *A History of Psychology in Autobiography,* II, 1932, xiv.

Ferrier, David (1843-1928). *J. Ment. Sci.,* 74, 1928, facing 375.

Flugel, John Carl (1884——). *Psychologies of 1930,* xv.

Forel, August (1848——). (1) *Clark Univ. Decentennial Celebration,* 1899, facing 409 (with signature). (2) *J. f. Psychol. u. Neurol.,* 13, 1908, frontispiece (with signature). (3) L. R. Grote, *Die Medizin im Selbstdarstellungen,* 1927, (with signature). (4) *Psyche,* 8, 1928, frontispiece no. 32.

Franz, Shepherd Ivory (1874——). *A History of Psychology in Autobiography,* II, 1932, xv.

Freud, Sigmund (1856——). (1) G. S. Hall, *Life and Confessions,* 1923, 335. (2) F. Wittels, *Sigmund Freud,* 1924, frontispiece (with signature). (3) *Int. Zsch. f. Psychoanal.,* 12, 1926, frontispiece no. 3. (4) *Psyche,* 6, 1926, frontispiece no. 23; W. B. Pillsbury, *The History of Psychology,* 1929, facing 253. (5) *Ment. Hygiene,* 14, 1930, suppl. following 462.

Galton, Francis (1822-1911). (1-48) K. Pearson, *Life, Letters and Labours of Francis Galton,* I, 1914; II, 1924; IIIA, IIIb, 1930 (numerous portraits, and several silhouettes and sketches made at ages of 8-88 years; some reproduced with signature). (49) *Scient. Mo.,* 30, 1930, 124; H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 170.

Gault, Robert Harvey (1874——). *Wittenberg Symposium,* 1928, 5.

Golgi, Camillo (1843-1926). *J. Ment. Sci.,* 72, 1926, facing 453.

Götz, Martius (1853-1928). *Arch. f. d. ges. Psychol.,* 61, 1928, facing 273 (with signature).

Groos, Karl (1861——). *A History of Psychology in Autobiography,* II, 1932, xv.

Hall, Marshall (1790-1857). F. Fearing, *Reflex Action,* 1930, facing 123.

Hall, G(ranville) Stanley (1844-1924). (1) *Amer. J. Psychol.,* 14, 1903, facing 267 (with signature). (2) G. S. Hall, *Life and Confessions,* 1923, frontispiece (four pictures at ages of 6, 14, 22 and 35 years). (3) *Ibid.,* 50 (four pictures at ages of 6, 14, 22 and 35 years). (4) *Ibid.,* 99 (age 45). (5) *Ibid.,* 335 (with group of psychoanalysts). (6) *Amer. J. Psychol.,* 35, 1924, facing 313 (with signature); L. Pruette, *G. Stanley Hall,* 1926, frontispiece. (7) *Scient. Mo.,* 30, 1930, 116.

Hartmann, Eduard von (1842-1906). G. S. Hall, *Founders of Modern Psychology,* 1912, facing 181.

Helmholtz, Hermann L. F. von (1821-1894). (1) G. S. Hall, *Founders of Modern Psychology,* 1912, facing 247; *Life and Confessions,* 1923, 217. (2) *Helmholtz's Treatise on Physiological Optics,* Eng. trans., I, 1924, frontispiece (with signature).

Hervieu, Paul (1857-1915). *Année psychol.,* 10, 1904, frontispiece.

Heymans, Gerardus (1857-1930). *A History of Psychology in Autobiography,* II, 1932, xv.

Hobhouse, Leonard Trelawney (1864-1929). *Quar. Rev. Biol.,* 3, 1928, facing 502.

Høffding, Harald (1843-1931). (1) *A History of Psychology in Autobiography,* II, 1932, xv. (2) *Harold Høffding in Memoriam,* 1932, frontispiece (with signature). (3) *Arch. f. d. ges. Psychol.,* 86, 1932, facing 289 (with signature).

Hoisington, Louis Benjamin (1883——). *Wittenberg Symposium,* 1928, 5.

Howard, Delton Thomas (1883——). *Wittenberg Symposium,* 1928, 6.

Hunter, Walter S. (1888——). (1) *Psychologies of 1925,* facing 83; *Ped Sem.,* 33, 1926, following 152. (2) *Psychologies of 1930,* xvi.

Jaensch, Erich (1883——). *Wittenberg Symposium,* 1928, 6.

James, William (1842-1910). (1) *Essays: Philosophical and Psychological in Honor of Wm. James,* 1908, frontispiece. (2) J. Sully, *My Life and Friends,* 1918, facing 316. (3) H. James, *The Letters of Wm. James,* 1920, I, frontispiece (age 65). (4) *Ibid.,* 20 (age 18). (5) *Ibid.,* 86 (age 25). (6) *Ibid.,* II, frontispiece (age 53). (7) *Ibid.,* facing 134 (with Josiah Royce). (8) *Ibid.,* facing 160 (with Henry James). (9) *Ibid.,* facing 314 (with Henry Clement). (10) *Scient. Mo.,* 12, 1921, 382; 30, 1930, 114; R. H. Gault and D. T. Howard, *Outline of General Psychology,* 1925, frontispiece. (11) G. S. Hall, *Life and Confessions,* 1923, facing 183. (12) R. C. Givler, *Psychology,* 1922, facing 252; W. B. Pillsbury, *The History of Psychology,* 1929, frontispiece. (13) M. Münsterberg, *Hugo Münsterberg,* 1922, facing 132.

Janet, Pierre (1859——). (1) *Psyche,* 7, 1926, frontispiece no. 26; *Wittenberg Symposium,* 1928, 6; *Psychologies of 1930,* xvi. (2) *A History of Psychology in Autobiography,* I, 1930, xv.

Jastrow, Joseph (1863——). (1) *Wittenberg Symposium,* 1928, 6. (2) *A History of Psychology in Autobiography,* I, 1930, xv.

Jennings, Herbert Spencer (1868——). *Quar. Rev. Biol.,* 3, 1928, facing 502.

Jones, (Alfred) Ernest (? ——). *Int. Zsch. f. Psychoanal.,* 15, 1929.

Jørgensen, Carl Aage Valdemar (1888——). *Wittenberg Symposium,* 1928, 7.

Judd, Charles Hubbard (1873——). *A History of Psychology in Autobiography,* II, 1932, xvi.

Katz, David (1884——). *Wittenberg Symposium,* 1928, 7.

Kelley, Truman Lee (1884——). (1) *Scient. Mo.,* 28, 1929, 39; *Psychologies of 1930,* xvi.

Kiesow, Friedrich (1858——). (1) *Arch. f. d. ges. Psychol.,* 65, 1928, frontispiece (with signature); *Wittenberg Symposium,* 1928, 7. (2) *A History of Psychology in Autobiography,* 1, 1930, xv.

Kline, Linus Ward (1866——). *Quar. Rev. Biol.,* 3, 1928, facing 502.

Koffka, Kurt (1888——). (1) *Psychologies of 1925,* facing 129; *Ped. Sem.,* 32, 1925, following 566; *Psyche,* 8, 1927, frontispiece no. 30; *Psychologies of 1930,* xvi.

Köhler, Wolfgang (1887——). (1) *Psychologies of 1925,* facing 145; *Ped. Sem.,* 32, 1925, following 566; W. B. Pillsbury, *The History of Psychology,* 1929, facing 253; H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 248. (2) *Psychologies of 1930,* xvii.

Kornilov, Konstantin Nikolaevitch (1879——). *Psychologies of 1930,* xvii.

König, Arthur (1856-1901). *Zsch. f. Psychol.,* 27, 1902, facing 145 (with signature).

Kraepelin, Emil (1856-1926). (1) *Arch. f. d. ges. Psychol.,* 58, 1927, frontispiece (with signature); *J. Ment. Sci.,* 73, 1927, facing 361. (2) *Psychol. Arbeit.,* 9 1927, follows 358 (portrait of sculptured bust). (3) *Ment. Hygiene,* 14, 1930, suppl. following 462.

Krueger, Felix Emil (1874——). *Wittenberg Symposium,* 1928, 7.

Ladd, George Trumbull (1842-1921). *Scient. Mo.,* 30, 1930, 117.

Ladd-Franklin, Christine (1874-1930). *Scient. Mo.,* 30, 1930, 377.

Langfeld, Herbert Sidney (1871——). (1) *Wittenberg Symposium,* 1928, 8. *Scient. Mo.,* 34, 1932, 88.

Lashley, Karl Spencer (1890——). *Scient. Mo.,* 30, 1930, 571.

Lehmann, Alfred (1858-1921). *Arch. f. d. ges. Psychol.,* 42, 1922, frontispiece.

Lewes, George Henry (1817-1878). J. Sully, *My Life and Friends,* 1918, facing 262.

Lipmann, Otto (1880——) *Zsch. f. angew. Psychol.,* 36, 1930, frontispiece (with signature).

Loeb, Jacques (1859-1924). *Quar. Rev. Biol.,* 3, 1928, facing 488.

Lombroso, Cesare (1836-1909). *Ment. Hygiene,* 14, 1930, suppl. following 462.

Lotze, Rudolph Hermann (1817-1881). G. S. Hall, *Founders of Modern Psychology,* 1912, facing 65.

Lubbock, John (1834-1913). *Quar. Rev. Biol.,* 3, 1928, facing 488.

Ludwig, Karl (1816-1895). G. S. Hall, *Life and Confessions,* 1923, 217.

Maudsley, Henry (1835-1918). (1) *J. Ment. Sci.,* 64, 1918, facing 117; *Ment. Hygiene,* 14, 1930, suppl. following 462.

McDougall, William (1871——). (1) *Psychologies of 1925,* facing 273; *Ped. Sem.,* 33, 1926, frontispiece. (2) *Wittenberg Symposium,* 1928, 8; *Psychologies of 1930,* xvii. (3) *A History of Psychology in Autobiography,* I, 1930, xv.

Mesmer, Frederick Anton (1733-1815). *Scient. Mo.,* 29, 1929, 155.

Meumann, Ernst (1862-1915). (1) *Arch. f. d. ges. Psychol.,* 34, 1915, following 156. (2) *J. Educ. Psychol.* 6, 1915, 427.

Meyer, Adolf (1866——). *Ment. Hygiene,* 14, 1930, suppl. following 462.

Morgan, C(onwy) Lloyd (1852——). (1) *Quar. Rev. Biol.,* 3, 1928, facing 488; *A History of Psychology in Autobiography,* II, 1932, xvi.

Mosso, Angelo (1846-1910). *Clark Univ. Decentennial Celebration,* 1899, facing 383 (with signature).

Münsterberg, Hugo (1863-1916). (1) M. Münsterberg, *Hugo Münsterberg,* 1922, frontispiece (with signature). (2) *Ibid.,* facing 26 (with his students at Freiburg). (3) *Scient. Mo.,* 30, 1930, 120 (with signature).

Nafe, John Paul (1887——). *Psychologies of 1930,* xvii.

Paget, Richard (1869——). *Psyche,* 8, 1927, frontispiece no. 29.

Parker, George Howard (1864——). *Quart. Rev. Biol.,* 3, 1928, facing 502.

Pavlov, Ivan Petrovitch (1849——). (1) *Scient. Mo.,* 17, 1923, 394. (2) *Ibid.,* 620 (with son). (3) *Dedicated to Pavlov: 75th Jubilee,* 1924, frontispiece (with signature). (4) *Scient. Mo.,* 20, 1925, 330. (5) *Psyche,* 7, 1926, frontispiece no. 25. (6) I. P. Pavlov, *Lectures on Conditioned Reflexes,* Eng. trans., 1928, frontispiece (sketch, with signature). (7) *Ibid.,* facing 16 (three photographs, outdoors and at play). (8) *Ibid.*, facing 320 (at operating table). (9) *Ibid.*, facing 368 (in study). (10) *Scient. Mo.*, 26, 1928, 446. (11) *Quar. Rev. Biol.*, 3, 1928, facing 505; *Scient. Mo.*, 29, 1929, 283; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 78. (12) *Scient. Mo.*, 29, 1929, 378 (with H. Cushing). (13) F. Fearing, *Reflex Action*, 1930, facing 282. (14) *Psychologies of 1930*, xviii.

Pièron, Henri (1881——). (1) *Psyche*, 6, 1925, frontispiece no. 21. (2) *Wittenberg Symposium*, 1928, 8. (3) *Quar. Rev. Biol.*, 3, 1928, facing 505.

Pillsbury, Walter Bowers (1872——). (1) *Wittenberg Symposium*, 1928, 8; *A History of Psychology in Autobiography*, II, 1932, xvi.

Pinel, Phillippe (1745-1836). *Ment. Hygiene*, 14, 1930, suppl. following 462.

Prince, Morton (1854-1929). (1) *Psychologies of 1925*, facing 199; *Ped. Sem.*, 32, 1925, frontispiece. (2) *Wittenberg Symposium*, 1928, 9. (3) *J. Abn. & Soc. Psychol.*, 24, 1929, facing 249.

Reymert, Martin Luther (1883——). *Wittenberg Symposium*, 1928, 12.

Ribot, Theodule Armand (1846-1916). (1) *J. d. Psychol.*, 12, 1915, facing 260 (with signature). (2) R. H. Gault and D. T. Howard, *Outline of General Psychology*, 1925, facing 200.

Rignano, Eugenio (1870——). *Psyche*, 6, 1926, facing 40, no. 24.

Robertson, George Croom (1842-1892). J. Sully, *My Life and Friends*, 1918, facing 182.

Romanes, George John (1848-1894). *Quar. Rev. Biol.*, 3, 1928, facing 488.

Royce, Josiah (1855-1916). (1) H. James, *The Letters of Wm. James*, 1920, II, facing 134 (with Wm. James). (2) M. Münsterberg, Hugo Münsterberg, 1922 (with James and Palmer). (3) *Scient. Mo.*, 30, 1930, 118 (with signature).

Sander, Friedrich (1889——). *Psychologies of 1930*, xviii.

Sanford, Edmund C(lark) (1859-1924). *Amer. J. Psychol.*, 36, 1925, facing 157 (with signature).

Schniermann, Alexander Linowitsch (1899——). *Psychologies of 1930*, xviii.

Seashore, Carl Emil (1866——). (1) *Psychol. Monog.*, 39, 1928, frontispiece no. 178 (a telephotograph). (2) *Wittenberg Symposium*, 1928, 9; *A History of Psychology in Autobiography*, I, 1930, xvi.

Seguin, Edouard (1812-1880). S. P. Davies, *Social Control of the Mentally Deficient*, 1930, facing 38.

Sidgwick, Henry (1838-1900). J. Sully, *My Life and Friends*, 1918, facing 278.

Small, Willard Stanton (1870——). *Quar. Rev. Biol.*, 3, 1928, facing 502.

Sommer, Karl Robert (1864——). *Ment. Hygiene*, 14, 1930, suppl. following 462.

Southard, Elmer Ernest (1876-1920). (1) *Ment. Hygiene*, 4, 1920, frontispiece. (2) *Ibid.*, 14, 1930, suppl. following 462.

Spearman, Charles Edward (1863——). (1) *Wittenberg Symposium*, 1928, 9. (2) *Psychologies of 1930*, xviii; *A History of Psychology in Autobiography*, I, 1930, xvi.

Spencer, Herbert (1820-1903). J. Sully, *My Life and Friends*, 1918, facing 288.

Stern, Louis William (1871——). (1) *Wittenberg Symposium*, 1928, 9; *A History of Psychology in Autobiography*, I, 1930, xvi.

Stratton, George Malcolm (1865——). *Wittenberg Symposium*, 1928, 10.

Stumpf, Carl (1848——). *A History of Psychology in Autobiography*, I, 1930, xvi.

Sully, James (1842-1923). J. Sully, *My Life and Friends*, 1918, frontispiece.

Terman, Lewis Madison (1877——). (1) H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 16. (2) *A History of Psychology in Autobiography*, II, 1932, xvi.

Thorndike, Edward Lee (1874——). (1) *Teachers College Record*, 27, 1926, facing 459 (with signature). (2) *Quar. Rev. Biol.*, 3, 1928, facing 502; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 102.

Titchener, Edward Bradford (1867-1927). (1) *Studies in Psychology; Titchener Commemorative Volume*, 1917, frontispiece (with signature); *Wittenberg Symposium*, 1928, 12. (2) *Amer. J. Psychol.*, 38, 1927, facing 489 (with signature). (3) *J. General Psychol.*, 1, 1928, frontispiece; *Scient. Mo.*, 26, 1928, 282. (4) *Ibid.*, 30, 1930, 121.

TROLAND, LEONARD THOMPSON (1889-1932). *Psychologies of 1930*, xix.

VERWORN, MAX (1863-1921). *Quar. Rev. Biol.*, 3, 1928, facing 488.

VON FREY, MAX (1852-1932). *Scient. Mo.*, 34, 1932, 478.

WARD, JAMES (1843-1925). (1) *Amer. J. Psychol.*, 36, 1925, facing 449 (with signature). (2) R. H. Gault and D. T. Howard, *Outline of General Psychology*, 1925, facing 300.

WARREN, HOWARD CROSBY (1867——). (1) *Scient. Mo.*, 28, 1929, 39. (2) *A History of Psychology in Autobiography*, I, 1930, xvii.

WASHBURN, MARGARET FLOY (1871——). (1) *Amer. J. Psychol.*, 39, 1927, frontispiece (with signature). (2) *Wittenberg Symposium*, 1928, 10. (3) *Psychologies of 1930*, xix; *A History of Psychology in Autobiography*, II, 1932, xvii.

WATSON, JOHN B(ROADUS) (1878——). (1) *Psychologies of 1925*, facing 1; *Ped. Sem.*, 32, 1925, following 202. (2) *Psyche*, 7, 1927, frontispiece, no. 27 (with signature); *Quar. Rev. Biol.*, 3, 1928, facing 505. (3) W. B. Pillsbury, *History of Psychology*, 1929, facing 253; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 160.

WEISS, ALBERT PAUL (1879-1931). (1) *Wittenberg Symposium*, 1928, 10; *Psychologies of 1930*, xix.

WITMER, LIGHTNER (1867——). *Clinical Psychology: Studies in Honor of Lightner Witmer*, 1931, frontispiece.

WOODWORTH, ROBERT SESSIONS (1869——). (1) *Psychologies of 1925*, 111; *Wittenberg Symposium*, 1928, 11; *Ped. Sem.*, 33, 1926, frontispiece. (2) H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 134; *Psychologies of 1930*, xix; *A History of Psychology in Autobiography*, II, 1932, xvii.

WUNDT, WILHELM MAX (1832-1920). (1) G. S. Hall, *Founders of Modern Psychology*, 1921, facing 311. (2) *Scient. Mo.*, 11, 1920, 383; 30, 1930, 122 (with signature). (3) *Arch. f. ges. Psychol.*, 40, 1920, i (death-bed sketch). (4) *Ibid.*, ii (bronze plaque); E. G. Boring, *A History of Experimental Psychology*, 1929, frontispiece (bronze plaque). (5) R. H. Gault and D. T. Howard, *Outline of General Psychology*, 1925, facing 400. (6) *Psychol. Rev.*, 28, 1931, facing 153 (with signature). (7) *Amer. J. Psychol.*, 44, 1932, facing 615 (with signature). Cf. *ibid.*, 32, 1921, 177.

YERKES, ROBERT MEARNS (1876——). (1) *Quar. Rev. Biol.*, 3, 1928, facing 505. (2) *A History of Psychology in Autobiography*, II, 1932, xvii.

ZELLER, EDUARD (1844-1908). G. S. Hall, *Founders of Modern Psychology*, 1912, frontispiece.

ZIEHEN, THEODOR (1862——). *A History of Psychology in Autobiography*, I, 1930, xvii.

ZWAARDEMAKER, HENRICK (1857-1930). *A History of Psychology in Autobiography*, I, 1930, xvii.

We limited our search for portraits to the Columbia psychological library because we had to set ourselves a finite task, and also because the library, which is large, probably includes all the books and periodicals that are available to most psychologists. If any omissions are brought to our attention, we shall be glad to rectify them in the supplementary lists which we hope to publish from time to time.

Columbia University L. V. BERGMAN
KARL M. DALLENBACH

DR. WILCOX ON "THE RÔLE OF FORM IN PERCEPTION"

In the July number of this JOURNAL,[1] Dr. Wilcox criticized the conclusions reached by Fehrer and myself regarding the rôle of form at the lower threshold of vision.[2] We found that it required 15 times more illumination to perceive form, even though the form seen did not coincide with the stimulus-form, than to report on the mere presence of light, and 25 times more illumination to name the stimulus-form correctly than was required to perceive the vague haze characteristic of liminal vision. We were unable to demonstrate the superiority of any one form over any other according to numerous criteria of goodness since no one form ranked first in a majority of the tests. We did not find that the circle possessed a lower threshold than other forms or that it was superior according to other criteria. Dr. Wilcox found that his *O*s "invariably" reported their liminal experiences shaped as circles. He believes that forms are first and primary in experience and that the perception of light at the threshold is always of a circular form. He implies that circles should have the lowest thresholds, should be reported first when any form at all is perceived, and hence should be the best of figures in perception. Dr. Wilcox's criticisms of our work depend (1) upon questions of fact, and (2) upon misinterpretations of our position as well as a neglect of the evidence we offered in support of our conclusions.

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[1] W. W. Wilcox, Helson and Fehrer on "The rôle of form in perception," *op. cit.*, 44, 1932, 578-580.

Are forms, circles, perceived at the lower threshold of vision? Our findings were not unique: they corroborate the work of Poppelreuter, Gelb and Goldstein, with abnormal subjects, and Seifert in vision, that of Zigler and his students both in vision and on the skin, and that of Kleitman and Blier in vision.[3] A writer of nearly 30 years back may be regarded as impartial. Piper pointed out in her well-known work on dark adaptation that form perception is lacking at the lower threshold. Instead of form, she says, "Man hat vielmehr den Eindruck eines unbestimmt umgrentzen schwachen Lichtnebels, der manchmal nicht ohne weiteres von den ganz ähnlich sich bietenden subjektiven Lichterscheinungen unterschieden werden kann."[4] If Dr. Wilcox choses to call such experiences formed it is evident he is using the term form as no other writers, even some configurationists included, use it. For him anything and everything is formed and the word loses all significance both in this and any other discussion of form. We were criticizing a view of the efficacy of form which employed the more ordinary meaning of the word. Dr. Wilcox may use words as he likes but he should realize it was not his meaningless term that we were concerned with.

Dr. Wilcox accuses us of the stimulus-error in attempting to account for our results.[5] Since no particular form was consistently preferred in our experiments while in his the circle was invariably perceived it does not require much thought to decide whose *O*s were biased. We avoided the stimulus-error by requiring our *O*s to report what they first saw without regard to the stimulus being exposed. Dr. Wilcox not only neglects the measurements given for the threshold values of the various figures we used but also seems to be unacquainted with our actual mode of procedure, all of which was fully explained in the article which he criticizes.

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[2] H. Helson and E. V. Fehrer, The rôle of form in perception, this JOURNAL, 44, 1932, 79-102.

[3] A brief résumé of the work of these people, with the exception of Seifert, was given in our article, *op. cit.*, 83 f. Seifert's article will be found in *Zsch. f. Psychol.*, 78, 1917, 55-144.

[4] H. Piper, *Über Dunkeladaptation, ibid.*, 31, 1903, 187. Piper's reference to form is unequivocal: "Es ist nicht mehr möglich unter solchen Bedingungen die *Form* des Erregers zu erkennen."

[5] Wilcox, *op. cit.*, 579.

The only evidence our critic has offered to support his contentions are the reports of his *O*s gained, so far as his published statements go, by the simple expedient of asking them to report what they saw near the threshold. There can be no scientific discussion of an issue merely hinging upon the reports of one set of *O*s as against those of another. Our conclusions were based upon measurements and several methods of calculation designed to bring out the various properties a form might have. In none of them did the circle take first place or even second. Can Dr. Wilcox dismiss such facts as these with the mere assertion of stimulus-error? Why did the circle rank fourth among the figures reported as first seen if it is invariably present at the threshold? Why was it necessary to make a larger number of exposures for the circle than for rectangle, square, and triangle in order to get 50 correct reports of form if circles tend to be seen more readily than other figures? Why was the rectangle called correctly at a lower illumination than the circle if circles are invariably seen at the threshold? Why did not the circle give a lower threshold than the other figures if it is present coincidently with the first appearance of light? Why were three other figures reported as present phenomenally (without regard to figure actually exposed) more times than the circle if the circle typifies liminal experience? We are entitled to a consideration of such facts as these in any criticism of our work but Dr. Wilcox seems unaware of them.

One misstatement, a *tour de force,* requires correction. Dr. Wilcox states that we criticize the view "that perceptual and bodily responses of the organism *form* a whole, a unit, a configuration."[6] This is absolutely untrue. This criticism is wholly of his own making. We are in hearty accord with this proposition which must be sharply distinguished from the proposition, often made by the narrower group of configurationists, that form as such is first and primary in experience.

We do not believe that Dr. Wilcox's experiments and criticisms, based as they are upon a conception of form so broad as to be devoid of meaning, either impugn our conclusions or lend support to a generalization far too wide and sweeping in the light of all the evidence. Our work concerned the testing of an hypothesis under special conditions where it was found wanting. It is Dr. Wilcox's privilege to change the hypothesis but the facts stand.

Bryn Mawr College HARRY HELSON

THE EFFECTS OF DISTRACTION

After completing a series of experiments in 1918, Cassel and Dallenbach concluded that "the effect of 'distraction' upon the sensory reaction is equivocal. The distractor may inhibit, and lengthen the reaction; it may facilitate, and shorten the reaction; or it may become habitual, and have no effect at all."[1] Reports by their observers indicated that these discrepant results might be dependent upon the temporal relations of the distractor and upon the attitude of the observer during the distraction.

Writers of textbooks seem to have ignored these conclusions, in spite of the implications respecting the effects of distraction in general; and their challenge for further experimental work seems to have been neglected. To attempt an experimental verification, the writer trained 5 *Os* until they were able to perform at a high and constant rate the simple motor skill which serves as the basis for this part of the projected experiment. The *Os* were confronted with a board containing 100 metal pegs arranged to form a square; and they were instructed to remove these pegs one at a time and to place them as rapidly as possible in a like arrangement on a duplicate board. After a long training period, the *Os* engaged in experimental determinations in which 'distraction' and control runs were interspersed. In the former they were subjected to a continuous tone of 260 cycles, while the control runs were carried on without this distractor. (Since *E* was outside the room and invisible to the *Os* it was assumed that he did not constitute an important distractor.)

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[6] *Ibid.,* 578.

[1] E. E. Cassel and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 1918, 29, 129-143.

Data now available for a large number of trials fail to exhibit any statistically reliable difference between times for observers engaged in control series and the same observers in 'distracted' runs. This would seem to support the contention of Cassel and Dallenbach that continuous distractors are readily habituated. Although variations in attitude are reported, there is no evidence of co-variability of attitude and time required for a particular run. Apparently the conclusions of Cassel and Dallenbach may not be extended to cover 'motor' reactions. A later publicaiton will report our results with a task which involves judgment and which will, we hope, more closely approximate the conditions of a 'sensory' reaction.

Cornell University JOHN G. JENKINS

THE FORTIETH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The Fortieth Annual Meeting of the American Psychological Association was held at Cornell University, Ithaca, New York, on Thursday, Friday, and Saturday, September 8, 9, and 10, 1932. Registration figures show a total of 612 persons in attendance classified as follows: 121 members, 250 associate members, 31 newly-elected associates, and 210 persons not affiliated with the Association.

At the business meeting, 19 Associates were transferred to the status of Member and 193 Associate Members were elected. The Secretary announced that the Forty-first Annual Meeting will be held at the University of Chicago on September 7, 8, 9, 10, 11, 12, and 13, 1933. Professor Harvey A. Carr was appointed as a member of the Executive Committee for 1932-1933, and John E. Anderson, Franklin Fearing, and the Secretary were elected as the Program Committee for 1933. It was voted to amend Section 2, Article VIII, of the by-laws by adding the following sentence: "In case a husband and wife both hold membership, one may elect to receive the Psychological Bulletin instead of the abstract journal of the Association providing an additional payment of two dollars a year be made to the Treasurer when the annual subscription is paid." Newly-elected officers are: President, L. L. Thurstone; Directors, Edward A. Bott and Henry E. Garrett; Division of Anthropology and Psychology, N.R.C., Walter S. Hunter and Herbert Woodrow; S.S.R.C., L. L. Thurstone; A.A.A.S., Harvey A. Carr and Walter F. Dearborn. It was voted that the American Psychological Association, Inc., should apply for membership in the Inter-Society Color Council. Clarence E. Ferree, A. T. Poffenberger, and Forrest Lee Dimmick were appointed as delegates to the Inter-Society Color Council. The

Association voted to establish a new committee to be known as the Committee on Research in the Social Sciences consisting of the three representatives on the S.S.R.C., the President and the Secretary of the Association as *ex officio* members and two members to be appointed by the Council of Directors. R. S. Woodworth and John E. Anderson were appointed to complete the membership of this newly-established committee. Following acceptance, with thanks, of the report of the Committee on Psychology of the National Advisory Council on Radio in Education, it was voted that the Association express its appreciation for what the Committee has accomplished and especially to thank Dr. Walter V. Bingham for the effective way in which he planned and executed the nation-wide broadcasting of psychology lectures during 1931-1932.

In commemoration of the fortieth anniversary of the American Psychological Association, Joseph Jastrow, one of the founders of the Association and its first Secretary-Treasurer, spoke briefly concerning the events leading to the launching of the Association in 1892. A cablegram to the Directors of the Laboratory of Psychology of the University of Leipzig commemorating the one hundredth anniversary of the birth of Wilhelm Wundt and congratulating members of the Leipzig Laboratory for carrying on so worthily the work of their great master was authorized and transmitted while the meetings were in session.

Seventeen sessions for formal papers were scheduled and a total of 106 papers were presented. If one assumes that the program of the Association represents a cross-section of current psychological research, then it would appear that research with animals and young children as subjects is in the ascendency. Papers on mental tests as such were conspicuous by their relative absence. Nevertheless, it would be premature to conclude that research interest in mental testing is declining. Rather, this type of research acvtivity seems to be directed toward the measurement of personality and character traits as indicated by the nature of the papers included in the session on Personality and Character. In contrast with previous meetings, there were several papers on applied or technological psychology. The dominant characteristic of the program as a whole may be summarized by saying that the papers reflected a widespread interest in objective, behavioristic experimentation with emphasis upon physiological analyses of behavior.

Two of the seventeen sessions were devoted to clinical psychology. At the annual meeting of the Section on Clinical Psychology, Edgar A. Doll was elected Chairman and Richard H. Paynter was elected a member of the Executive Committee for 1932-1935. A total of 230 persons are now listed as being affiliated with the Section on Clinical Psychology.

On Friday afternoon at 4:30 P.M., a Round Table on *Radio in Education* was held with Paul S. Achilles as Chairman. Informal reports of research studies concerning the effectiveness of radio addresses were made. Announcement was made that details of researches in this field are being assembled by Dr. F. H. Lumley, in charge of radio research, Ohio State University.

On Thursday and Friday afternoons, the Department of Psychology of Cornell University was host at two delightful teas held in the Laboratory of Psychology, Morrill Hall.

On Friday evening, the Presidential Address, "Age and Human Ability" was delivered by Dr. Walter R. Miles. Pointing out that the science of psychology has shown special vigor in those regions where the age aspects of behavior are emphasized, Dr. Miles argued that life-age might well become the entity for study in psychology just as space-time is the subject for study in physics. It was also shown that the psychological study of the effect of age on human abilities has been largely confined to the first two or two and a half decades of life, thus leaving relatively untouched five or six decades of maturity and later maturity. Without minimizing the importance of previous studies on adults, Dr. Miles devoted his address chiefly to a general presentation and discussion of the extensive series of studies on later maturity which he has been directing at Stanford University. An outline picture of age changes in the mental abilities so far investigated was presented. In general it appears that sensori-motor functions show early and decided decline, although in some instances the decline, can be compensated for by other and more complex behavior. Abilities which can be designated as more nearly psychological or mental tend to reach their peak at later life ages. The results so far obtained suggest the possibility of constructing a psychological map of human life-age.

In commemoration of the occasion as being the Fortieth Annual Meeting of our Association, Dr. Miles prefaced his address by an appropriate expression of gratitude to the American pioneers in psychology who organized our Association in 1892, many of whom are still living.

To provide well-nigh perfect facilities for a scientific meeting at which 612 persons registered is an achievement which can be duplicated by few institutions in this country. This was possible at Cornell not only because of the physical lay-out but also because of meticulous planning and efficient executive action. Professor Bentley and his associates have earned the heartfelt gratitude of our membership.

University of Minnesota DONALD G. PATERSON

THE WUNDT CENTENARY

The Institute of Psychology of the University of Leipzig celebrated the centenary of the birth of Wilhelm Wundt on October 29 and 30, 1932. The actual centenary was the 16th of August. The celebration consisted of three main functions, a demonstration of the work being done in the Institute, a formal commemoration meeting, and a banquet with less formal tributes.

The laboratory demonstration was introduced by the Director, Professor Krueger who outlined the general policy of the laboratory. Professors Klemm, Vokelt, and the assistants showed the apparatus being used for the work carried on under their direction.

The commemoration meeting was held in the aula. A number of shorter tributes were made by university and city dignitaries and by representatives of psychological societies, then Professor Krueger presented an extended account of Wundt's contributions to psychology, and an estimate of his work and position in the world of science.

At a closing banquet many brief testimonials were given and greetings read from individuals and psychological organizations in all parts of the world. The resolutions of the American Psychological Association were especially emphasized.

On October 30, Fräulein Wundt invited those present to her home to see her father's collected works and manuscripts, note books, and various mementoes. This was one of the most significant events of the celebration.

W. B. PILLSBURY

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Purposive Behavior in Animals and Man. By EDWARD CHACE TOLMAN. New York, Century Co., 1932. Pp. xiv, 463.

"This book has grown out of an experimental interest in animal learning grafted upon an arm-chair urge towards speculation." The book is appropriately dedicated to the white rat (without whose services it could not have been written) but it might equally well have been dedicated to the speculative philosopher.

Purposive behaviorism is a complete system of psychology most closely related to configurationism and perhaps a sub-variety of the Gestalt doctrine. Purpose, cognition, expectation, and similar terms are used to describe behavior without any mentalistic implications. These terms characterize behavior as *molar* but not necessarily behavior as *molecular.* This distinction between the molecular and molar views of behavior is clearly drawn. The molecular view, referred to Watson, defines behavior in terms of underlying physical and physiological details. The molar view, presented by Professor Tolman, recognizes that behavior has properties of its own which are not deducible from physiological processes. Among these 'emergent' properties are purpose and cognition—"behavior as behavior reeks of purpose and of cognition (p. 12)."

The ultimate goals of behavior are physiological states of quiescence which are sought or disturbances which are avoided. The animal's behavior expresses subordinate demands and readinesses for specific goal-objects. Some goal-objects are demanded more than others. A shift of incentive demonstrates in the learning curve the great importance of the goal-object in the learning process. A great deal of important experimental work is summarized.

Purposive behaviorism concerns itself only with 'docile' behavior, *i.e.* with behavior which is modifiable as distinct from bare reflexes and tropisms. The leading theories of learning—the conditioned reflex theory, trial and error, the Gestalt theory—are critically examined and Professor Tolman's own views clearly presented. A distinction is drawn between stimulus laws and capacity laws.

Behavior does not go on *in vacuo* but rests upon basic environmental conditions called 'behavior supports,' such as the black and white objects to be discriminated or the alley of the maze. The rat behaves as if the maze were of a particular sort and a slight change of experimental conditions may disrupt behavior. In this objective sense the rat is said to show expectation. Sensory supports are called 'discriminanda' and motor supports 'manipulanda.' Other concepts such as perception, mnemonization and inference are developed.

In one part of the book entitled "Rats, Cats, Apes, and Men" an attempt is made to bridge the gap between 'the rat in the maze' and 'the psychologist in the laboratory.' Here the problems of insight, conscious awareness, and inventive ideation are tackled. Conscious behavior for Tolman is not fundamentally different from other behaviors. Consciousness appears to be a sampling reaction, a 'running-back-

and-forth.' The author is somewhat reluctant over the 'shameful necessity' for raising the question of awareness, for the behavior of rats was described without talking about consciousness. Howbeit men insist that they are conscious and have ideas. Some hypothesis is needed and the 'running-back-and-forth' hypothesis is developed.

The discussion of the psychologist in the laboratory is perhaps the weakest part of the book. Introduced apologetically the topic appears to have little genuine interest for the author. Introspection is speech behavior. The distinction between *Kundgabe* and *Beschreibung* is discussed. The latter—the 'pure' or 'true' introspection—is said to be an abortion because there is no stuff to a 'running-back-and-forth.' 'Raw feels' do not and cannot get across. If there be 'raw feels' correlated with 'discriminanda-expectations' no science can touch them. Images, like sensations, are 'discriminanda-expectations' but with internal stimuli. Feeling and emotion are referred to briefly.

The part of the book entitled "Motivation and Learning" contains much valuable material. The ultimate motivations of behavior are appetites and aversions. The former are demands for physiological quiescence, the latter demands against physiological disturbances. Second order drives and personality mechanisms are discussed. The term 'instinct' is retained for types of response or determinants of behavior which are common to practically all members of the species.

In a final section of the book the system is considered as a system and a special summary is presented for psychologists and philosophers. Tolman's system of psychology is diagramed and critically related to other systems.

From the formal point of view the presentation suffers from the introduction of too many neologisms. The difficulty is illustrated by the following quotations which are (perhaps unfairly) taken out of their context: "These discriminanda-expectations, these 'sensations,' become conscious and constitute the 'conscious feeling' of pleasantness or unpleasantness, if the organism starts to behavior-adjust to running-back-and-forth with respect to them (p. 261)." "By virtue of his initially active means-end-readinesses and discriminanda- and manipulanda-readinesses he can behavior-adjust, initially, to certain portions of the field and then, if he is fortunate, the requisite new discriminanda-, manipulanda-, or means-end-readinesses (*i.e.* sign-gestalt-readinesses and -expectations) will 'click' (p. 371f.)." Many of the ideas could be clothed more simply. A glossary is provided as an aid.

Despite the neologisms the general style is delightfully informal and on the level. The book is distinctly a contribution to psychology and is well worth the careful study of serious students. It is an authoritative statement of Professor Tolman's views and system of psychology.

University of Illinois PAUL THOMAS YOUNG

The Psychological Register: Vol. III. Edited by CARL MURCHISON with the coöperation of an international board and the assistance of Luberta Harden. Worcester, Mass., Clark University Press, 1932. Pp. xi, 1269.

The *Register* furnishes the bibliographies of psychologists the world over, together with certain biographical data. This volume, marked Vol. III, is a revision of the 1929 volume, which is now to be known as Vol. II. The designation Vol. I is reserved, we are told in the Preface, for a volume, still in course of preparation, that will be devoted to the psychologists who died before the initiation of the series.

The present edition is a vast improvement upon the earlier. Not only is it more complete (40 countries are represented as compared with 29, and approximately 2400 names are entered as compared with 1250), but it is more competently and carefully prepared and edited. Most of the defects of the 1929 edition (cf. the review in this JOURNAL, 42, 1930, 153-156) have been corrected. The greater geographical range has in the main been achieved by a more logical system of classification. Canadian psychologists, who were listed with those of the United States under the heading 'America,' now enjoy a separate section, as do also the psychologists of the other dominions and possessions of the British Empire. Only the psychologists of England, Scotland, and Wales are now listed under the heading 'Great Britain.' The increase in number of names entered is due in part to the greater time and care taken in preparation, and in part to the enlargement and the wider geographical distribution of the coöperating board (43 members from 36 countries as against 8 members from 8 countries).

The book is arranged alphabetically by country and, within these divisions, by name. It closes with an index of names, which enables the user to find an entry when he does not know the country under which it appears. The arrangement is the same as it was in the earlier edition, and it probably represents the permanent mold into which the Series is to be cast. If so, this is very unfortunate; for an alphabetical arrangement accompanied by a geographical index—as in the standard *Who's Who*—would be much more serviceable. The reader would never have to turn to the index to find an entry, and the geographical index would permit a more detailed classification—by city or town as well as by country. The adoption of this form would also emphasize the obvious need for translating into English the biographical information now printed in a polyglot of English, Spanish, German, French, and Italian, and occasionally in other less known languages.

The bibliographies also are cited in a variety of languages. English, French, German, Italian, and Spanish titles are given in the original language. No rule, however, is followed in respect to other languages. Sometimes the titles appear in the original; at other times they are translated into English, German, or French; and all this happens within the same division, to the same author, and on the same page! (Cf., pp. 1012 ff., 1234 ff.) Doubtless, copy comes to the editors in this medley of tongues; but certainly, if all the titles are not to be put into English—as is preferable—some consistent procedure could and should be adopted.

In marked contrast to the earlier edition, the references now follow a uniform style of citation. With but few exceptions, date, volume, and inclusive pages are given. The limiting dates, however, are not uniform: while most of the bibliographies stop with 1931, some of the American entries extend into 1932. Similar variations appeared in the earlier edition. Yet good editorial practice demands that the limiting date be the same for all.

The present volume has the same format as the earlier edition. It is printed in double columns of 8 pt. solid type. The authors' names are printed in boldface—surnames in capitals, given names in capitals and lower case. The printers of the volume, lacking some of the matrices necessary for this work, attempted to meet the requirements by hand-setting. This is shown by the fact that the accents and diacritical marks used in printing the surnames are set on 6 pt. letters (see, for example, pp. 666, 992, 1042); and also by the fact that the umlauts are frequently off center (compare, for example, pp. 832 and 834, 850 and 851) and of different sizes (compare, for example, pp. 586 and 587, 788 and 789). These defects, which could have been remedied easily and at slight cost, give the page an unsightly appearance and also violate the canons of good printing. The editors of the volume should not have allowed their printers to use this subterfuge.

A number of names are incorrectly placed; for example, John Adams, p. 896; J. M. Baldwin, p. 17; John Greig, p. 931; F. C. S. Schiller, p. 967; E. W. Scripture, p. 436; L. Urwick, p. 875. As the biographical data clearly indicate, these men reside in countries other than those of their listing. Most of the misplacements may be accounted for on the ground that the individuals are placed in the countries of their origin instead of residence; but if that principle be accepted, then many other changes would have to be made if consistency is to be achieved. Questions concerning place of listing would have been avoided had the single alphabetical arrangement mentioned above been used. Here then is still another reason for its adoption.

K. M. D.

How To Interview. By WALTER VAN DYKE BINGHAM and BRUCE VICTOR MOORE. New York, Harper & Bros., 1931. Pp. xiv, 320.

The interview, or "conversation with a purpose" (p. 3), is shown to have so wide use and so much variability in methods and applications as to warrant careful study of the uses, techniques, and dependability of results. "Regarding pitfalls of interviewing and the best ways of avoiding them, there exists a wealth of hard-won practice and tradition not only among industrial investigators and employment managers, but also among physicians and psychiatrists, lawyers skilled in questioning, police inquisitors, salesmen, journalists, teachers, vocational counselors, and particularly social workers. Their practices and techniques it is our purpose to scrutinize, and to compare them with the results of experimental studies" (p. ix). This is the authors' main problem. They sought to improve the techniques and to avoid the pitfalls, to the end of obtaining more reliable data by means of the interview. The study was made possible by a grant from the Social Science Research Council.

In the investigation, conferences were held with sixty experienced interviewers and in some instances "their actual interviews were observed." Hypotheses for experimental tests grew out of differences in views and practices. The authors regard the testing out of methods and criteria and the improvement of techniques as more important, at the present stage of development, than the specific facts and data obtained. The interview, it should be noted, is used not only to secure information about attitudes, beliefs, interests, etc., but also to motivate and to instruct people. The interviewer, to save time, to prevent omissions, to avoid confusions and to elicit coöperation, must prepare his questions carefully before hand, must be thoroughly conversant with his problem, must follow a definite plan and outline, attend carefully to the precise wording of key questions and other such details: but above all he must be objective minded, and manifest a "straightforward honesty and frankness" that will engender respect and confidence on the part of the interviewee.

Two sorts of error are found, the variable, easily detected by the usual statistical methods, and the constant error. In the latter the statements not only vary one from another, but in general tend to vary in one direction from the facts they purport to describe; they are easily detected in "the scatter of observations." The causes of constant errors may be located in the suggestibility or "leading" tendency of certain questions, in bias of persons interviewed, in certain mental sets of the investigator, etc., and may, on investigation, lead to better technique. Such studies by the investigators themselves are valuable. Chapter 2 is devoted to the systematization of the vocational interview, giving sets of aids in the way of blank forms to be filled out by the applicant, others to be worked out jointly by applicant and counselor, and still others to be filled out by the counselor only. The first of these call for present occupational plans, home and family circumstances, physical and mental condition, educational and work histories, vocational interests, etc.; the second covers the same topics from the standpoint of appraisal of the assets and liabilities of the applicant, while the last calls for recommendations, impressions and opinions, and scores made on certain tests. A manual of directions for counselors is also briefly outlined and described. The next chapter is devoted to the practical matters of learning how to interview and the giving and discussing of 52 suggestions. In a chapter on The Employment Interview one gets a view of the immensity of interviewing not only to help persons find the right job, but also to increase their earning capacity and to increase their comforts and happiness in the work. There are interviews for job analysis, for follow-up and adjustment of persons placed, for instruction, and many other purposes, and, finally, interviews when the employee leaves or is dismissed. The latter often furnish useful information regarding working conditions, attitudes of workers, etc. Pressure of work reveals the fact that these functions of interviews must often be imperfectly performed. In 1929, for instance, R. H. Macy & Co., New York, interviewed for employment 176,000 applicants of whom only about 13,000 were employed (1 in 13)! In such interviewing there is first a short sifting interview of ½ to 3 minutes followed by longer interviews, the filling out of application blanks, and probably even some tests, of the more promising candidates. Considerably more care is taken when one is to be promoted or appointed to an executive position. Sales managers fail to agree closely on individuals in brief personal interviews, the correlation of anyone's ratings with the ratings by others usually running from 0.55 to 0.85 (which is about 0.16 to 0.47 better than a pure guess). But even this is a very considerable improvement over results of earlier tests of similar kinds.

Other chapters deal with interviews in industrial relations, in marketing studies and commercial surveys, in social case-work, in education, in mental clinics, in journalism, and in law, and with evaluations of results. The last three chapters (14–16) present in considerable detail two researches in the actual use of interviews for industrial fact-finding. In the first research employees were interviewed to determine their attitudes toward the provisions of an employment guarantee plan, and in the second one textile workers were interviewed during a strike with a view to ascertaining their attitudes toward arbitration. In these latter (experimental) studies, results from cumulative groups of increasing size were correlated with those from all the interviewees, to ascertain approximately the number of interviews necessary for fairly reliable results. The authors find that the interview, skillfully used, has its greatest value in ascertaining the attitudes and feelings of the interviewees. Facts concerning external physical events and dates of past occurrences were less satisfactorily obtained. Divergences among results secured by different interviewers were accounted for to a considerable extent by the different compositions of the groups interviewed by the several interviewers—nationality, sex, membership in different workers' organizations, etc.—and it was found that great care should be taken that the questions be made clear as to their implications or meaning, and that a final checking with the interviewee of his replies contributed considerably toward this end. Results showed that 200 "well selected and carefully directed interviews are highly indicative of what can be obtained from two or three times as many." The accuracy of results were checked both by using records made accessible and by data from a secret ballot. One may well query whether, and to what extent, the more reliable results obtained as to attitudes and feelings of the interviewees as compared with those as to memories of events may be due to a greater limitation of possible replies and to the more stereotyped forms of reply and the more frequent discussion (social inter-stimulation) in the case of attitudes. The only test of the validity of reports relating to attitudes and feelings would seem to be consistency among the replies.

The clear and interesting manner in which these studies have been presented by the authors will undoubtedly tend to stimulate further research on the interview. A valuable feature of the book is a classified bibliography of 338 titles.

J.P.

Contemporary Schools of Psychology. By R. S. WOODWORTH. New York, Ronald Press, 1931. Pp. vi, 332.

Woodworth's own words characterize this book. It seeks to "provide an objective view of contemporary psychology so far as concerns the schools and their illuminating views of humanity" (p. iii.). It is argued that the view shall not be exhaustively critical but for the most part expository. The setting for the exposition is created by a brief historical chapter which says that the history of psychology is a continuous series of revolts against an "established order." In 1900, the date which separates the "contemporary schools" from the history of psychology, the science is said to have subscribed to a study of consciousness, to the method of analysis, to associationism and to a physiological bias. Since 1900 the chief schools, developed by the natural tendency of ambitious young men to "pick flaws," are behaviorism, psychoanalysis, purposivism, existentialism, and Gestalt psychology. The chapter closes with a prophecy of the sort of unity to be found in the science, a prophecy which does not find fulfillment, however, in the last chapter.

Chapter 2 makes a survey of the existential school of psychology. This school is described as the logical descendant of the introspective school and of the method of impression. The development of the school is traced through the work of Wundt, Ebbinghaus, Müller, the Külpe school, and Titchener. Chapter 3 tells the story of behaviorism which is, for the most part, the story of Watson. This school is seen to have developed out of (1) functionalism (James, Dewey, Angell), (2) the performance psychologies (Galton, Ebbinghaus, Cattell, McDougall), (3) animal psychology (Thorndike), and (4) conditioned and unconditioned reflexes (Bekhterev, Pavlov). The last part of this chapter considers the later development of Watson's point of view and the work of other behaviorists such as Max Meyer, Weiss, Hunter, and Lashley. Altogether the author writes about behaviorism with more competence and understanding than he displays with respect to existentialism.

Chapter 4 considers Gestalt psychology. This is another revolt against "the established order," viz., associationism; but whereas behaviorism reacted against intellectualism and consciousness, configurationism reacted against analysis. A short summary of the origins of Gestalt psychology (Ehrenfels) is followed by an account of some of the experimental work which supports the configurational school, *i.e.* in the field of perception (including figure and ground), animal behavior (Koffka, Köhler), and learning.

Chapter 5, the longest in the book, tells of the development and of the program of psychoanalysis and related schools. The story begins with Charcot, the early struggles of Freud, and with the initial work of the psychoanalytical school. Several of the concepts central to the school are then described, viz., repressed infantile sexuality, motivation, polarity, the subconscious, and the like. In a brief appreciation of the movement, Woodworth ventures the opinion that Freud's psychology can scarcely "rank with the great scientific theories which coördinate existing knowledge and serve as guides to further discovery" (p. 161). In spite of its "freshness and daring," its concepts are said to be retrograde rather than forward-looking. The remainder of the chapter is devoted to a brief account of Adler and Jung.

Purposivism or hormic psychology is the topic of Chapter 6, and as is almost inevitable, it centers largely upon the work of McDougall, especially with reference to instinct. It seems to the reviewer that Woodworth rightly challenges McDougall's assertion that hormic psychology puts the science at a parting of the ways. The challenge is offered in terms of the proposition that "purposes" within behavior are of one character whereas a general philosophy of purpose lies outside of psychology and is to be accepted or rejected without any prejudice to the science itself.

The last chapter (Chapter 7) is an exceedingly brief account bearing the title, "The Middle of the Road." The middle of the road turns out, in part, to be a convenient place to mention Spearman, Myers, Spranger, Stern, Claperède, Benussi, de Sanctis, Kiesow, Piéron, Dumas, and others; and, in part, a place where it can be said that hundreds of psychologists owe no primary allegiance to any of the schools.

We thus see that Woodworth has stuck to his program of giving an objective but uncritical exposition of the schools of psychology. It is as though he had written an extended review of the *Psychologies of 1925* and of *1930* together with occasional references to the first volume of the *History of Psychology in Biography* so that beginners might have a general perspective of the science. The major conclusion is that younger psychologists should obey the injunction of a negro melody and "keep in the middle of the road," although no definition of this position is offered save that which is suggested by the fact that it is impossible to locate all the members, say, of the *American Psychological Association* under any one of the banners of the five schools named. The book is engagingly written and often reveals the genial spirit of the author; but it leaves one with a deep yearning for a major piece of systematic writing.

University of Illinois COLEMAN R. GRIFFITH

Race Psychology. By THOMAS RUSSELL GARTH. New York, Whittlesey House (McGraw-Hill), 1931. Pp. xiv, 260.

This volume of Garth's is the first to appear in which presumably the aim is to give a systematic survey of the problems, methods and results of experimental race psychology. Undoubtedly a book of this sort could be a valuable contribution to a field where the canons of experimental science have too often been neglected in attempts to accumulate "results" to prove an *a priori* hypothesis. Perhaps the reviewer expected too much of the book, especially in view of the statement in the preface that the author "has endeavored to keep the general reader in mind as much as would be consistent with a scientific handling of the materials." At any rate, his general feeling on reading the book has been one of disappointment. The "general reader" has been kept too much in mind, particularly the reader saturated with the backwash of sociological theorizing which has captivated the imagination of the "tenderminded" during the past decade.

It is debatable whether or not a book on race psychology designed to serve as an elementary text-book or as a popular treatise for the non-psychological public should be written at the present time. The non-technical reader lacks the background requisite to the proper evaluation of the masses of data presented all too sketchily and uncritically in Garth's book. It is misleading to give the impression that an unambigous body of factual material exists which can be "interpreted" in support of an environmental explanation of race differences in performance on psychological tests. The proper scientific attitude in this field at present is one of agnosticism, an attitude which is alien to the "popular" reader. What is needed in race psychology is not an uncritical compendium of test "results" with interpretations of the sort referred to, but rather a discriminating, critical appraisal of the problems, methods, and results.

Space limitations prevent a detailed consideration of the subject matter of the book. In general, an attempt is made in the first two chapters to state the author's conception of the essential problem of race psychology, namely the question of the relative effects of heredity and environment in the causation of observed behavior differences. A very elementary survey of the methods and present status of studies of "mental heredity" is presented. These introductory sections are followed by chapters on the methods and results of experimental work on race differences in intelligence, color preferences, musical talent, personality, fatigue, and community of ideas. A final chapter on the evaluation of races epitomizes the author's position on the causes of race differences. Environment and "racial mobility" are held to be the essential causal factors.

Perhaps the two best features about the book are a bibliography of 196 titles and a summary table listing the following facts about the experimental studies of race differences: date, investigator, races compared, number of cases, results. A summary of results in brief tabular form would necessarily be fragmentary and, in view of the absence of a common unit in the several studies for expressing amounts of difference, often misleading and meaningless. A few examples might be cited: "94% of Indians below age," "whites excel Negroes," "Negros two years younger," "performance of Negroes ⅔ that of whites," "Negro deficiency 88 quotient units" (this is obviously an error), "Whites have stronger personality 15.4%." Such vague and varied comparative measures illustrate the desirability of the use of a standard unit for expressing race differences, a point made several years ago by Peterson, and which Garth appears not to have appreciated. In fact Garth urges "the use of the I.Q. in expressing the comparative intelligence of racial groups," despite the difficulties which he must have encountered in constructing his summary table. One feels almost apologetic in pointing out the obvious and numerous objections to the use of gross I.Q. difference as an index of "comparative intelligence of racial groups." The I.Q. is not a "score" (*i.e.* a direct index of performance), it depends upon various questionable procedures as regards standardization when used to compare groups heterogeneous for race and age, and gross I.Q. differences cannot possibly indicate the amount of difference between two specific groups in the only sense in which "amount of difference" has any statistical meaning; namely, in terms of normal frequency curve area. Garth believes that the I.Q. is an "easily understood measure," as an index of comparative mental status of different groups. The reviewer would like to insist that gross I.Q. difference alone cannot be understood, however hard one try. If the Stanford-Binet I.Q. is objectionable as a comparative unit, the group test I.Q. is even more so. It is amazing that such a value should be advocated in a scientific treatise.

The fact that the writer of a popular treatise labors under the impulsion to reach definite conclusions for his readers may account for certain errors in fact, overstatements, and unjustified conclusions found in the present book. A few of these will be mentioned to substantiate the reviewer's notion that a book on race psychology which does not deal with the methods and results technically and critically cannot but falsify the status of existing knowledge and scientific opinion in the field. We are told on page 29 that "the work of Galton served to prepare the way for work more firmly grounded in experiment, and this is the work especially of Mendel who was able to discover three principles of heredity. . . ." Actually, Galton and Mendel were born in the same year (1822) and Mendel's *Versuche über Pflanzen-hybriden* was published in 1865, four years before Galton published *Hereditary Genius*. The fact that Mendel's work was not "rediscovered" until about 1900 does not validate Garth's statement, made in the easy, off-hand fashion of one set to write for the "general reader." On page 35 correlation coefficients between National Intelligence Test I.Q. and degree of white blood in Indians are presented as follows, by grades: 4th, 0.70; 5th, 0.76; 6th, 0.22; 7th, 0.23; 8th, 0.24. Garth's conclusion from these values is "that education tends to reduce to a minimum, or even nothing, the effect of the presence of Indian blood in the educated mixed-blood individual." The problems of genetics and of race psychology would indeed be simple if they could be solved in this easy fashion! As a matter of fact, these figures raise several questions without answering any at all. Among the questions suggested by the coefficients are those regarding the nature of the white "blood" in the several groups, the adequacy of the samplings, the reliability of estimates of degree of Indian blood, the testing methods, and the range of classes of "degree of Indian blood" used in calculating the correlation coefficients.

A gross interpretation of the work of Peterson and Lanier is made on page 84. Garth's statement is as follows: "Differences so far found in the intelligence of races can be easily explained by the influence of nurture and of selection. See again Peterson and Lanier. . . .. The low I.Q. of Indians and Negroes are undoubtedly due to these factors." I do not know what Garth means by "selection" here, but as used by Peterson and Lanier the factor of selection and that of nurture are not similar in operation but are directly antithetical, as far as their interpretative significance goes. Peterson and Lanier found a decreasing race difference in comparing negroes and whites in Nashville, Chicago, and New York. They suggest as alternative explanations better environmental conditions or selective migration, with the further statement that both may operate although their relative effects cannot be determined as yet. Garth adopts the first alternative and misunderstands the second to the extent of believing that it is evidence for the first.

A major defect of the book is the failure of the author to analyze critically the testing methods used to secure the data to which he refers or presents in such profusion. There is little discussion of the reliability or applicability of the various types of tests to the problems, and even less effort to find out whether or not the data mean what they are purported to mean. An example is the implicit acceptance of Klineberg's explanation of white superiority in test performance as due to superior "speed" where as in "accuracy" no race differences exist. Another illustration is found in the attitude towards Seashore's Tests of Musical Talent, group tests notorious for their low reliability. We are told that "but for the introduction of a clever and scientific set of group tests we should at this point be frustrated in any experimental evaluation of musical talent of races." Somewhat later the following *argumentum ad verecundiam* is noted: "We have given this description of the traits to be examined from the standpoint of the inventor of the tests, whose superior experience and extensive experimental studies give great weight to his observations.". In general, one prefers to form an opinion as to the scientific merit of methods and results on the basis of their intrinsic value, as determined by accepted standards of evaluation obtaining within the given branch of science. Garth is too prone to accept methods and results at their face value.

Garth is "prejudiced" in favor of a nurtural explanation of observed race differences in performance. He deplores the fact that "stupid race prejudice" has operated to restrict the opportunities of "inferior peoples" and to influence opinion concerning the comparative abilities of different races. He closes the chapter on Intelligence and Euthenics with the hope that an educational engineer may arrive who will render to the Indian and to the Negro in America "the treatment consistent with their euthenic needs." While this may be an admirable sentiment it really has little to do with scientific race psychology. The "environmentalistic" uplift attitude which has influenced many psychologists to abandon the scientific point of view towards the problem of the causation of race differences in behavior is no more justified, in the absence of crucial evidence, than the implicit belief in heredity as an explanation of such differences. The fact that the latter hypothesis remains unproved does not establish the validity of the former. It is unfortunate that Professor Garth did not choose to remain unbiased on this question, since his arguments may give rise to widespread misapprehension on the part of uncritical readers.

Vanderbilt University LYLE H. LANIER

Psychology. By ROBERT S. WOODWORTH. Revised Ed. New York, Henry Holt & Co., 1929. Pp. xiv, 590.

Professor Woodworth has reorganized and completely rewritten this book—the first edition of which was extensively reviewed in this JOURNAL (Vol. 33, 1922, 430-435). The revision extends from the significant dropping of the subtitle ("A Study of Mental Life") and the rearrangement of the various chapters to modification of the style.

Woodworth's plan in the new arrangement of the material, which he states "is frankly an experiment" (iii), was "to start with the broader and less technical topics and proceed to analysis and still more minute analysis" (19). In accordince with this plan he considers—after an introductory chapter in which he presents his idea of what psychology is and does—such general activities and factors as Intelligence, Memory and Learning, Heredity and Environment, and Action and Motivation. He then turns to analysis and takes up the special activities of Feeling and Emotion, Sensation, Observation, Thinking, and Imagination; and then he proceeds to "still more minute analysis" and treats of the physiological activities, viz., the activities of the endocrine glands and the nervous system, the latter of which "has most to do with integrating the various organs into a whole individual and giving shape to his activities" (501). The concluding chapter, Personality, is given over to the synthetic problem: to the treatment of the individual as a whole and of his social adjustments.

Despite drastic alterations of the book, its point of view remains unchanged. Notwithstanding the fact that Woodworth "professes to belong to no one of the schools" (20), he again gives us a functional psychology of the empirical and biological type that is cast in the stimulus-response mold. His "middle-of-the-roadness" consists in ignoring systematic questions; it does not mean that he writes without a point of view, or that he is catholic in his treatment of the subject-matter. His book is as biased in its direction as a polemical essay, but the beginning student is not made aware of it chiefly because controversy is taboo and few attempts are made at definition. The reader is allowed or forced, as in the first edition, to bring his own definitions to many of the technical terms employed in the book. Moreover, where definition is attempted, the results are common-sense and general, vague and uncertain. For example, his basic concepts, stimulus and response, are tautologically defined: "a stimulus is any form of energy acting upon a sense-organ and arousing some activity of the organism" (225); a response is "any activity of the organism that is aroused by a given stimulus." But that is not all; these concepts are no sooner presented than they are modified by the introduction of the organism as the mid-term in the S - O - R formula. The O is, like Freud's censor, the *deus ex machina;* it selects the stimulus, and determines the nature and extent of the activity aroused.

Woodworth, moreover, does not specify the kind of activities studied. Though psychology is said to be the scientific study of the activities of the individual as a whole (3), not all the activities of the whole organism are considered. The selection might be comprehensible if the phrase "individual as a whole" were unequivocally defined, but it is not; and the muster roll of the activities treated merely adds to our perplexity. That wholeness is not the criterion for inclusion, the reader very soon discovers. The activity of the circulatory system, for example, which extends throughout the entire body, is excluded; as is also—though with apologies (501)—the activity of the muscular system. On the other hand, such isolated activities as the knee jerk and the pupillary reflex are included. What is the principle of selection? Upon what basis is this activity included and that one excluded? The author does not state, and his exposition does not reveal. In desperation one is forced to take as a guiding principle the facetious definition that "psychology is what the psychologist considers."

The discredited law of exercise—particularly its negative aspect, atrophy through disuse—is introduced without defense or comment in an early chapter and is carried forward through the book as an explanatory principle. The Danish physiologist

Lange is said to be a psychologist (304).—But these are details (the like of which the informed reader can find many), that lie outside of the general scope of the present review.

The book is pleasantly and entertainingly written. The style, while still free and easy, and conversational, is more conventional than that of the first edition. The elimination of colloquialisms is a distinct gain.

The new arrangement, which breaks with tradition, is successful. The exposition proceeds from the general to the specific. Topics of immediate interest are first considered and then the exposition turns by gradual steps to topics more remote and abstract. Woodworth renounces the logical method of presentation for the psychological. The result is good—may more 'experiments' of this kind be made.

K. M. D.

Elements of Human Psychology. By HOWARD C. WARREN and LEONARD CARMICHAEL. New York, Houghton Mifflin Co., 1930. Pp. ix, 462. 118 illus.

In this revised edition of Warren's *Elements of Human Psychology* the authors attempt to recognize the contributions of the *Gestalt,* the introspective and the behavioristic methods to psychology. The general make-up of the book follows the older edition, though several changes have been made in chapter headings and the order of presentation. Considerable amplification and re-defining have been made which have served to increase the clarity and reading qualities of the material. Numerous pertinent illustrations are used with excellent effect, and should do what many elementary texts do not—make concrete the fundamental concepts of psychology.

Approximately one-third of the book is devoted to the nervous system and its functions. This seems justified by the many attempts of the authors to explain mental and behavioral functions by means of hypothetical functions of the nervous system. In an elementary text such explanations seem to lend themselves to unintentional explanatory principles, even though there are many cautions given the reader to recognize them as merely hypothetical.

The attempt to combine introspective and behavioristic methods has led to a direct comparison between them, since they are often used to explain the same facts. Whenever possible the association theory in terms of images is used; and the behavioristic (conditioned response) method, whenever the evidence for imagery is not conclusive. It is pointed out that motor imagery is difficult to classify since actually it may be "slight muscle responses." Verbal associations have this same difficulty. A 'word' may be analyzed psychologically as sound, visual, muscle, or other experience. The frequency of such difficulties as are found in "unconscious" associations leads one to the central cleavage between the introspectionists and the behaviorists. 'Human Psychology' is described as "the science which deals with the interaction between man and his environment by means of the nervous system and its terminal organs, together with the mental events which accompany this interplay." On reading the book one is inclined to favor the behavioristic viewpoint for two reasons: first, when the imagery-association theory does not seem adequate, the conditioned response theory does, and there is no evidence to support the possibility that it may *not* operate equally well where association between images is given as the explanation; secondly, because of the above, and because the "interaction between man and his environment" seems more important to the central biological interests of most psychologies, one is inclined to accept the imagery as worthy of study, but inconsequential as a functional part of the interaction of man with his environment.

As in the older edition, the book contains an index and dictionary of psychological terms. An appendix of seven chapters includes the following: the central nervous system, neural and mental processes, the nature of color, tables, review questions, suggested class demonstrations, suggestions in using the book. The end of each main chapter contains references for subsidiary readings.

University of California JACK BUEL

Physique and Intellect. By DONALD G. PATERSON. New York, The Century Co., 1930. Pp. viii, 297.

In this clearly written and well-organized summary, Paterson contributes to the analysis of individual differences in "intelligence" and "temperament" by showing that they are almost totally unrelated to differences in physique or physical condition. The data from various experimental studies in this field are shown to be in remarkably close agreement, but their original interpretations have been quite diverse and often unwarranted by the evidence at hand. By inquiring as to the adequate control of such variables as age, sex, race, and social status, and limiting the interpretations of statistical results to those listed in texts on statistics, a very large proportion of the studies are found to be non-critical or at best only suggestive of a supposed relationship. "The belief that there is necessarily an intimate connection between physical and mental traits is 'easy to scotch but hard to kill.' The author believes that many of the cited investigations were obviously conducted for the purpose of collecting substantiating evidence rather than as impartial inquiries designed to ascertain the true status of the relationship between physique and intellect."

The content of the book can best be indicated by a quotation from the summary given in the final chapter.

"Our detailed survey of available quantitative evidence has demonstrated that prevalent notions regarding the intimacy of the relationship between physical traits and intellect have been greatly exaggerated. Search in the realm of gross anatomy for a physical correlate of intellect has yielded uniformly negative results. It appears that such structural characteristics as height and weight are correlated only slightly with intelligence, narrowly defined. Even measurements of head size and shape are found to be relatively independently variable with respect to intellect, and skeletal development measured by precise X-ray photography yields either zero or low correlations with intelligence. The same may be said of dentition. Physiological development, measured in terms of pubescense, is found to be relatively unrelated to mental development, and so are complicated morphological indices of body build.

"A more surprising finding is the negative trend of the evidence with respect to mental correlates of ordinary physical defects and certain presumably deleterious physical conditions. In addition, there is the disappointing outcome of glandular therapy as a predicted panacea for mental deficiency. The evidence seems quite clear that mental development proceeds in relative independence of physical condition except when disease processes or injuries directly attack the nervous system, especially the higher centers.

"The suggestion is frequently encountered that physical traits may be found associated to a greater extent with temperament than with intellect. Even here, however, little optimism is justified. An intimate connection between body build and temperament has not been disclosed. The positive findings of Kretschmer, Wertheimer and Hesketh, Farr, and Mohr and Gundlach are restricted to definitely pathological material" (269 f.).

Having cleared the field of numerous hypotheses which were not adequately supported by experimental evidence, Paterson goes on further to suggest a number of experiments and theories which might logically follow this survey of the field. "Without doubt the negative trend of the evidence reviewed has important implications for theories of the nature and organization of personality. The findings clearly support the *theory of unique traits* as formulated by Thorndike, Kelley, Toops, Hull, and others" (p. 276). Paterson, furthermore, says that this theory was supported in the Minnesota Mechanical Ability investigation, with respect to I.Q., mechanical ability, and physical agility, which he holds to be unique traits each with respect to the other.

"Progress in personality evaluation and analysis will be furthered by the discovery of additional identifiable and measurable traits unique among themselves and unique with respect to the unitary traits already isolated and measured" (276 f).

The reviewer anticipates a difficulty in the theory of unique traits if it is extended to differences in sensory acuities and fine motor skills. Here the individual differences are so highly "unique" or specific for each performance measured that they lose nearly all value for the prediction of other performance, even those which would seem to be qualitatively related. We are greatly in need of another critical review similar to Paterson's which would cover theories which refer the bases of individual differences to *functional* constants, such as speeds of nervous conduction, and "nervous energy." It would not be at all unlikely to find that this set of theories which seem to be implicit in most of the literature on individual differences is resting on no firmer basis than the previously supposed relation between physique and intellect. Are we not neglecting the fact that the *same organism* can function in *many different ways,* and that these methods of work may be a basis of the individual differences?

The style and organization of the book afford a good example of scientific evaluation of hypotheses and their supporting evidence. The book is likely to find a useful place as a reference in courses and research bearing upon individual differences of either physique or intellect. It may be read profitably by teachers of elementary psychology since it shows up very clearly the defects of many popular ideas often considered in these courses. The students will not be the only ones to lose some cherished beliefs.

University of Oregon ROBERT H. SEASHORE

Psychology: Science or Superstition? By GRACE ADAMS. New York, Covici Friede, 1931. Pp. 299.

The criterion of the success of any discipline would seem to the author to be the amount of public interest in it; and this is apparently to be judged by the amount of drawing-room conversation and by the extent of newspaper publicity any discipline obtains. From this point of view a great deal of this volume might be summarized by the statement (p. 209) "until psychoanalysis captured the popular imagination, there was no public interest in psychology." The corollary, which the author stresses, is the failure of experimental psychology. In this regard we find James in 1899 confessing: "In my humble opinion there *is* no 'new psychology' worthy of the name." To this statement, the author feels compelled to add (p. 269) "yet the science of psychology is essentially the same as it was thirty-two years ago. It has not made a single substantial or fundamental discovery since 1899."

The method of development of the book is historico-critical, beginning with Aristotle and ending with the present day. Miss Adams tries to show that few psychologists have completely divorced themselves from Aristotle's problem which was to "discover and ascertain the nature and essence of the soul." Indeed it is her opinion that William James and G. Stanley Hall are the only two American psychologists really worthy of consideration, primarily because both, in later years, denied the experimental method and developed their systematic positions along metaphysical lines. Of all the others, foreign as well as American psychologists, only Titchener and Watson (in his early period) stuck to their theoretical postulates with the result that their systems are sterile so far as popular interest is concerned and so far as they have had any effect on popular thought. This is because they were able to treat only of the simpler processes and found an experimental attack on the more complex processes impossible. All of the other psychologists, and many are mentioned and criticized, have based their psychological theories "not upon accumulated evidence but upon personal opinions" (p. 280). This group includes the psychoanalysts as well as the academic psychologists. So, with few exceptions, most psychologists "are still swimming, some floating, others with bold overhand strokes, in the soothing waters of metaphysics, which lead straight into the caverns of theology where man and God are fused."

The book is entertainingly written even though some points seem to be made largely by errors of statement or by ignoring certain materials. For example, mental tests gained their popularity under the application of Terman's "mathematical wizardry" to the Binet-Simon tests—a mathematical wizardry which involved the calculation of a simple ratio. Since the World War, which was the zenith of mental testing, "their only really noticeable expansion has been in the educational institutions where their inventors were teaching before they heard the call to military service." But what about all of the children in practically every school system in America where the giving of psychological tests are matters of routine? Again we find (p. 244) that when Köhler's apes found one stick too short to reach the bait, "he would snatch up two, hollow out the end of one, whittle away the end of the other, fit the two sticks tightly into the end of each other." But, unfortunately, Miss Adams does not tell us whether the ape went over and asked Köhler for the loan of his penknife or whether the ape made a knife for himself by chipping a flint on the iron hinge of his cage.

It is not worth while to unduly extend this review by citing all the errors of omission and commission. A few will suffice. Miss Adams has insisted that industrial applications of psychology have failed and that such applications have consisted merely in the giving of mental tests for the elimination of "inefficient employees in business" (p. 205). In contrast to this statement some space is given to an account of Anderson's work from a psychiatric approach to employment problems at Macy's store. No mention in this connection is made of the work of C. S. Myers, Viteles, W. V. Bingham or of any of the German workers in this field.

Apparently for Miss Adams, experimental work on industrial problems ended with the death of Münsterberg. She consequently proves her thesis—to her own satisfaction at least—that experimental psychology has failed and that psychoanalysis has captured the popular interest to the point that psychoanalysis *is* psychology outside of the academic institutions. Her statement is plain and unequivocal (pp. 117 ff.): "In other words, to the world outside the colleges, psychology now means neither empiric philosophy, nor mental physiology, but psychoanalytic hygiene." In support of this statement, the author (p. 178) lists 28 names of individuals who since 1909 have been awarded the "distinction" by the American Psychological Association "as its leaders." (Langfeld's name is spelled with a final "t.") The author asks the reader to examine this list and see how familiar the names may be. One wonders if the test would fare any better if applied to the names of the recent past-presidents and boards of governors of the Chemical, Physiological, Physical, or Biological Societies in America.

The author further says that "when a psychiatrist has exhausted the material obtained from patients and friends, he can always turn to literature" (p. 175). Following this suggestion, the reviewer recommends that Miss Adams obtain a copy of Aesop's fables and read the story of the fox without a tail.

University of Pennsylvania SAMUEL W. FERNBERGER

The Great Apes: A Study of Anthropoid Life. By ROBERT M. YERKES and ADA W. YERKES. New Haven, Yale University Press, 1929. Pp. xx, 652; 172 illus.

The Yerkes have brought together within the compass of this book all of the available information regarding anthropoid life. It is one of the most thorough and competently written hand-books of our time. It will long be used as a source and reference book by those interested in the great apes.

The work is divided into 46 chapters, arranged systematically into 6 parts. The 5 chapters of Part I are historical. Chapter 1 is concerned with ancient knowledge of anthropoid life; Chapter 2, with mediaeval knowledge; and Chapter 3 with 18th Century knowledge. Chapter 4 traces the emergence of our knowledge of the gorilla, an anthropoid unknown during the 18th Century; and Chapter 5 presents briefly the derivation of the popular and technical terms and the taxonomic and systematic information that is essential to an understanding and appreciation of the subject-matter. The four succeeding parts are devoted to the separate consideration of the different anthropoid types: Part II, the gibbon; Part III, the orang-outan; Part IV, the chimpanzee; and Part V, the gorilla. The materials in these different parts are, to the great convenience of the reader, arranged topically.

Part VI, composed of three chapters, supplies a comparative survey of the field. Chapter 44, the first of the three, gives a systematic comparison of the five principal anthropoid types—the siamang in addition to the four mentioned above. It closes with a résumé cast in tabular form (552-561). Chapter 45 compares similarly four of the representative types of primates; namely, lemur, monkey, ape, and man. It closes with a bibliography of 42 titles which relate specifically to the topics of the chapter. Chapter 46 gives a "summary of the anthropoid research in retrospect and prospect." The future seems bright.

The work closes with a comprehensive bibliography (593-617), and an excellent index (619-652). The bibliography "except for the omission of anonymous publications and articles in newspapers and popular magazines which seem to lack scientific value . . . is supposed to be complete through 1927" (footnote p. 593).

"Only incidentally does the volume support theses or present hypotheses; it is offered, in the disinterested spirit of science, to promote knowledge and enlightenment through the encouragement of honest, painstaking, unprejudiced observation" (ix). The authors have been eminently successful in their task.

K. M. D.

A Handbook of Statistical Nomographs, Tables, and Formulas. By J. W. DUNLAP and A. K. KURTZ. Yonkers-on-Hudson, World Book Co., 1932. Pp. vii, 163.

This book is included in the Measurement and Adjustment Series edited by Professor L. M. Terman, and seems to the reviewer to be the most useful single tool available to the worker in educational and psychological statistics. Having observed the manuscript in preparation, I am aware of some of the difficulties encountered, first in the preparation of the nomographs and tables and in the derivation of formulas, and second in securing the remarkable degree of accuracy attained. The authors believe that the error in reading the nomographs will not enter before the third or fourth significant figure of the final answer. I determined the standard errors of 100 means, first from Nomograph 5, and then with a calculating machine. These agreed in all instances to the second significant figure and in most cases to the third. Constants entering into construction of the tables have been taken to at least eight significant figures. All have been carefully checked and, in case of any doubt, recalculated. Many of the formulas have been independently derived by the authors and seem unusually free from error.

The usefulness of the nomographs (28 in number) in solving some 38 formulas is almost too patent to need comment. After using a few of them I should estimate a saving of from one-third to one-half the time required for the usual substitutions and calculations. The standardization of notation throughout the book is highly commendable. Several of the tables are new and will undoubtedly prove useful. Tables of probable errors are included, seemingly, for the sole benefit of those who have become accustomed to think in such terms rather than in terms of the standard error. The value of the book might be enhanced by a loose-leaf binding of some sort so that the nomographs, when being used, could be placed flat on a desk or table. Possibly they would be more durable if printed on a heavier and water-proofed paper. These are problems of mechanics, however, and may not be thought of as detracting seriously from the outstanding merit of the book.

Western Kentucky State Teachers College M. B. JENSEN

Die Seele und das Ich im Homerischen Epos, mit einem Anhang Vergleich mit dem Glauben der Primitiven. By JOACHIM BÖHME. Leipzig, Teubner, 1929. Pp. 130.

Böhme attempts to determine the order in which the different meanings belonging to a concept such as 'heart,' 'mind,' and 'self' have come into usage. He analyzes the text of Homer to ascertain the connotation of such terms as used by Homeric man. He also tries to determine the meaningful implications of such terms in the mind of primitive man, relying chiefly in this case on the recent work of Arbman.

Böhme submits that there are three possible views in regard to the order of development of the meanings which a concept carries. First, there is the view that a word such as heart, which has both organic and mental or spiritual reference, had originally an organic meaning and gradually acquired broader shades of meaning including those of mental significance. Secondly, there is the opposite view that primitive man first used such words with mental signification and that organic reference had to await the accumulation of definite knowledge of anatomy and physiology. Thirdly, there is the view that primitive man used concepts in a generic sense embodying both organic and mental implications of meaning and that differentiation of the various shades of meaning had to await increase in knowledge in regard to mental as well as in regard to organic facts. Böhme subscribes to the third view on the ground that such words as hunger, fatigue, effort, and sex bear both organic and mental reference in Homeric writings, and also because concepts tend to have general, undifferentiated and non-specific meaningful reference in Homeric literature. Food revived Homer's soldier in spirit as well as in body. Even in the case of terms which are used today exclusively in a mental or spiritual sense, Böhme attempts to show that as used by Homeric man these words first carried a double meaningful connotation—bodily and mental—and referred to situations involving life processes or man's doings.

Although the author gives the impression of having been extremely careful in his analysis of meanings in the Homeric text and provides ample quotations to support his claims, the reviewer submits that at certain points any study of this type is bound to leave matters in a state of uncertainty and inconclusiveness because of insidious errors which attend subjective interpretation. The danger of attempting to press too many details under a general principle is perhaps not wholly escaped.

Wellesley College MICHAEL J. ZIGLER

The Meaning of Music. A Study in Psychological Aesthetics. By CARROLL C. PRATT. New York, McGraw-Hill Book Co., 1931. Pp. 253.

This is an attempt to achieve—through a melange of psychology (mainly revamped Titchener with a few side-glances at *Gestalt*) and of aesthetics—some ultimate knowledge of the way in which we perceive music and of how it affects us. Although not an impossible presentation of the problem, it approaches this more closely than would be expected from the author of so many excellent reports of experiments. The book has its good points, but its failings decidedly outnumber them. They arise principally from four factors: first, from attempting to thread the labyrinth of aesthetics without being thoroughly master of the field; secondly, from an apparent inability on the part of the author to crystalize a line of reasoning into definite tenets and then to keep them from coalescence; thirdly, from insufficient documentation—we had almost said knowledge—drawn from the psychological work done on the problem in Germany; and fourthly, from incoherent structure, due apparently to too hasty writing, and to the desire of the author to include in his text, no matter how irrelevant, the findings from his own experiments. Such a sweeping criticism calls in turn for much documentation in order to be fair to the author; but to criticize a book of aesthetics adequately, especially when there are so many points to be taken up, demands the writing of another book. Rather than involve ourselves in slippery and illusive arguments without sufficient space to define them, we refer anyone seeking knowledge on the meaning of music to Professor Pratt's book—with the single warning to expect no more enlightenment than he has previously obtained from earlier music aestheticians.

One point only must be mentioned. If we have been able to grasp the structure of the second section of the book correctly, the primary aim is to show that the chief problem in aesthetics would be solved if more stress were laid on "Movement in Music" (p. 185)—a factor "badly neglected in most treatises" (p. 110). Granted that no one so far (and this includes Pratt) has given an adequate psychological explanation or description of the phenomenon, the topic has certainly not been neglected. From the time of Aristoxenus' *Harmonics,* through Nichelmann and Hanslick, to Ernst Kurth's *Grundlagen des Linearen Kontrapunkts* (1916) and his *Musik-Psychologie* (1931), the topic has been discussed extensively. The first of Kurth's books mentioned opens with the statement: *"Melodie ist Bewegung."* E. M. von Hornbostel has also mentioned the idea in his *Psychologie der Gehörserscheinungen,* where he has briefly developed more of its implications than does Professor Pratt.

Cornell University R. S. HILL

Clasificación y estudio estadístico de 3,719 alumnos, de las escuelas secundarias de la cuidad de México por medio de exámenes mentales. By MONTANA LUCIA HASTINGS. Mexico City, Publications of the Secretary of Public Education, 1929. Pp. 192.

While this study is primarily intended to demonstrate to educators of Mexico the value of testing and the mental measurement movement in general as it may be applied to schools, it has a secondary importance—the scientific value of the data it contains. The author throughout the report is imbued with the idea of demonstrating to the reader the application of statistical method as illustrated by the results of her study. This probably accounts for the inclusion of elementary phases of statistical treatment and for the omission of facts valuable to the scientist. But as an educational document in its setting it has importance. It is probably the first and only study of its kind in Mexico.

The subjects of the study were not selected to be representative of all children of the ages considered, in the Republic of Mexico; but they represent the population to be found in the first year of secondary schools, particularly of the City of Mexico.

One of the tests used was a translated Army Beta test. The author had begun using the Beta test on Mexican children in the United States while writing a master's thesis at the University of California. She is an American who has transferred her activities, in the study of Mexican children, from the United States to the Republic of Mexico. The same methods and technique were used in Mexico as in the United States. The same testers, trained by the author, tested all the subjects throughout this study. The other test used was the Otis Test for Reasoning in Arithmetic, which the author says was easily translated into Spanish with no change or apparent loss to the test—excepting where she had to use the word "kilogram" for the word "mile." The work was done under the auspices of the

Director of Secondary Education and with the encouragement of Professor Moises Saenz, Sub-secretary of Public Education.

In addition to the children of the first year of secondary school, there were 145 of the sixth year of primary instruction, and 189 native Indians in a school called "The House of Native Students." These 334 were deducted from the 3,719 in the larger number. In the remaining group of 3385 there are 2567 boys, and 818 girls. The years of testing extended through the school years of 1926-28. The 1926 students were not tested with the Otis test.

The reviewer was able to obtain the following data by putting the results of smaller tables into larger ones: Score on Beta of 2567 boys (mean age 14.1 ± 1.1 yr.) is 70.7 ± 8.5; and that of 818 girls (mean age 14.07 ± 1.11 yr.) is 65.6 ± 9.3. Score for total 3385 (mean age 14.1 ± 1.1 yr.) is 69.3 ± 8.9. Score on Otis of 1782 boys is 11.4 ± 2.1, of 796 girls is 9.4 ± 1.9. Score for total 2578 is 10.8 ± 2.1.

University of Denver THOMAS R. GARTH

Die Kunst. By BRODER CHRISTIANSEN. Buchenbach, Felsen-Verlag, 1930. Pp. 260.

Christiansen is the author of at least two other books on problems in aesthetics, and apparently has won for himself a certain skill in the juggling of concepts and the construction of systems. The present essay stems from Goethe's mention of *"die sinnlich-sittliche Wirkung"* of colors and from one of Kandinsky's manifestos, from which several terms and fundamental concepts are borrowed. Christiansen maintains that an artistic object possesses two sides: the apparent outer "sensations" of colors, vowels, and tones; and their corresponding spiritual correlates or *Innenklänge.* Blue, for instance, not only exists as the sensation of blue, but also possesses the *Innenklang* of cold, reserve, and distance. Vowels range from soft and round to sharp and pointed. A circle is cold and reserved, and therefore has through its *Innenklang* more relation to blue, than the *Innenklang* of blue has to that of any other color. These elements, however, seldom exist alone, but usually in compounds of other elements. Just as vowels and consonants are linked together to form a "grammar of art," so their *Innenklänge* are joined through logical relations to form a grammar. According to Christiansen, aesthetics has previously dealt only with the outer grammar of artistic objects, and has therefore missed the true solution. He proceeds to elaborate by means of his new elements a theory of the tragic and comic and of structural unity.

In so far as Christiansen stresses that peculiar phenomenon of the interrelation of disparate sensory fields (bright tones and bright colors, sharp tastes and odors), already noted by Von Hornbostel and Köhler (*Gestalt Psychology,* 1929) and in so far as his *Innenklänge* offer an explanation thereof, his book makes a sound contribution to the discussion. But except for occasional illuminating sparks, his elaboration of the subject is usually too far-fetched, too unreal. For instance: all art flows from God. The artist is merely His instrument, creating without knowing what he creates. The work of art is His attempt to better and to render more logical creations which were not altogether successful on the first trial. Such phrases give the impression that Christiansen has not only woven enough rope with which to do away with himself, but also unfortunately with those few good ideas which the book contains.

Cornell University R. S. HILL

Studies in Philosophy and Psychology. By G. F. STOUT. London, Macmillan & Co., 1930. Pp. xiii, 408.

Save for one paper, these studies are corrected reprints of essays, reviews, and critical discussions covering the period from 1888 to 1927 and done in the best manner of British Empiricism. Of particular interest to psychologists are the papers on "The Herbartian Psychology" and "Ward as a Psychologist." The first is the only historical essay in the volume and brings freshly to notice the main features of the *Lehrbuch zur Psychologie* and the *Psychologie neu Gegründet auf Erfahrung, Metaphysik und Mathematik.* The second (Chapter V) is both a review of and a critical commentary upon Ward's *Psychological Principles.* Much of the criticism centers upon Ward's interpretation of genetic psychology, a criticism which earns a gracious rejoinder by Mrs. (Mary) Ward (pp. 123-127).

Chapters II, III, and IV deal, respectively, with voluntary action (prompted by Shand's discussion of "Attention and Will"), the perception of change and duration, and the nature of conation and mental activity. Already in these chapters empirical psychology and philosophy are brought together; and through the rest of the book philosophy dominates. The topics discussed are as follows: the common-sense conception of a material thing, things and sensations, the way in which memory-knowledge may be immediate, Bradley's theory of relations, Bradley's theory of judgment, Bradley's theory of truth and falsity, Russell's theory of judgment, the nature of error, immediacy, mediacy and coherence, real being and being for thought, some fundamental points in the theory of knowledge, and the nature of universals and proportions.

With respect to the paper on conation and mental activity Stout is now inclined to treat of self as an embodied self rather than as purely mental, an interpretation that might have been put on the original paper. He has now changed materially his views on things and sensations, remarking (i) that the external object is external to the embodied self, (ii) that matter and mind are ultimately and essentially distinct but ultimately and essentially inseparable, and (iii) that sensible appearance may be material but not physical. These changes in Stout's thought are given detailed expression in his recent Gifford Lectures. The paper entitled "Bradley on Truth and Falsity" adds to and partially corrects the views expressed in "Bradley's Theory of Relations." In so far as the article on "Error" disagrees with the paper on "Real Being and Being for Thought," Stout would choose the latter. A definitive definition and description of "presentations" is given in the paper on "Some Fundamental Points in the Theory of Knowledge."

University of Illinois COLEMAN R. GRIFFITH

The Nature of Life. By EUGENIO RIGNANO. New York, Harcourt, Brace & Co., 1930. Pp. x, 168.

The late Professor Rignano has here brought to systematic completion a point of view, various aspects of which were developed in his earlier works: "The Inheritance of Acquired Characters," "The Psychology of Reasoning," and "Biological Memory." The book presents a militant, at times almost an evangelical, exposition and defense of an energetic vitalism, a vitalism based not on a belief in entelechies or vital forces but on the assumption of a dynamic nervous energy which belongs within a causal and deterministic system. Such an hypothesis, the author holds, represents the only logical escape from the equally untenable extremes of pure mechanism and pure vitalism.

Rignano's views are too well known to require restatement, and this book contributes little that is new. He advances once more the hypothesis of mnemic traces, which renders possible a belief in the inheritability of acquired characters, and defends it as the only possible explanation of the purposive nature of all living phenomena. He develops somewhat further, too, the notion of fundamental affective tendencies, discussed at length in his *Psychology of Reasoning,* and shows how necessary such an assumption is to an understanding of social progress. The greater part of the book, however, is devoted to the defense of the thesis that all living activity, however simple, is essentially purposive. To this end he assembles great masses of facts and quotations, ranging in their reference from such elementary physiological phenomena as assimilation and metabolism to such social manifestations as justice and morality. It is an imposing array. But even to one who is in general sympathetic toward his point of view it seems regrettable that there is so little of argument and so much of invective. Even Loeb, the only mechanist who receives the courtesy of extended quotation, is chided for his prejudice rather than confronted with a reasoned argument. To the reviewer it seems that this will prove simply another book which will confirm the beliefs of those who agree and will be dismissed with a shrug by those who do not.

Cornell University R. B. MacLeod

Geschehnis und Erlebnis. By Erwin Straus. Berlin, Springer, 1930. Pp. vi, 129.

The book introduces itself with the promise to start from the "pension neurosis" and to end there and to sandwich in pictures of the "problem of time," which the author, who is a psychiatrist, declares to be "the central problem of theoretical psychology." There is nothing new, practical-psychiatrical, which the American psychologist could learn about the pension neurosis from this book. But he can learn from it how strong the tendency of contemporary German psychologists (and psychiatrists, it seems!) is to speculate philosophically rather than to solve the problems of psychical abnormality by biological methods. Psychoanalytic methods, which few American psychologists will admit to be strongly biological, are declared by the author to be altogether too strongly biological. Therefore he builds up his "psychology" on a philosophy of the "time experience." Essentially he means thereby the fact that a human being conceives his present life in relation to the past and the future, whereas an animal is not capable thereof. Thus he can speak of a "psychical trauma" or "deformation" easily in cases where there surely is no trauma in a neurological sense. This time-theory he believes is a contribution to a better understanding of the "pension neurosis" which has worried the German economists since sixty years ago and which since the creation of millions of world war veterans is worrying increasingly the economists of all the nations.

University of Missouri Max F. Meyer

L'esprit et ses maladies. By Marcel Nathan. Paris, Rieder, 1930. Pp. 80, 60 illus.

Whoever is interested in the abnormalities of human behavior will find here much that is fascinating. Sixty pages are devoted to the reproduction of photographic studies which are quite remarkable. The reviewer cannot at this moment recall ever having seen any collection quite so satisfying. Not only were the subjects well selected but all the techniques of photographic skill were drawn upon to make the effects as nearly life-like as possible. In some instances several photographs taken in quick succession are arranged in order upon the page, giving almost as good an idea of the action involved as would a cinematograph. And just about everything one could ask for is here, from reproductions of old drawings and artists' conceptions to encephalitic sequelae.

The text, of which there are but seventy pages, is much less attractive and important. It is merely a running description of the different forms of mental abnormality with a little about the ways of interpretation. The catholicity of mind and the scholarship revealed make pleasant reading. It is the sort of thing one would like to have available when one is asked by a person of intelligence and education for a brief reliable statement about the general field of psychopathology.

University of Oregon EDMUND S. CONKLIN

Philosophia Perennis. Abhandlungen zur ihrer Vergangenheit und Gegenwart. Festgabe Joseph Geyser zum 60 Geburtstag. Vol. I. Abhandlungen über die Geschichte der Philosophie. Vol. II. Abhandlungen zur systematischen Philosophie. Edited by FRITZ-JOACHIM VON RINTELEN. Regensburg, Josef Habbel, 1930. Pp. xviii, 525; x, 719.

The two volumes of this work consist of essays by representative Catholic scholars from all parts of the world (in German, French, Italian, Spanish, and English) which are published as a memorial of the sixtieth birthday anniversary of the Munich philosopher and psychologist, Joseph Geyser. America is represented by Professor Moorhouse F. X. Millar, of Fordham University with an essay entitled, "The Significance of St. Augustine's Criticism of Cicero's Definition of the State;" and England by M. C. D'Arcy and Leslie J. Walker, both of the University of Oxford with essays entitled respectively "Probability and Certainty," and "The Bearing of Modern Physical Theories on Catholic Philosophy."

In accord with the life-long attempt of Geyser to interpret the results of empirical science from the general platform of Aristotelian realism, the present work is divided into three parts, beginning with a sub-division devoted to problems in the history of philosophy, which is followed by a systematic part concerned with problems in the field of logic, epistemology, ontology, natural philosophy, psychology, ethics, and metaphysics. The third part of the collection consists of three essays, which attempt to evaluate the work of Geyser himself in the fields of psychology, epistemology, and metaphysics. Psychologists will be especially interested in the research and observations on emotions and sentiments by Agosto Gemelli, professor of experimental psychology at Milan, the psychology of thinking by Richard Hönigswald of the University of Freiburg, the psychology of value by Martin Honecker of the University of Breslau, and the discussion of Geyser's own contribution to psychology dictated by the late Max Ettlinger of the University of Münster from his deathbed.

Ettlinger calls attention to the fact that Geyser's philosophy and psychology developed through intercourse with the circle of students gathered at the University of Munich, for which Theodor Lipps, an "unusually stimulating teacher and personality" was the center. Lipps himself was struggling at the time with problems which were leading him away from his earlier psychologism in the direction of a metaphysics of mind which was especially congenial to Geyser. Geyser in his own development finds no conflict between empirical psychology of the type presented by Lipps and Brentano and a philosophical psychology, since, in his opinion, any psychological method, when carried out thoroughly, leads ultimately to metaphysics. In his appreciation of the experimental results of the Würzburg School, as well as in his utilization of the results of modern biological research, Geyser gives a concrete illustration of this doctrine. There is a brief discussion of Geyser's more recent contributions to the field of religious psychology which grew out of his criticisms and discussions of the work of Rudolf Otto and of Max Scheler. The two volumes are bound appropriately in scarlet cloth and are fine examples of the printer's art.

Vanderbilt University HERBERT SANBORN

Enriching the Curriculum for Gifted Children. By W. J. OSBURN and BEN J. ROHAN. New York, The Macmillan Co., 1931. Pp. xiv, 408.

The authors start with a clear, though perhaps somewhat exaggerated and not entirely unemotional, account of the harmful consequences which result in the ordinary school when all the children are given the same assignments and are expected to complete them within a given time regardless of their interests and abilities. The gifted child is described as possessing a large amount of energy, a strong emotional urge, and the ability to see likenesses and differences. The descriptions leave the impression that these qualities are entirely lacking in the dull child.

Considerable space is given to emphasizing the importance of taking into account the different types of ability in the selection of gifted children and that class standing is "very unreliable as an earmark of brightness." Yet in their own experiment the authors used class standing and the results of one reading test as the sole measures of the brightness of the child. The plan was to allow each pupil who was up in his school work to join a "hobby" club which met once a week for the study of some subject which was of special interest to him. The remainder of the pupils spent this period in remedial work. However, all but five or six pupils qualified for the "hobby" clubs before the end of the year, which leads one to question the appropriateness of the title.

Though the contrast between the theoretical and experimental sections of the book is disappointing, the reader will, nevertheless, find within its pages many constructive suggestions for meeting the individual needs and interests of his pupils.

Harvard University PSYCHE CATTELL

Foundations of Abnormal Psychology. By FRED A. MOSS and THELMA HUNT. New York, Prentice-Hall, 1932. Pp. xii, 548.

The authors distinguish two classes of mental disorders, the true ones and those which are pseudo-disorders. They mean by the former those which have a foundation in tissue pathology, by the latter those which are bad habits of behavior and can be removed by reëducation. They issue a warning against recognizing the latter as diseases (p. 72): "Man is by nature interested in the occult, . . . this was seen in the wide acceptance of the teachings of Mesmer and more recently in the case of Freud. . . . In Los Angeles at the present time we can demonstrate as grotesque superstitious methods of dealing with mental disorders as have been so far reported in the history of psychology."

The book is divided into two parts, the first being a "general consideration" and the second a "consideration of specific diseases." The first part contains most of the historical matter, the discussion of "what is normal," and the general possibility of treatment. The second part considers the different kinds of bacterial infection, the different exogenous and endogenous toxins, glandular and nutritive disturbances, various pathological tissue abnormalities, the mental diseases of which the causes are virtually unknown, and those disorders which the psychoanalysts try to cure by their special methods.

Those who are still accustomed to find in a book on abnormal psychology chiefly discussions of the occult, the mysterious, the unconscious, will be disappointed, for none of those subjects are treated here, save to the extent which is necessary to give reasons for their omission. The book is intended for readers who accept the idea that scientific abnormal psychology and scientific psychiatry are essentially the same thing.

University of Missouri MAX F. MEYER

Children at the Crossroads. By AGNES E. BENEDICT. Drawings by Cyrus LeRoy Baldridge. Foreword by Howard W. Nudd. New York, The Commonwealth Fund, Division of Publications, 1930. Pp. 238.

This is a popular book, without references or index, intended to illustrate and emphasize by the use of appropriately selected cases certain general principles given in the summaries. The book is the first to present individual case-stories of maladjusted rural children treated by the visiting teachers in the program of the National Committee on Visiting Teachers. This Committee was created in about the year 1922 to administer for the Commonwealth Fund a program of thirty demonstrations of visiting teacher work. The two earlier books issued by the same fund, *The Problem Child in School,* by Mary B. Sayles, and *The Visiting Teacher at Work,* by Jane F. Culbert, deal with special aspects of the work of this Committee, of which Mr. Howard W. Nudd is chairman. The "cases" and drawings are interesting, and the book will doubtless increase the imagination and resourcefulness of teachers, parents, and social workers. J.P.

Magische Geisteshaltung im Kindesalter und ihre Bedeutung für die religiöse Entwicklung. By KARL ZEININGER. Beiheft 47 Zeitschrift für angewandte Psychologie; Beiträge zur Jugendpsychologie, No. 1. Leipzig, Johann Ambrosious Barth, 1929. Pp. x, 155.

The problem of the study is to trace the religious behavior of the individual from its early manifestation in the child to its adult form. The writer believes that in the child as in the race religious responses develop from the non-religious in the presence of the mysterious; early responses are classed as magic, taboo, sacrifice, and the like. The religious conceptions are drawn from such writers as Rudolph Otto, Karl Girgensohn, and Nathan Söderblom.

The monograph is stimulating in its attempt to cover scientifically certain aspects of life not ordinarily included in a treatise on human nature. The method of quoting incidents from a wide field illustrates but hardly proves what the writer has undertaken.

Florida State College for Women P. F. FINNER

The Activity Movement. By CLYDE HISSONG. Baltimore, Warwick & York, 1932. Pp. x, 122.

This dissertation is a critical appraisal of the psychological foundations of progressive education. Free activity as an instrument is accepted, but freedom as a goal is held to be an inadequate basis for education. On the other hand, Thorndike, who is regarded as typical of mechanistic and scientific psychology, is criticized for stressing unduly the possibility of deliberately controlling the learning process by manipulation of the environment, while neglecting the suggestion as to desirable goals which derive from a study of the developing potentialities of the young child.

Ohio State University H. B. ENGLISH

The Principal Nervous Pathways. By ANDREW THEODORE RASMUSSEN. New York, The Macmillan Co., 1932. Pp. 73.

This atlas contains a valuable series of neurological charts and schemas, accompanied by concise explanations. Although designed primarily for use in courses in neuro-anatomy in the medical curriculum, psychologists and students of physiological psychology will find the book of great value. All of the sensory and motor systems, including the autonomic, are presented in great detail and with admirable clarity. The reviewer knows of no other atlas which contains such complete and convenient schematization of the nerve tracts.

Vanderbilt University LYLE H. LANIER

Cosmic Consciousness: A Study in the Evolution of the Human Mind. By RICHARD MAURICE BUCKE. New York, E. P. Dutton & Co., 1931. Pp. xviii, 384.

This new (the seventh) edition should prove a source of considerable enjoyment to the devotée of solipsism. A notice is justified only from the slight danger that the title may lead the uncritical layman to judge from it that the book makes a contribution to psychology.

Western Kentucky State Teachers College MILTON B. JENSEN

Social Psychology: The Psychology of Attraction and Repulsion. By JOHN JEFFREY SMITH. Boston, Richard G. Badger, 1930. Pp. xxv, 468.

This book is intended as a text for students of social psychology. Social psychology is considered as the study of social attraction and repulsion. Personal as well as group relations are considered. The book is a compendium of opinions culled from a host of authorities. These authorities are for the most part non-scientific and little space is devoted to the presentation of experimental results. Each chapter is followed by exercises. There is a bibliography of 252 titles and a complete index.

University of California CALVIN HALL

Safety and Health in Organized Camps. By J. EDWARD SANDERS. New York, National Bureau of Casualty and Surety Underwriters, 1931. Pp. viii, 133.

This volume reports an investigation into the frequency and causes of accidents and illnesses in a large group of children's summer camps, and includes tentative recommendations as to administrative procedures. It bears only a slight relation to academic psychology.

Institute for Juvenile Research, Chicago LUTON ACKERSON

Essays on Marriage. By FREDERICK M. HARRIS. New York, Association Press, 1931. Pp. vi, 208.

A collection of unfinished manuscripts posthumously edited by Harrison S. Elliott form the background for an orderly presentation of the primary problems of marriage. The breadth of analysis and depth of insight is based on broad reading and practical experience in counseling. A comprehensive survey of the present status of marriage and attitudes, customs and laws lead the writer to define marriage as a partnership. "The personal relationship involved in marriage is on the way to success when the depth and range of the interests shared between the two partners are sufficient to make the common enterprise significant and rewarding to both." This functional definition of marriage is discussed primarily about the crucial elements of "the strictly sexual relationships, the economic adjustment, and the children."

The most significant work is contained in the central chapters on "The Relation of Sexual Desire and Conjugal Affection," which are frankly discussed as entities—the former as an individual function and the latter as growing out of social experience in the family—and "the sexual relationships of marriage," which while based upon the sound basis of physiological function is understandable primarily in light of a breadth of sound knowledge on the part of the reader. It is this presumption which is apt to leave the average reader with little but a satisfactory two hours' reading in a field in which all are interested. From these central chapters on the discussion of the results of ignorance and silence the author goes on to a rather innocuous argument about selection of mates and the "limitation" of the family.

University of Pennsylvania R. A. BROTEMARKLE

Our Knowledge of Other Minds. By W. WYLIE SPENCER. New Haven, Yale University Press, 1930. Pp. 145.

A logical analysis is made of numerous mental characteristics usually taken as criteria of the existence of other minds. The following apparent evidences are examined reflectively before being rejected: the concept of a mind associated with a body; instinct; consciousness; sensory experience; mental process; emotions; and noetic powers. The argument from analogy of the existence of other minds is cast aside after a cursory treatment. The evidence most convincing to the author is the recognition of one's social conduct. We know other minds, it is claimed, in so far as we know that we are coöperating with other human beings in activity of such a nature as to call upon our full measure of conscious activity. "This explanation rests on the conviction of the existence of a social order in which one plays one's part among similars." Obviously, the above mentioned coöperation and conviction are the common possession of all; undoubtedly they support the argument from analogy of the reality of other minds. In general, the arguments are derived from the older concepts of psychology at the expense of the late experimental findings and the resulting concepts.

University of Utah M. C. BARLOW

Cours de Philosophie. I. Manuel de philosophie à l'usage des classes de Philosophie et de première supérieure. By A. CUVILLIER. Paris, Armand Colin, 1931. Pp. xxxiv, 722.

The volume is intended to serve as a general introduction to classes in philosophy and mathematics in the schools which prepare students for institutions of higher learning in France, and is followed in the complete work by a second part, whose sub-title is *Logique, Morale, et Philosophie générale.* The author believes that through an introduction to the problems of philosophy by way of psychology the student will be furnished with a concrete foundation of material for later theoretical discussions which might otherwise degenerate into pure speculation. With this in mind a comprehensive survey of the general and special methods in use by psychologists today is given, hand in hand with a critical discussion of the data furnished by a large number of representative investigators in all fields. The American psychologists from whom material has been drawn are James, Jastrow, Titchener, Warren, Watson, and Yerkes.

Vanderbilt University HERBERT SANBORN

A Philosophy of Reality. By E. L. YOUNG. Manchester, Manchester University Press, 1930. Pp. xi, 266.

This book begins with the thesis that "things are what they seem." If ether vibrations are colored then a brain, in so far as it vibrates in the same way, is colored. So with all sensations. The objective world is made up of "marketable sensations" whereas a mind is erected upon a group of sensations that are no longer marketable because internal to the body. The essay then moves on through a study of spiritual life, the five senses, time, space and number, good and evil, and progress; but there is neither a continuous argument nor a suggestion of systematic thinking. A few names are mentioned in the text without further reference and there is no index. The author's philosophy of reality has a very unreal tang.

University of Illinois COLEMAN R. GRIFFITH

Marie Louise: Napoleon's Nemesis. By J. ALEXANDER MAHAN. New York, Thomas Y. Crowell Co., 1931. Pp. viii, 364.

The writer of this entertaining book attempts to show the influence upon Napoleon's downfall of Marie Louise's subconscious hatred of the French and Napoleon. He gives many references to documents and literature, but his psychology is naïve, expressing itself in terms of the prenatal influences which caused Marie Louise's attitudes and mentioning the various "parts" of her personality. At the same time, such well known factors as Napoleon's upbringing, his compensation mechanism for his small stature, and his physical disintegration are ignored.

Institute for Juvenile Research, Chicago LOWELL S. SELLING

Das Generationsproblem in der deutschen Dichtung der Gegenwart. By HEDWIG KOCH. Pädagogisches Mag., Heft 1308. Lagensalza, H. Beyer & Sons, 1930. Pp. 116.

The author believes that literature throws valuable light upon the problems arising out of the opposition of two generations. In contemporary German literature she finds the father-son conflict to be characterologically the most important and aesthetically the most impressive. The revolt of modern youth as expressed in literature shows itself in the attitude (1) toward members of the older generation individually, (2) toward the world of that generation with reference to (a) family, (b) school, (c) profession, (d) state.

The youthful generation is divided into four characterological types (1) protesting, (2) warring, (3) adapting, (4) resigning, and the influence of the *Zeitgeist* in the formation of these types is discussed. A bibliography is included.

University of Iowa BONNO TAPPER

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HABIT-INTERFERENCE AS DEPENDENT UPON STAGE OF TRAINING

By ELSA M. SIIPOLA and HAROLD E. ISRAEL, Smith College

When two incompatible habits are learned successively so that interference appears in the learning of the second habit, it seems only natural that the stage of training reached in the first habit before shifting to the second should be an important condition influencing the amount of interference. Various researches have shown that the amount of interference is somehow related to the amount of previous training, but as to the direction and the exact nature of the relationship the evidence is inconclusive. Some investigators have found that more training leads to greater interference, while others have found, in direct contradiction, that the amount of interference bears an inverse relation to the amount of previous training. The aim of this research was that of determining directly the nature of the relationship between amount of interference and stage of previous training. A feature of the method was the use of measured proficiency rather than mere frequency of repetition as the criterion for the stages of training.

HISTORY OF THE PROBLEM

This review of the history is limited to the material which deals directly with the specific problem under consideration. Within the tremendous literature on the general topic of interference the point of a relationship between amount of interference and stage of previous training is made more often the subject for casual reference or easy assumption than for experimental treatment. Such evidence as is available comes in many cases from incidental data obtained in the investigation of some other aspect of the general topic.

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Münsterberg was one of the earliest experimenters to touch upon the problem of this paper.[1] He investigated interference by altering automatic daily habits such as the placing of the watch in a certain pocket. He found that with continued practice in the new habit the interference gradually disappeared as the new habit became automatic and replaced the old. With repeated shifting from one habit to the other the mutual interference diminished with each new shift and gradually disappeared entirely. Münsterberg's experiments gave no direct evidence upon the problem of the manner in which different amounts of separate practice in the first habit affect the amount of interference; it is strange that Münsterberg's work has been rather commonly cited upon this specific point.

Bair attempted to find out "whether a habit which has been made permanent with many practices offers more resistance to displacement by a different habit than one formed by only a few practices."[2] Alternative forms of a simple place-habit (*e.g.* card-sorting) were used as the conflicting tasks. Each *S* had first a single trial of the first task, then a single trial of the second task, then 10 repetitions of the first, and finally one more trial of the second. Comparison was made between the amount of interference (increased time) at the first shift following a single trial and the amount of interference at the second shift following 10 trials of the first task. The results showed consistently less interference for the shift after 10 repetitions. In further experiments Bair extended his series to include practice periods of 1, 10, 20, 30, and 40 repetitions between the successive shifts to the second task. The results showed that the greater the number of repetitions of the first task preceding the shift, the smaller was the amount of interference-effect. Bair concluded that interference decreases proportionally to increase in practice of the first task, and that long continued practice may have an actual facilitating effect upon the performance of the second, antagonistic task (p. 45). In drawing these conclusions Bair failed to mention a fact which may have been responsible in large part for the nature of the results obtained. The arrangement of the series in the experiments provided practice in shifting from task to task along with practice in the first task; each *S* went through the entire series. That practice in shifting from task to task is effective in decreasing interference had been shown by Münsterberg and by Bair himself in an earlier experiment. Bair's results, then, justify no simple conclusion as to the nature of the relationship between interference and practice in a first task.

Culler obtained some data upon our problem by the use of choice-reactions.[3] For the first task the *S* reacted to a red light with the right hand and to a blue light with the left; for the second task the hand-color relationships were reversed. One group of *Ss* had 140 reactions for each hand before shifting to the second task while the other group had only 60. Interference was measured in terms of the differences in reaction-time between the last 30 trials of the first task and the first 30 trials of the second: The results showed definitely more interference for the group with the greater amount of training in the first task. Culler's conclusions are in direct contradiction to those of Bair.

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[1] H. Münsterberg, *Beiträge zur experimentellen Psychologie,* 1889, IV, Pt. I.

[2] J. H. Bair, The practice curve: A study in the formation of habits, *Psychol. Monog.,* 5, 1902, (no. 19), 35.

[3] A. J. Culler, Interference and adaptability, *Arch. Psychol.,* 3, 1912, (no. 24), 1-80.

Culler's conclusions should be examined carefully with consideration for the manner in which the amounts of interference were determined. In customary usage interference is said to occur when practice in one task makes the performance of a second, incompatible task more difficult or less proficient than it would have been without the previous practice. With speed of reaction as the measure of proficiency, the customary manner of determining amounts of interference would be that of comparing speed at a given stage in the learning of the second task with speed at the same stage in original learning of that task by a control group or, if the two tasks are of the same inherent difficulty, with speed at the same stage in the learning of the first task. Culler, however, compared initial speeds in the second task with final speeds in the first task. His two groups with different amounts of practice arrived at widely different levels of speed in the first task before they were shifted. After the shift the speed for the long-trained group fell far below the high level finally reached by this group in the first task, while the speed for the less-trained group showed less decline from the relatively low level at which it finished the first task. On the basis of this comparison the long-trained group was said to show more interference. This manner of treatment implies a conception of interference quite different from the usual one, and logically it involves the assumption that there is to be expected always, unless interference occurs, an amount of positive transfer in exact proportion to the amount of improvement during practice of the first task. Such an assumption seems wholly unwarranted, and its introduction leads to obvious difficulties. Suppose that the two groups in an experiment similar to Culler's should each start the second task at an initial speed exactly the same as the initial speed for the first task; according to customary notions this case would show an obvious lack of interference for both groups since in each case the new task was performed just as if there had been no previous practice. Subjected to Culler's treatment this case would show that the long-trained group had much more interference. Culler's conclusions, then, must be taken to be strictly dependent upon his method of treating the results, and they can not be regarded as significant for the problem taken in its customary sense.

Kline used paired associates to investigate the relationship between the strength of an original associative bond and the difficulty with which a new association could be substituted.[4] For the original associations such familiar combinations as states and their capitals were used, and the strength of the bond was measured by the number of individuals in a group able to recall the second member of the pair. Interference in the learning of the new association was measured by the increase in reaction-time for the 'false' associates as compared with the times for the original responses and for those of a control series. Kline concluded that interference is maximal when the original bond is of medium strength, that there is less interference when the bond is very strong, and that there is little or no interference when the bond is weak. Kline's results are difficult to compare with those of other researches since his material is so different and especially since strength of the original connection is treated statistically as group frequency.

Wiltbank gave white rats various amounts of training in one maze, then shifted them to a second maze and compared the readiness with which the differently trained groups learned the new task.[5] Errors, time, and number of trials were used as measures, and percentages of interference or transfer were computed in comparison with the figures for the control group. In terms of errors and number of trials required, interference is definitely shown for the less-trained groups (2 to 8 trials in the first maze), while positive transfer appears for the groups with much practice (16 or more trials). In total time required for learning the second maze, all groups showed a high transfer-effect (75% to 95%) with only slight differences between the groups. In general, Wiltbank's results indicate an inverse relation between interference and training in the first task (in agreement with Bair and to some extent with Kline), although it is difficult to explain the consistent transfer-effect shown by the figures for total time.

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[4] L. W. Kline, An experimental study of associative inhibition, *J. Exper. Psychol.*, 4, 1921, 270-299.

Hunter trained rats in light discrimination and then reversed the direction in which they were required to run.[6] One group was given 100 repetitions of the first habit before the shift, and another group was trained until the responses were 95% correct (an average of 286 trials). The members of the highly-trained group required on the average 603 trials to learn the reversed task, while the less-trained group learned the second task in an average of 475 trials. Hunter concluded from his results that when the first task is approximately half learned, the amount of interference is about one half of the total possible amount which results if the first task is completely learned. The results hardly justify the drawing of such a precise relationship, since 'half-training' was not determined by proficiency but was merely implied by the fact that the less-trained group had a uniform number of trials below one half of the average number required for complete learning. However, Hunter's results do indicate that a higher degree of training in the first task results in a greater amount of interference (in agreement with Culler).

The investigations reviewed above seem to include all of the direct evidence available upon our specific problem. There is a striking lack of agreement in the conclusions reached, and the results are not highly comparable. Although a great variety of materials and procedures makes comparison difficult, a more serious difficulty in comparison arises from the fact that stages of training were generally determined by arbitrarily chosen numbers of repetitions and from the lack of uniformity of terms and of places in the curves used for measuring interference. These investigations serve chiefly in offering suggestions for improvement in methods of isolating and attacking the problem.

Some theoretical considerations seem sufficiently important for the problem to be mentioned here. The significance of much of the former work on problems of learning has recently been questioned by psychologists of the *Gestalt* school. In the light of this new criticism the problems have been formerly approached from the standpoint of untenable assumptions which may be termed 'associationistic' in that specific linkages or bonds between elements have been assumed to determine reproduction.[7] In connection with the topic of interference, the associationistic approach is reflected in the law of associative interference (stated in 1894 by Müller and Schumann) which puts the matter into terms of a tendency for an established bond between two elements to inhibit the formation of a similar bond between either and a third element. The *Gestalt* approach questions the existence of an active associative tendency which acts as the primary force (*Motor*) to determine reproduction and which must interfere with any attempt to alter the reproductive sequence.

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[5] R. T. Wiltbank, Transfer of training in white rats upon various series of mazes, *Behav. Monog.*, 4, 1919, (no. 17), 1-65.

[6] W. S. Hunter, Habit interference in the white rat and in human subjects, *J. Comp. Psychol.*, 2, 1922, 29-59.

[7] W. Köhler, *Gestalt Psychology*, 1929, 269-347.

While it is true that early associationists, especially those in Germany, regarded association as an active tendency, such a view is extremely uncommon among later psychologists who might be called the associationistic group. In fact, a more pertinent criticism might concern itself with the vagueness of the present concepts of association and with the fact that the associative tendency has become so static as to be regarded, in extreme views, as a tendency only in the statistical sense of probability. At any rate, we may derive from this *Gestalt* protest the warning that interference is not to be thought of as the inevitable consequence of a persistent force set up by previous association and operating on every occasion to resist change in the original sequence of events.

A second point of protest, one much more directly related to the problem under consideration, is the suggestion that mere frequency of repetition is not important among the conditions determining reproduction and recall. The emphasis of frequency to the exclusion of other conditions in learning is certainly more properly considered a feature of behaviorism than of associationistic thinking in general; the associationists have resisted stubbornly the attempts to eliminate such meaningful conditions as similarity and contiguity from the lists and laws. There is no prominent current theory which does not recognize the importance of incentive, set, or the determining tendency in some form.

However innocent the current associationistic theories may appear of the charges brought against them, it must nevertheless be admitted that the assumptions considered objectionable are often implicitly present in the experimental work. In the work on interference, frequency of repetition is generally accepted alone as an adequate measure of the effective strength of a habit or of the stage reached in the learning process. The prevalence of this practice may be judged by the frequency with which it occurs in the few researches cited above.

The positive suggestions of the *Gestalt* psychologists toward an approach to problems of learning (and specifically to problems of interference) are taken by the writers to be approximately as follows.[8] The primary determinants of reproduction are, first, the total organization of the conditions (notably incentives and set) under which the items occur together originally *plus* such changes as may occur in this organization during the interval before reproduction, and, secondly, the total organization of the conditions at the time of possible reproduction. If these two organizations are congruent, immediate reproduction will occur; if the two are dissimilar, reproduction will fail. Interference, if it is actually experienced or shown by errors in performance, is a result of conflicting stresses set up at the time of reproduction owing to the presence of two conflicting tendencies toward reproduction. To affect reproduction each of the tendencies must be a reproductive tendency. Association (by frequency of repetition) should refer to a mere mechanical condition by which the *strength,* but not the direction or character, of an organized reproductive tendency may be influenced. Hence, frequency of association can affect interference only indirectly by making the trace of the organized whole (reproductive tendency) more stable and enduring.

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[8] Köhler states his unwillingness to attempt the formulation of a complete theory of reproduction. The points mentioned here are those to which Köhler gives provisional acceptance.

In support of such a view some evidence has been presented which has direct bearing upon our problem. Lewin performed experiments in which he demonstrated the relative unimportance of frequency of repetition.[9] When conditions were established during the practice period such as to encourage the 'wrong' reproductive attitude, decided interference occurred in the test period despite the small number (4 to 6) of repetitions. When practice conditions favored the proper reproductive attitude, there was no interference with the formation of a potentially conflicting association even though a great number (200 or more) of practice repetitions were given. Schwarz investigated interference by reversing one act in a complicated motor task.[10] By altering the difficulty of the other portions of the whole performance, he was able to regulate the *S*'s attitude (attentive spread). Schwarz found that direction of attention away from the crucial act greatly increased errors of reversion. Pauses for introspective report were also effective influences. Schwarz concluded that the organization of the total field is of primary importance in the determination of interference-effect.

On the side of theory, then, there are two directly opposed views as to the nature of interference. The researches which have given data upon our specific problem have for the most part followed in the associationistic tradition which leads to a formulation in terms of connections between items. A further point, not necessarily inherent in the associationistic approach but prominent in the experimental technique of these investigations, is the acceptance of frequency of repetitions as the sole determinant of the strength of connections and hence of their effectiveness in causing or resisting interference. This general approach, applied to our specific problem, holds promise of the existence of a constant relationship between amount of interference and stage of previous training and would logically imply that the relationship is more likely to be positive than inverse. The results obtained, however, are inconclusive. Although some relationship was found in all cases, Culler and Hunter found a positive relationship while Bair, Kline, and Wiltbank found evidence for a negative relationship. The anti-associationistic or *Gestalt* view suggests a formulation of the problems of interference in terms of conflicting reproductive tendencies or organizations each dependent upon conditions of the total situations. The character of these organizations depends not at all upon the amount of training; if mere amount of training has any effect upon interference it must be the result of an influence upon the strength or stability of the organizations. Hence the *Gestalt* view holds little promise for the finding of any constant relationship between amount of interference and stage of previous training. The work of Lewin and Schwarz has demonstrated the effectiveness of other factors in overshadowing the influence of mere practice. However, even from the standpoint of this critical view, the problem under consideration assumes sensible importance if it can be dealt with in terms of actual stages of habit-formation, effective strengths of the organizations, rather than in terms of frequency of repetition.

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[9] K. Lewin, Das Problem der Willensmessung und das Grundgesetz der Assoziation, *Psychol. Forsch.,* 1, 1921, 191-302; 2, 1922, 65-140 (discussed by Köhler, *op. cit.*). Lewin takes as a point of departure the work of N. Ach, *Ueber den Willensakt und das Temperament,* 1910. Ach's view was that the strength of a determining tendency (reproductive) could be measured by placing it in opposition to an associative tendency. By increasing the number of repetitions the associative tendency could be strengthened until the interference was overcome. Ach, and others of the Würzburg school, seemed content to demonstrate the existence of other tendencies as well as that of association by frequency; the experimenters of the *Gestalt* school are intent upon showing that association by frequency is of minimal importance.

[10] G. Schwarz, Ueber die Rückfälligkeit bei Ungewöhnung, *Psychol. Forsch.,* 9, 1927, 86-158.

THE EXPERIMENT

The problem lends itself to a direct approach, that is, to the straightforward procedure of providing training in one task to any desired stage, then shifting the *S* to the learning of a second task and measuring the amount of interference in terms of the relative amount of difficulty encountered. In view of the apparent inadequacies of method found in the previous investigations, especial effort was made toward improvement of the experimental technique for this research along three lines: in the selection of the tasks, in definition and measurement of stages of training, and in provision of adequate measures of interference-effects. Telegraphic sending was selected as offering the greatest possibility for two conflicting tasks of practically equal difficulty (two forms of a single code) in which the factor of manipulative skill could be isolated and controlled. As the most exact and significant definition and measure for stages of habit-formation, actual proficiency was chosen. In the attempt to secure more adequate measurement of interference-effects, provision was made for comparing the course of improvement in the two tasks up to a definite stage of proficiency with respect to all of the various aspects of the process including time, errors, and total trials. Training in the second task was continued beyond the point of bare mastery, in order that interference-effects might be detected if they should persist or appear for the first time during the later stages.

MATERIALS AND PROCEDURE

In the two forms of the code used the first 10 letters of the alphabet were represented by 10 combinations of dots and dashes. Toward securing equality in difficulty, the same 10 'dot-dash' combinations were used for both forms. A further check upon possible inequality of the two forms was provided by starting half of the *S*s in each group with one form and the other half with the other. Since the two forms were made up of the same 10 letters and the same 10 symbols, the incompatibility of the two tasks lay entirely in the pairing of letters and symbols.

The two forms of the code were arranged after many preliminary experiments. The length was fixed at 10 letters in order to allow for complete learning of one task within an experimental period. The symbols consisted of two, three, or four units (dots and dashes) each. Care was taken that neither of the symbols assigned to any letter should closely resemble the Morse symbol for that letter; a surprisingly high proportion of women college students admit having had some earlier acquaintance with conventional telegraph codes. For the purposes of the experiment it was desirable to find two forms which would give maximal interference, and much tedious experimentation was done upon this point. It was found that transfer rather than interference occurred if the two symbols for the same letter were similar either in number of units (*e.g. dot, dash* and *dash, dot*) or in general pattern (*dot, dash* and *dot, dash, dot, dash*). The symbols finally adopted for each letter differed in the two forms in both number of units and general pattern.

	Form X	Form Y
A =	*dot, dash, dash, dash*	*dash, dot, dash*
B =	*dash, dash*	*dot, dot, dash, dot*
C =	*dash, dot, dash*	*dot, dash*
D =	*dash, dot, dash, dot*	*dot, dash, dash*
E =	*dot, dash, dash*	*dash*
F =	*dot, dot, dot*	*dash, dot, dash, dot*
G =	*dash*	*dot, dash, dash, dash*
H =	*dot, dash*	*dash, dot, dot, dash*
I =	*dot, dot, dash, dot*	*dash, dash*
J =	*dash, dot, dot, dash*	*dot, dot, dot*

The *S* was seated at a table upon which a standard telegraph key was permanently mounted. For each trial she was required to tap out as rapidly as possible the 10 symbols for the 10 letters as arranged on a typed list. Whenever prompting was required, the *S* pressed a second key which revealed a complete chart of the code, both letters and their symbols. She was required to press this prompting key also whenever an error occurred. Both keys were operated with a single hand and were placed at a distance of about 10 in. apart. Each occasion of prompting or of error was thus represented within the single time record for the trial by an approximately uniform increment. An electric counter recorded the number of times the prompting key was pressed. The *E* presented the lists, timed the trials, and kept her records from a position behind a screen. Experimental periods were approximately an hour in length and for each *S* there was one period of experimenting on each successive day. The 20 *S*s were women students with the exception of 2 who were instructors. None of the *S*s who were used in preliminary experiments took part in the experiment proper.

PRELIMINARY TRAINING

In order to eliminate the influence of improvement in mere manipulative skill, improvement which would show up as transfer-effect, all *S*s were given intensive preliminary training in the sending (copying) of the dot-dash combinations to be used as symbols and in the manipulation of the prompting key. This training was continued until after improvement in speed had ceased and the *S* was apparently at her limit of motor speed.

The preliminary training followed the general procedure of the main experiment except for the fact that the lists presented to *S* were made up of the dot-dash combinations to be copied directly. Twenty-five practice lists were used, each having the symbols in different order. Some of the lists were placed in the prompting box so that the prompting key had to be pressed after the copying of each symbol. In order to motivate the *Ss* the *E* informed them that the same dot-dash combinations were to be used as symbols in a code to be learned later. Training of the *Ss* to what seemed their limits of improvement required from 60 to 120 repetitions. At the final speeds reached the *Ss* required only from 8 to 10 sec. for copying each list.

For the purpose of detecting any further improvement in mere motor speed, which might occur during the course of the main experiment, provision was made for the copying of four practice lists during each of the later experimental periods. It was possible, therefore, to compare each *S*'s speed of sending during the learning of the second task with her speed during practice in the first task. Comparison of figures for the last four lists in the preliminary training with those for the four lists on the last day of practice in the second task, that is, comparison of speed at the beginning and at the end of the experiment proper, showed only slight differences. Five *Ss* lost slightly in speed, 10 improved slightly, and 5 showed absolutely no change. Since the results are treated later in terms of the differently trained groups, comparison here is more significant in those terms. In group average time for a trial the Half-trained Group improved 1.3%, the Barely-trained Group 0.2%, the More-trained Group 4.9%, and the Most-trained Group 6.2%. Mere manipulative skill improved only very slightly during the experiment, and the order and direction of the changes in motor skill are such that they could have had no significant influence upon the large differences between groups shown in the experiment proper.

TRAINING IN THE FIRST TASK

On the basis of experience gained in preliminary experiments, four stages of training in the first task were selected and were defined in advance in terms of proficiency. The *Ss* were divided into 4 groups of 5 each.

Groups with brief training:

(1) Half-trained. Training stopped as soon as five of the ten symbols were correctly produced in a single trial. (The designation for this group is not to be taken literally.)

(2) Barely-trained. Training stopped when three errorless trials occurred in succession.

Over-trained groups:

(3) More-trained. Training continued through the first experimental period in which errors were consistently absent and in which the speed remained practically constant throughout all trials.

(4) Most-trained. Training continued until there had been no errors and no appreciable improvement in speed for at least three days.

Although the criteria for the over-trained groups appear somewhat indefinite it was possible in actual practice to distinguish such stages with a fair amount of definiteness. For the More-trained Group of the *Ss* arrived at a stage where for the first time they could with considerable effort avoid errors and maintain consistent speed throughout a whole period. For the Most-trained Group the task had become absolutely automatic in its performance so that each new trial was for the *S* a matter of routine.

The general method used in learning the code has already been described. At the beginning of the first period 20 trials of copying the symbols were given in order that this period might correspond more closely to the initial period of learning of the second task where the shift would occur after a number of trials of the first task. At the beginning of the actual training the *S* was told, along with the explanation of her task, that she should strive for speed and hence should dispense with the use of the prompting key just as soon as possible. The time for each trial was announced at its completion. An interval of about 10 sec. was required between trials for recording, and a rest period of one minute was given after every 10 trials. A point was made of engaging the *S* in general conversation during these rest periods to prevent thinking over the code and engaging in implicit practice. Forty lists of the letters in different orders were used, and the order in which the lists were presented was changed daily. The Half-trained and Barely-trained Groups completed their training during a single experimental period. The two over-trained groups practiced on the first day until 3 successive errorless trials were achieved; their training was continued on the following days at the rate of 40 trials each period until the desired proficiency was reached.

The number of trials required by individuals to reach the defined proficiency for the group varied tremendously. For the Half-trained Group the range was from 8 to 18 trials, for the Barely-trained from 17 to 39, for the More-trained from 83 to 120, and for the Most-trained from 260 to 460. These figures illustrate clearly the impossibility of defining stage of training in terms of repetitions. The group averages in number of trials required and final time for a trial were:

	Av. Number of Trials	Av. Final Time	Total Improvement
Half-trained	12.4	25.1 sec.	40%
Barely-trained	26.1	15.8 sec.	60%
More-trained	95.6	11.9 sec.	70%
Most-trained	308.0	8.1 sec.	80%

The approximate percentages of total improvement in speed were obtained by comparing the times for the first and the last trials of this task; the times for the first trial were of the order of 40 sec. The figures show that it was necessary to add each time an increased number of repetitions in order to secure progressively decreasing increments of improvement in speed, as would be expected from the shape of the typical learning curve. The four values for the final time of each series indicate that the groups were shifted from this task at stages which were distinctly different as was indicated also by the error criteria incorporated in the definitions of the stages.

LEARNING OF THE SECOND TASK

The shift from the first to the second task occurred always in the middle of an experimental period, and the interval between the last trial of the first task and the first trial of the second was uniformly 3 min. This arrangement and interval were chosen because of some evidence which would indicate that the smaller the time interval between tasks the greater will be the interference. The method used for the second task was exactly the same as that for the original task. The learning extended over three periods. On the first day the shift to the new task occurred after 10–30 repetitions of the first task, and practice was continued until 3 successive errorless trials were achieved. On the second day there were 40 repetitions and on the third day 20.

The *S*s were instructed to use exactly the same method in learning the new code as that used for the first task. Preliminary experiments had shown a wide difference among *S*s as to the manner of approaching a new task. Some *S*s were constantly reminded by the letters of their previous symbols and involuntarily related each symbol to both its new letter and the one formerly represented. Other *S*s were never reminded of previous connections and practically failed to recognize the symbols; they accepted fully the notion that they were starting an entirely new task. The possible effects which these two different attitudes might have upon interference were not determined in our brief preliminary experiments, but the point is certainly one well worth separate investigation. In the attempt to secure as much uniformity as possible the *S*s were told that they were absolutely through with the old code and must now put it completely out of mind. As was shown by the introspective reports this attempt to induce voluntary control was only partially successful even at the beginning. However, it did serve to prevent the mistake on the part of the *S*s of accepting the new task as one involving the relating of two codes.

INTERFERENCE DURING INITIAL LEARNING OF THE SECOND TASK

In the treatment of the results the basic method used is that of comparing the learning to a certain stage of proficiency in the second task with the original learning to the same stage in the first task. Greater difficulty in the learning of the second task is taken to indicate interference-effect, and greater facility with the second task is taken to indicate transfer-effect. Comparisons between the differently trained groups are based upon the relative size and direction of these effects.

For a first comparison a convenient and significant stage is that of bare mastery, the stage at which the first errorless performance occurs. Results obtained for the learning of each task to this stage of proficiency are given in Table I.

These results indicate, in general, interference-effect for the two briefly-trained groups and transfer-effect for the overtrained groups. Differences in difficulty might show up as differences in any one, or in any combination, of three variables: number of trials, number of errors, and bare speed for each trial. The time record was designed to reflect both bare speed and delay for correction of errors, so that the time for a trial as recorded is in part a function of the number of errors. As the results come out, the average time for a trial differs very little between tasks and shows in the second task only very slight differences between groups; for this particular experimental arrangement the differences in difficulty show up primarily in the number of trials required. Since the total time reflects both the number of trials and the time for each trial, it is a more adequate single measure than any of the former. According to the total time required, the Half-trained Group shows very little effect, the Barely-trained Group shows definite interference, the More-trained Group shows definite transfer, and the Most-trained Group shows the greatest amount of transfer.

TABLE I

RESULTS FOR LEARNING IN EACH TASK TO THE POINT OF BARE MASTERY

The signs indicate the direction of the differences; 'plus' indicates transfer-effect, 'minus' interference-effect. For the Half-trained Group training in Task A stopped at 'half-mastery;' the figures supplied in parentheses are averages for the 10 Ss of the Barely-trained and the Most-trained groups. These two groups were selected because of close similarity between their curves and that for the Half-trained Group up to the point where the latter stopped.

		Most-tr. Group	More-tr. Group	Barely-tr. Group	Half-tr. Group
Av. No. of Trials	Task A......	26	29	26	(26)
	Task B.......	18	27	29	24
	% diff........	+30.8	+6.9	−11.5	+7.7
Av. No. of Errors	Task A......	127	143	138	(132)
	Task B.......	101	135	147	133
	% diff........	+20.1	+5.6	−6.5	−0.4
Av. Total Time	Task A......	698	894	672	(685)
	Task B.......	584	823	755	690
	% diff........	+16.3	+7.9	−12.3	−0.7
Av. Time per Trial	Task A......	28.8	30.8	25.8	(27.3)
	Task B.......	30.4	30.5	26.0	28.7
	% diff........	−5.5	+1.0	−0.7	−5.1

The percentages and directions of difference between the results for the two tasks are presented graphically in Fig. 1.[11] Comparison of the effects shown for the different groups leads to the conclusion that in general there is an inverse relationship between amount of interference and amount of previous training. The results for the Half-time Group, however, suggest a modification to the effect that the inverse relationship holds only beyond a certain moderate amount of training. Bair and Wiltbank found this inverse relationship, and Kline suggested the modification to the rule.

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[11] For comparable figures from other researches see Wiltbank, *op. cit.*, 42, and Hunter, *op. cit.*, 40.

The results have been treated up to this point in relation to gross differences spread over a large segment of the learning curves. In the hope of locating and tracing the occurrence of the effects more precisely, complete learning curves for each of the tasks were plotted for all *S*s. It would seem a simple matter to compare the curves and to detect interference at the points where the second curve falls above the first. However, for these curves the most striking differences were in length and not in height so that direct vertical comparison could be of little significance. Moreover,

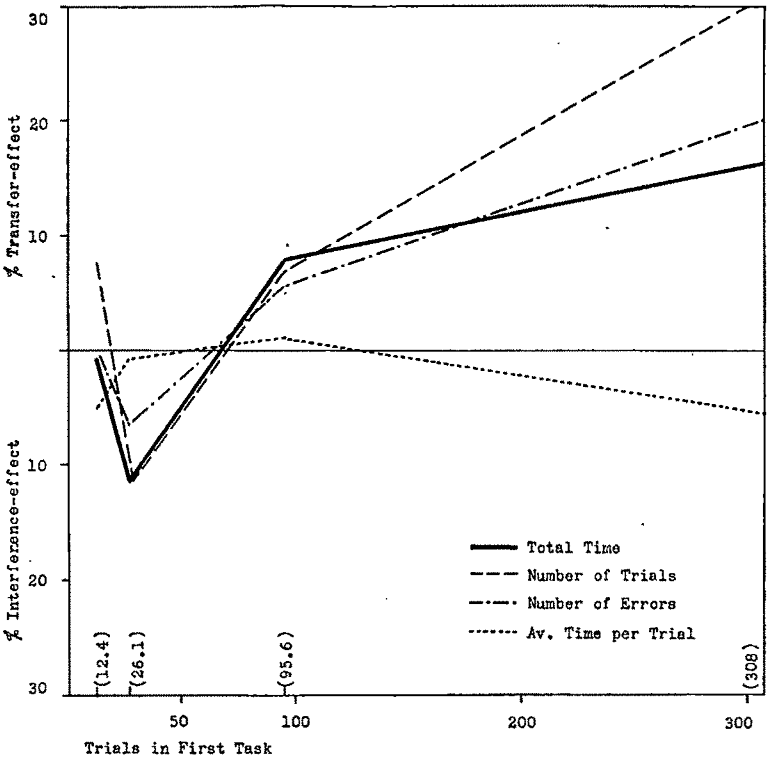

FIG. 1. PERCENTAGES OF TRANSFER- AND INTERFERENCE-EFFECT IN THE BARE MASTERY OF THE SECOND TASK PLOTTED AGAINST THE NUMBER OF PRACTICE TRIALS GIVEN IN THE FIRST TASK

Figures just above the base line indicate average trials for each of the four stages of training provided.

because of the differences in length it was impossible to combine individual curves into curves for group averages. These difficulties were overcome by conversion of the raw curves into Vincent curves.[12] The total number of trials was first divided into tenths and the time required for each tenth was then found. The Vincent curves based upon group average figures are shown in Fig. 2.

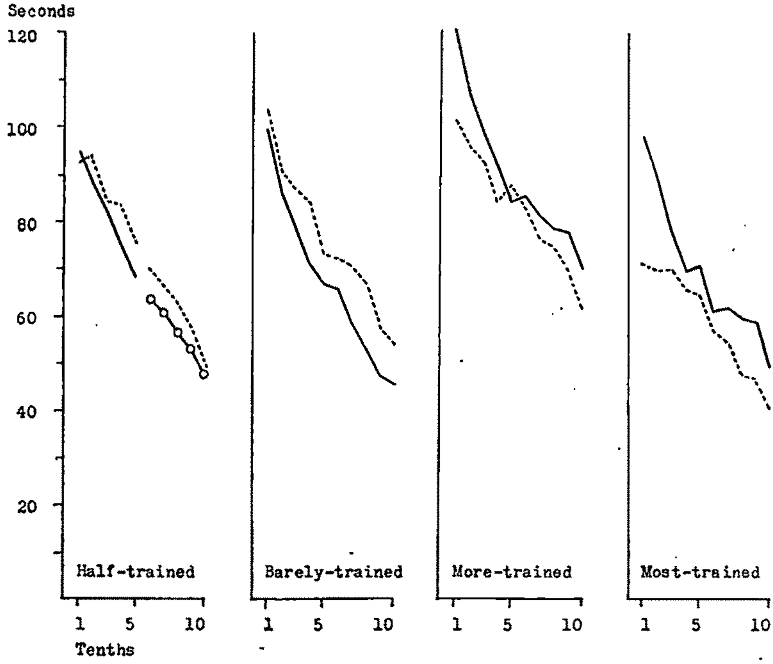

FIG. 2. VINCENT CURVES SHOWING THE AMOUNT OF TIME SPENT IN EACH TENTH OF THE TOTAL NUMBER OF TRIALS REQUIRED FOR BARE MASTERY OF THE TWO TASKS

Solid lines represent the first task and broken lines the second. The continuation of the solid curve for the Half-trained Group is based upon averages for the ten subjects of the Barely-trained and the Most-trained Groups. Figures for the More-trained Group were not included in these averages because of the obvious dissimilarity to the other three groups in level of performance.

The irregularity of the curves of Fig. 2 makes interpretation somewhat difficult. However, some general tendencies are rather clearly shown. For the two over-trained groups the curves indicate general transfer-effect, great at the beginning and end and less in the middle portion. For the

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[12] For discussion of Vincent curves, see S. B. Vincent, *Behav. Monog.*, 1, 1912, (no. 5), 16-17; also, W. S. Hunter, in *Foundations of Experimental Psychology*, 1929, 567-68.

Barely-trained Group interference-effect is shown throughout, increasing in amount from beginning to end. For the Half-trained Group interference-effect is indicated in the middle portion.

The fact that the curves for the second task, with the exception of the curve for the Barely-trained Group, are generally concave to the base is partly explainable by the manner in which learning started. For a number of trials at the beginning it was necessary to look up each symbol so that the task was really a mechanical one. Although the *Ss* were instructed to try to avoid use of the prompting key as soon as possible, the introspective reports show that during the early stages many *Ss* found it more speedy to press the key than to "wait for memory of the symbol to appear." However, this same situation was reported at the beginning of the first task where conditions were exactly the same. In the case of the second task one may assume, with general support from the introspective reports, that the symbols "appeared in memory" more slowly, so that *S* resorted to the use of the prompting key to a greater extent.

It is obviously not safe to draw detailed conclusions from comparison of these curves until their further course has been traced. However, comparison to this point does indicate that the effects vary during the course of training in the second task and that the manner of variation differs between groups. There is definite suggestion that the relative positions of the groups with respect to amount of interference-effect shown may depend upon the point at which the measurements are taken. In line with this suggestion the various measures were applied at the stage of 'half-mastery' (5 correct symbols), and percentages of transfer- or interference-effect were calculated. These percentages of difference between the learning of the two tasks to the point of 'half-mastery' are given in Table II. 'Plus' indicates transfer-effect and 'minus' interference-effect, as in the first table.

TABLE II

PERCENTAGES OF DIFFERENCE BETWEEN THE LEARNING OF THE TWO TASKS TO THE POINT OF HALF-MASTERY

	Most-tr. Group	More-tr. Group	Barely-tr. Group	Half-tr. Group
Av. No. of Trials, % diff.	+8.3	+3.7	+3.2	−8.3
Av. No. of Errors, % diff.	+11.5	+5.2	−1.0	−12.5
Av. Total Time, % diff.	+0.2	+7.8	+0.4	−6.1

The principal difference between the effects indicated here and those shown by measurements to the point of bare mastery (Table I and Fig. 1) lies in the reversal of the positions of the two briefly-trained groups with respect to the greatest amount of interference-effect. Here at the stage of 'half-mastery' the Half-trained Group shows definitely the most interference, and the Barely-trained Group shows little effect in either direction. At this stage the results for these two groups fall into line with the general rule of inverse relationship between interference and previous training, while at the stage of bare mastery they seemed to constitute an exception to the rule. Such a discrepancy between two sets of measurements taken at different early stages points to the importance of tracing the course of changes in the effects through further stages in the learning.

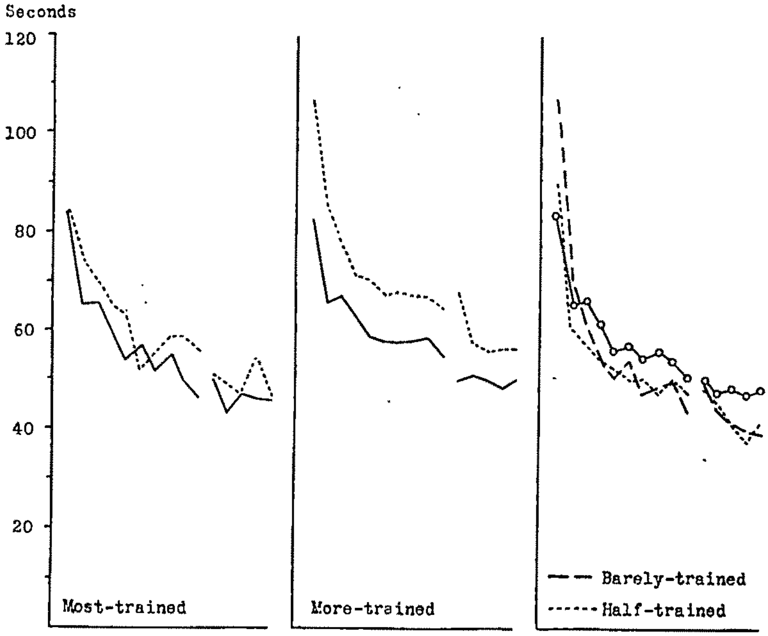

FIG. 3. CURVES SHOWING THE AMOUNT OF TIME SPENT IN EACH FOUR TRIALS DURING TRAINING BEYOND THE STAGE OF BARE MASTERY

Solid curves are for the first task, broken curves for the second. The solid curve in the case of the briefly-trained groups is based upon averages for the ten subjects of the over-trained groups. The gap in all curves indicates the interval between the two days.

INTERFERENCE DURING CONTINUED TRAINING IN THE SECOND TASK

Training in the second task was continued for two days after the stage of bare mastery had been reached. For consistency with the rest of the experiment it would have been desirable to define the length of this additional training in terms of arrival at a certain stage of proficiency. However, with the stage of errorless performance already passed, it was difficult to set a further standard in terms of errors, and, with the great inherent differences in speed, it was impractical to set in advance a standard in such terms. Hence the number of trials was taken as the only possible means for defining the length of the additional periods. Forty trials were given on the first day and 20 on the last day. Figures for comparable periods of training in the first task were available for the two over-trained groups. With the number of trials made constant and with errors practically absent, the total time is here a simple function of the time for each trial. Curves showing the amount of time spent in each 4 trials during the two periods beyond bare mastery are shown in Fig. 3.

These curves for the later stages of training seem to indicate interference-effect for the two over-trained groups and transfer-effect for the two briefly-trained groups. In the case of every group the relation of the second curve to the first is the reverse of that found during the period preceding the stage of bare mastery (Fig. 2). Between interference and previous training a positive relationship is shown at these later stages instead of the inverse relationship shown in the measurements at earlier stages.

Obviously the curves of Fig. 3 are not directly comparable to those of Fig. 2 where differences in height represent chiefly translated differences in the number of trials. In order that the two sets of results may be treated together, the figures for the later stages must be brought into relation to some defined stage of proficiency. If the average speed for each group in the first 5 trials of the first task is taken as the original speed of performance in this activity, percentages of total improvement can be found on this basis. Calculated to the average time for the last 5 trials on the last day these percentages of improvement are:

	First Task	Second Task
Most-trained Group	67.8	67.8
More-trained Group	64.4	64.6
Barely-trained Group	—	71.5
Half-trained Group	—	74.9

While these figures show that for the two over-trained groups the stages of proficiency reached were closely similar, there is the indication that the two briefly-trained groups were carried beyond in improvement. It is now possible to set 60% as a standard percentage of improvement in speed to be effected and to go back to the raw learning curves for the points at which this standard was attained. The figures obtained by this treatment of the results are given in Table III.

These figures indicate again interference for the over-trained groups and transfer for the briefly-trained groups when the measurements apply to learning to a relatively advanced stage. There is a definite tendency for the relations between groups to be roughly the reverse of what they were when measured at or before bare mastery. Although the calculations of percentages of effect for the two briefly-trained groups are based upon mere averages of the other *S*s, direct comparison of the figures for total time in the second task shows that here in Table III the figures for the two briefly-trained groups are disproportionately low in comparison to their position with respect to the other groups in Table I.

TABLE III

Results for Learning to the Point Where the Average Time Per Trial Is 40% of the Average Time for the First 5 Trials of the First Task

Results to this stage in the first task were not available for the two briefly-trained groups figures supplied in parentheses are averages for the 10 *S*s of the other two groups. The 'plus sign before percentages of difference indicates transfer-effect and the 'minus' sign indicates interference-effect.

		Most-tr. Group	More-tr. Group	Barely-tr. Group	Half-tr. Group
Av. No. of Trials	Task A......	42	45	(43)	(43)
	Task B.......	42	67	45	32
	% diff........	0	−48.8	−3.5	+26.4
Av. Total Time	Task A......	974	1174	(1074)	(1074)
	Task B.......	994	1573	1051	842
	% diff........	−2.1	−33.9	+2.2	+21.7

These comparisons are still subject to one limitation: proficiency at the early stages was defined in terms of errors and at the later stages we have sought to define it in terms of speed. This shift in criterion may be justified logically by the fact that at the early stages the principal aim of the *S*s was improvement with regard to errors while at the later stages, with errors already eliminated, the aim was improvement in speed. However, a more objective justification was sought by treating the results of the first day of training in terms of the achievement of a stated percentage of improvement in speed. Percentages of difference between the two tasks in total time were calculated to the point of 20% of improvement in speed over the original average speed for 5 trials of the first task. The two briefly-trained groups showed large differences toward interference-effect: 44.7% for the Barely-trained Group, and 36.6% for the Half-trained Group (calculated only to the point of 12% of improvement). A difference of 3.1% toward transfer is shown in the case of the More-trained Group. The figures for the Most-trained Group (with the exclusion of one *S* who did not improve in speed for either task during the first day, although she quickly eliminated errors) show some interference (about 18%). Comparison of the relationships between groups here with those at the later stage (both stages defined in terms of improvment in speed) lends support to our general conclusion. When measurement is shifted to a later stage, there is a real shift, roughly a reversal, in the arrangement of the groups in order of amounts of interference.

Fig. 4 shows in composite form the results obtained by measuring the effects at three different stages in the learning of the second task.

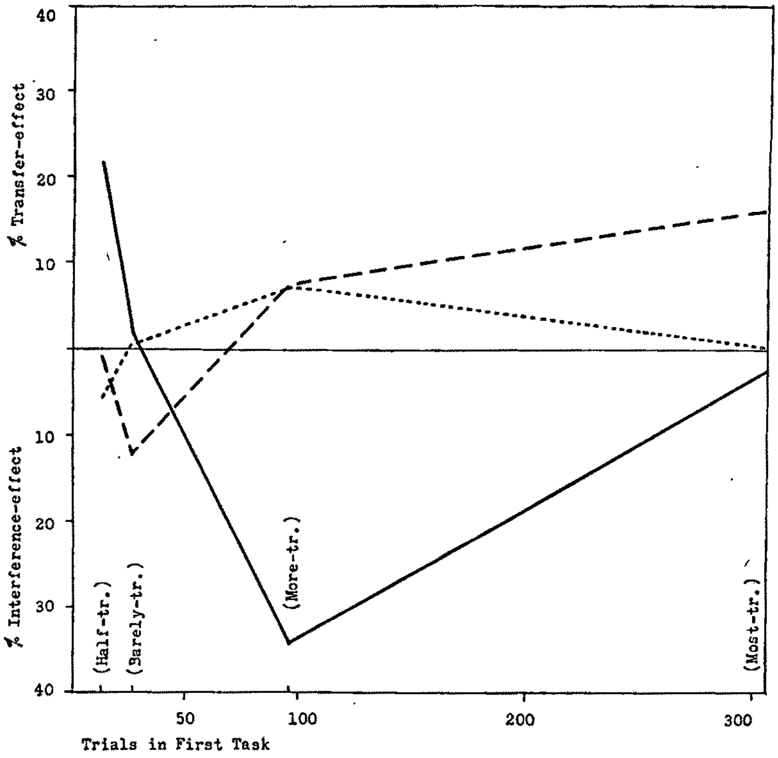

FIG. 4. PERCENTAGES OF TRANSFER- AND INTERFERENCE-EFFECT IN TERMS OF GAIN OR LOSS IN TOTAL TIME REQUIRED, PLOTTED AGAINST THE NUMBER OF PRACTICE TRIALS GIVEN IN THE FIRST TASK

The three curves show the results of measurements taken at three different stages of proficiency in the second task: 'half-mastery' (line of dots), bare mastery (line of dashes), and improvement of 60% in speed (solid line).

DISCUSSION OF RESULTS

The point of outstanding significance in the results is the demonstration of the manner in which the amount of interference, resulting from a given amount of previous training, changes during the successive stages of training in the second task. The time at which the interference will occur, as well as its amount, is determined by the stage of previous training. Hence, the question of which stage of training produces the most interference can be answered adequately only if measurements at several stages are taken into consideration. During early stages of training in the second task the greatest interference-effect is shown by the groups with brief training in the previous task; during later stages the greatest interference-effect is shown by the over-trained groups.

In the light of these findings it is easy to see how different investigations might lead to conflicting results if the measurements were made in each case at only a single stage. An examination of the conditions provided in the previous researches which have been cited suggests that the differences in results are due, at least in part, to differences in the stages selected for measurement. Insofar as may be judged from the number of trials and the nature of the tasks, it seems generally true that the investigators who found an inverse relationship between interference and previous training measured at relatively early stages, while those who found a positive relationship measured at advanced stages. While our findings can not be said to offer a basis for resolving all conflict between previous sets of evidence, they do point out another important way in which these previous results are unsuited for direct intercomparison.

The changes in amount and direction which the effects undergo during training in the second task seem to follow in each case a definite rule. The order in which the different groups arrive at their respective points of maximal interference-effect is precisely parallel to the order in which they shifted from training in the first task. The first three groups show maximal interference-effects at stages which correspond directly to the stages reached in the first task. This fact is brought out strikingly in Fig. 4 where the lowest point on each curve is that for the group which was trained in the first task to the same stage at which interference was measured in the second task.

The curves of Fig. 4, and our measurements in general, do not truly represent the extent of the effects at a particular stage; they represent the cumulative effects throughout the whole learning process up to a given stage. When, for example, a small degree of transfer-effect is shown for the Barely-trained Group at the stage of 60% improvement, this really means that just before this point of measurement there was a great amount of transfer-effect sufficient to cancel out the previous balance toward interference-effect and still leave a net gain for the whole period. At the stage of the last measurement the Most-trained Group was experiencing an increasing amount of interference which had already had sufficient effect to cancel out the large transfer-effect shown up to the stage of bare mastery. It seems natural to assume that this group would have reached its maximum of interference-effect at a later stage had the training been continued to a stage corresponding to that reached in the first task. This assumption gains in plausibility from the fact that the maxima for the first three groups show, in the order in which they occur, a progressive increase in size, while up to the stage of the last measurement the Most-trained Group has shown no interference-effect as great as even the smallest of those for the other three groups.

The evidence from the introspective reports is in complete accord with the formulation that interference occurs maximally when training in the second task is at the stage which was reached in the first task. All *Ss* described interference as definitely present at some time, and when the effect was at its height many actually ascribed their difficulties to the similarity between their present stage of performance and that at which they had left off in the previous task.

The manner in which the interference was experienced by the *Ss* is of interest here. No questions were asked by the *E*, and the reports were purely spontaneous on the part of the *Ss* except for an occasional request that some point mentioned be elaborated. Almost without exception the *Ss* reported that, when difficulty was experienced, it could be of either one of two distinct types. The first type, usually considered less 'serious' than the other, was described as the occurrence of a complete blank space in consciousness accompanied by inability to send any symbol. This type of blocking occurred quite suddenly—several letters of a list would be sent smoothly and the next letter would then produce a "mental vacuum." Finally the correct symbol would "appear" or the prompting key would be remembered. No suggestion of the first code entered consciousness, nor was there any tendency toward reversion either at the time of the blocking or later. As soon as the blocked condition was displaced, the *S* could go on sending the correct symbols as smoothly as ever. Although this type of difficulty involved an immediate loss of time, it was not regarded as at all serious, since it did not affect the ease or the speed of sending the rest of the symbols.

The second type of difficulty was described as direct intrusion by the symbols of the first code. Just as she started to send a symbol, the *S* would suddenly stop and say, "No, that is the old code." Once this had happened it was impossible for the *S* to keep the old symbols from coming to mind for as many as several trials or for even a whole period. The *Ss* regarded such periods, with considerable annoyance, as periods of truly serious difficulty. During such periods the *Ss* complained of the fact that, when one is equally familiar with two different sets of symbols, it is impossible to keep the two separate. Those who experienced such difficulty only at a late stage remarked that the separation had been easy before because the old task had become a routine matter of sending symbols automatically at high speed and so was completely dissimilar to the slow, effortful performance at the early stages of the second task.

It would seem, then, that the effective type of interference, the type which shows up plainly in the objective measures, involves a true reversion to the 'set' or attitude of the former task. Such a reversion is likely to occur only when the two tasks are at such stages that the general modes of approach, in this case the 'motor set' for a certain speed and the general direction or level of attention, are the same for both performances. When the two tasks are at dissimilar stages, an overt reversion may occur and be corrected without being recognized and will cause no further disturbance. When the two tasks are at similar stages, a reversion will at once bring up the whole previous set and will introduce a period of great difficulty. It is even possible, as is shown in several instances, for an overt reversion to occur and to be recognized without further consequences if the two tasks are at the time regarded as dissimilar. In such a case the *S* dismisses the matter with a remark such as, "My hand got ahead of me," and is able to settle back at once to the present task at its own slow pace.[13]

In view of the theoretical background of the problem the question arises as to whether the difficulty experienced during interference was primarily a matter of difficulty in association or in recall. While the separation of these two processes seems a somewhat artificial one, the introspective reports do indicate that the *S*s localized the difficulty in the process of recall (or reproduction). If interference is to be regarded as a conflict between attitudes or tendencies, the tendencies must be reproductive rather than associative.

A typical report of one *S* will serve to illustrate this point. At the time of great interference-effect, she said: "I try to think of the symbol or to send it, but it just will not come; the wrong one keeps getting in the way. Then I look at the prompting code, and there is the symbol. It belongs to the letter so much that I feel foolish at forgetting it, but a few minutes later I can't make it come when I need it."

The answer, then, to our original question of just which stage of training results in the most interference must begin with the provision that it depends upon the stage at which interference is measured. If measurements are taken at various stages and if we compare the maximal effects for the different groups regardless of the stage of occurrence, our results point to a positive correspondence between amounts of interference and amounts of previous training. Within the range of our measurements there is a progressive increase in the amount of interference shown for three groups in order of increased training. Unfortunately our measurements did not extend to the stage at which maximal effect for the Most-trained Group could be expected, but there is reasonable ground for assuming, both from the objective and the introspective evidence, that the effect for this group would have followed in the progression.

Despite the satisfyingness of such a generalization, critical consideration shows how little it can really mean. Although the amounts can be compared, the essential nature and the practical effect of the interference is different for each case. In the case of the briefly-trained groups the interference consists in difficulty at the initial, slow stages of performance; the difficulty is soon overcome and further learning progresses smoothly. With the over-trained groups there is no difficulty in performing the new task and in learning it to a certain stage; the difficulty here is a matter of speed, an inability to progress beyond a certain relatively advanced level. For a problem in which learning of a second task was required only to the point of bare familiarity, the results of previous overlearning in a conflicting task could have little or no significance, although the magnitude of the effect of such over-learning is in terms of the generalization very great.

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[13] Under the present experimental conditions, overt reversion to the extent of the actual sending of the former symbol occurred very infrequently. For all *S*s during the entire experiment only 92 instances were recorded. The number of such reversions has no apparent relation to the amount of interference shown according to other criteria. The fact that the significance of reversion may differ in different instances suggests possible inadequacy in an experimental technique which employs only motor reversion as the direct measure of interference.

In introducing the problem and its history we found it necessary to refer to two opposed theoretical views, the view of the *Gestalt* psychologists and the view which is called by them associationistic. In general our results are much more amenable to treatment in the terminology of *Gestalt* than in terms of associationistic connections. However, under the conditions of this experiment we did find that the stage of habit-formation, the 'mere strength' of the organization, can be a determinant of interference both as to its degree of effectiveness and as to the time of its occurrence. The conditions of the experiment were not sufficiently different from those under which motor learning goes on in everyday life to lead to agreement with the general proposal of the *Gestalt* psychologists to the effect that ordinarily mere strength of a habit is a negligible factor in influencing effective reproduction.[14]

SUMMARY

(1) Interference-effects appear at different stages in the learning of a second, inconsistent task, dependent upon the stage of training reached in the first task. For a given stage of original training the maximal effect will appear when the learning of the second task reaches a stage approximately the same as that finally reached during training in the first task.

(2) If interference-effect is measured during the initial stages of training, an inverse relationship between the amount of interference and the stage of previous training is found. If measured at more advanced stages of training, the amounts of interference bear a direct, positive relationship to the stages of previous training. If the maximal amounts of effect are compared without regard for their different points of occurrence, a positive relationship to the amounts of previous training is indicated.

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[14] Since the acceptance of this article for publication, J. A. McGeoch has published results of studies on the influence of degree of interpolated learning upon retroactive inhibition (this JOURNAL, 44, 1932, 695-708). An analysis of McGeoch's results reveals a tendency for the maximal retroactive effect to occur when the interpolated task has been learned to about the same degree as the original task. This suggests the possibility that the general law indicated by the present study of interference may apply also to retroactive inhibition. Experiments designed to test this possibility are now in progress.

AN IMPORTANT FACTOR IN SPACE PERCEPTION IN THE PERIPHERAL FIELD OF VISION

By C. E. FERREE, G. RAND, and C. HARDY, Johns Hopkins University

Two important reasons may be given for the poor space perception in the peripheral field of vision; defective imagery and a poorly developed sensorium. With reference to the relative importance of these reasons or factors, the concensus of opinion seems to incline to the latter. In a recent paper,[1] the belief is expressed that the refractive condition in the peripheral field can not play an important rôle because refractive errors in central vision do not seem to have very much effect on the space perception in the peripheral field. In this opinion the fact seems to be overlooked that as compared with central errors of refraction, the errors in the peripheral field are so great that not much effect could be expected. They modify the type of error found in the peripheral field, it is true, but clearness of imagery depends upon the amount rather than the type of defect.

It has been the purpose of the present experiments to study the refractive conditions in the peripheral field. One of the reasons for making this study is the bearing of these conditions on the explanation of the peculiarities and anomalies of peripheral space perception. Other important incentives have been their relation to acuity in the peripheral field; to achromatic and chromatic sensitivity in peripheral vision and to the limits of the form and color fields; their possible relation to defects of imagery in the central field and to ocular deviations; and the information which they give as to the position of the lens and the symmetry of conformation of the retina. In a later paper results will be given which bear still more directly on the importance of the refractive conditions in the peripheral field as a factor in peripheral space perception.

With reference to the above needs or any needs of practical application, the attempts that have been previously made to refract the eye for the peripheral field are unsatisfactory for the following reasons: (a) the methods used were either unsatisfactory as to accuracy or so cumbersome as to ap-

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* Accepted for publication November 29, 1931. From the Research Laboratory of Physiological Optics, Wilmer Ophthalmological Institute, Johns Hopkins Medical School.

[1] M. J. Zigler, B. Cook, D. Miller, and L. Wemple, The perception of form in peripheral vision, this JOURNAL, 42, 1930, 246-259.

paratus and procedure as to be worthless for practical work; (b) the results were not expressed in a form that has practical meaning or value; and (c) the determinations extended but a short distance into the peripheral field. A very important part of our purpose in making the study reported in this paper has been, therefore, the development of a method that will permit of determinations in the more remote parts of the field, that will be reasonably satisfactory as to accuracy and feasibility, and that will give results directly in terms of diopter values.

The two most important factors in the ametropia for the peripheral field are (1) the effect of the oblique incidence of the rays of light from objects in the peripheral field on the clearness of the image formed; and (2) the effect of size and shape of the eyeball and of anomalies and irregularities in the conformation of the retina on the distance of the percipient elements from the nodal point of the refracting system. With reference to the first of these factors it will be remembered that clear images are formed by lenses only when the object to be imaged is located on the principal axis of the lens. When the object is displaced from the principal axis a distortion of the image occurs which varies in amount with the angle of displacement of the object or with the angle of incidence of the light on the lens. In general, the effect of varying the angle of incidence is that of adding a weak plus sphere and a strong plus cylinder with its axis at right angles to the plane of incidence. The major effect is thus to create a strong astigmatism. A simple formula for this effect in the two meridians may be expressed as follows:[2]

$$F_1 = F(\mu - 1)/(\mu \cos b - \cos a)$$
$$F_2 = F_1 \cos^2 a$$

In these formulas F represents the focal length of the lens in question; F_1 the focal length in the meridian of least refractive power; F_2, the focal length in the meridian of greatest refractive power; a, the angle of incidence of the light; b, the angle of refraction at the first surface; and μ, the refractive index of the lens. In both meridians, then, the image is brought nearer to the lens as the angle of incidence increases, very much nearer in the meridian of the oblique incidence and very little nearer in the meridian at right angles to it. The refractive effect for the eye would thus be a slight myopia compounded with a strong myopic astigmatism. The effect on vision is modified, however, by the fact that the distance of the retina from the nodal point of the refracting system decreases rapidly as the distance from the fovea is increased. In the greater number of eyes the effect of this is to bring the retina between the foci in the two meridians or to cause a mixed astigmatism which increases in diopter value as the periphery of the retina is approached. As will be shown later in the paper, however, other types of eye are also found, but they do not seem to be of such frequent occurrence.

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[2] L. Laurance, *General and Practical Optics,* 1908, 163-166; 3rd ed., 1920, 245-247.

METHOD AND APPARATUS

For the refraction of the eye for the peripheral field a Zeiss parallex refractionometer modified to suit our purpose was used. Like all the objective methods of refraction, the method used with this instrument provides a means of locating the point or plane which is the anterior of the pair of conjugate foci of the refracting system of the eye. Of this pair the fundus is the posterior focus. Also like the other objective methods—refraction with the ophthalmoscope and with the retinoscope—the method is only approximately correct in principle, *i.e.* the fundus and not the percipient elements is taken as the posterior focus. The reflecting surfaces which make up the fundus cannot be assumed to have the same location as the percipient elements nor even to sustain an invariable relation to them in the different parts of the eye ground and in different eyes. The distance between these reflecting surfaces and the percipient elements is minute, it is true, but in a refracting system as powerful as that of the eye even minute changes of distance in the posterior focus may become of consequence in the location of the anterior focus. The subjective methods of refraction alone start with the true posterior focus of the refracting system. This is one of the reasons why they should be the final court of appeal in making an accurate refraction.

The subjective method, however, we find, can be used with but indifferent success in the peripheral field. Of the objective methods, that used with the parallex refractionometer has thus far been found by us to be the most satisfactory for this work.

In brief, the principle of locating the anterior of the conjugate foci of the refracting system of the eye utilized in the Zeiss refractionometer is as follows. At the principal focus of the objective lens of the instrument is placed a transilluminated test object. The rays of light from this test object are collimated by the lens and enter the pupil of the eye as a parallel beam. If the eye is emmetropic the image will be focused on the fundus (approximately). Likewise when the eye is emmetropic the light reflected from the fundus will emerge from the pupil of the eye as a parallel beam and will be focused by the objective lens of the instrument at the position of the test object. In order to be able to compare the distances of the test object and of the image so formed from the objective lens of the instrument, the principle of parallax is used; that is, the line of view of the examiner is displaced slightly to one side of the principal axis of the refracting system of the eye and objective lens. If the distances of object and image from the objective lens are equal, the image will superimpose on the test object and will not be visible (emmetropia). If it is nearer to the lens than the test object, the image will be seen displaced to one side of the test object, the side towards the illuminating tube of the instrument (myopia); if it is further from the lens than the test object, it will be seen displaced to the other side, the side opposite the illuminating tube of the instrument (hyperopia). In order to provide for the refraction of the eye in different planes in the examination for astigmatism, the system can be rotated through 180° and the determinations made at any position within this range. In case the position of the test object and of its image are found not to coincide, the test object is moved until exact coincidence is obtained. The vertex refraction can then be read from a scale which is seen just above the test object. For a more detailed description of the instrument, the reader is referred to the circular issued by the manufacturer.

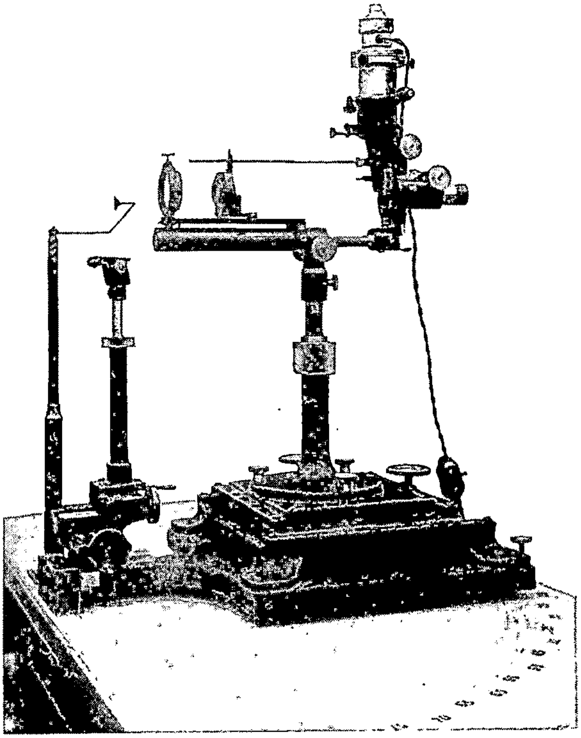

FIG. 1. MODIFIED ZEISS PARALLAX REFRACTIONOMETER

As the instrument is placed on the market, it is designed only for refraction at the center of the field. As modified by us, it can be used at any point in the horizontal meridian from the center of the field to an eccentricity of 60° temporally or nasally. Beyond this point the image reflected from the fundus is too dim to be distinguished. The instrument as modified for our purpose is shown in Fig. 1.

As seen in Fig. 1, the instrument is mounted on a carriage provided with ball and socket casters and made to pivot about a fixed point. The circle described by the carriage is graduated in degrees. The position of the carriage is read by means of a pointer which is located at the back of the carriage in the vertical plane passing through the principal axis of the optical system of the instrument. This pointer passes over the graduated scale. In order that the eye may be located with precision vertically above the fixed point and thus be at the center of rotation of the instrument, a small disk is mounted at the point of intersection of the perpendicular erected at the fixed point with the principal axis of the optical system. When the eye is in position the front of the closed lid is in contact with this disk. To maintain the eye in this position, the observer is required to bite a mouthboard in which the impression of his teeth has previously been made and hardened in wax. The disk is then turned out of the way.

In order to facilitate the location of the eye in the desired position, the mouthboard is mounted on a rest provided with up and down, back and forth, and right and left adjustments. The adjustments are made by coarse-threaded screws. The screws for the right and left and the back and forth adjustments are driven by small wheels; the screw for the vertical adjustment is operated by a large milled head.

Method of controlling fixation and accommodation. It is supposed to be possible to use the instrument both with and without a cycloplegic. Both of these conditions have been used in this work. The directions given in the descriptive circular referred to above for the refraction of the central field without a cycloplegic are as follows: The patient is directed to look into the instrument with the eye under examination and to fixate the bright clear spot seen in the center of the field. This is for the control of fixation. For the control of accommodation the test target, which becomes clearly visible when it is located at the patient's far point, is always moved from a position beyond the far point towards the far point. The intention of this is to cause the patient to relax his accommodation in the interest of clear seeing.

We have not found this method to be entirely satisfactory. More reproducible results were obtained and results that check better with the subjective methods of refraction, by enlisting the aid of the eye not under examination; that is, by the use of both eyes, better control could be exercised over both fixation and accommodation. The procedure is as follows. When central refraction is to be determined, the eye not under examination is required to look at the smallest detail it can discriminate on a test chart at a distance of 6 meters. If it is hyperopic or emmetropic, it is fogged to 0.50 diopter of myopia to induce relaxation of accommodation. If it is myopic, it is already fogged for distance. In that case a larger detail to be discriminated has to be selected on the test chart. To guarantee that the eye not under examination is taking the same fixation as the eye under examination when it is looking at the bright dot at the center of the test field, as is required in the use of the instrument, the test chart is so located that the bright spot is seen at the center of the detail which serves for the fixation of the eye not under examination. This satisfies the two essential conditions; namely, that the two eyes are fixating in proper coördination and that the eye under examination is fixating the bright dot at the center of the test field. In refracting for the peripheral field the eye under examination can not, of course, be caused to look into the instrument. In this case both eyes fixate the distant chart whenever possible. This was possible only from 25° to 60°. That is, for the range 0° to 25°, when the temporal field was being refracted, the instrument was interposed between the test chart and the eye under examination. The fixation then had to be maintained by the other eye alone. For the range 0° to 25°, when the nasal field was being refracted, the instrument was interposed between both eyes and the test chart. In this case the effect of distance was given to the eye not under examination by mounting close in front of it a small test letter placed at, nearer or beyond the principal focus of a double convex lens, thus causing the rays of light from the lens to enter the eye parallel, diverging or converging as desired. Coördination of fixation for the zero position of the instrument was secured by locating the object and the bright spot in the instrument in such relation that the test objects presented to the two eyes were seen as combined in the median plane. When a cycloplegic was used, the procedure was the same, with the exception that the fixating eye was not fogged.

With the eyes in position and the fixation held as described above, the refraction of the eye was determined at the center and in the nasal and temporal quadrants of the horizontal meridian at intervals of 5° and 10° out to 60°. At each angle of incidence tested, the refraction for both the horizontal and the vertical planes was recorded, *i.e.* the refraction in the plane of the incidence of the light and in the plane at right angles to it. The error of the determination was approximately 0.25 diopter in the central field and the mid-periphery, and approximately 0.37–0.50 diopter in the far periphery where the reflected images, owing to the high degrees of astigmatism present, were indistinct.

RESULTS

A consideration of the effect of obliqueness of incidence on the refracting powers of a lens would lead one to expect a condition of myopia in the plane corresponding to the plane of incidence in case of an emmetropic eye with its refracting system symmetrically disposed in relation to the line to sight; that is, the effect of the oblique incidence in this plane is that of a strong plus cylinder. What the condition would be in the plane at right angles to the plane of incidence would depend upon two factors: (a) the effect of the obliqueness of incidence upon the focus in this plane, which, as already stated, is equivalent to that of the addition of a weak plus sphere; and (b) the shape of the eyeball. In proportion as the eye is spherical in shape, the retina from center to periphery is brought closer to the nodal point of the refracting system. In an emmetropic eye, therefore, with a symmetrical refracting system, one would expect a condition of hyperopia in this plane beginning at a point comparatively near the center of the field. At some point in the periphery in this type of eye a condition of mixed astigmatism might be expected. A mixed astigmatism might be expected also in case of small refractive errors at the center, hyperopic or myopic. In case of a sufficient amount of myopia at the center, a condition of compound myopia might be found in the periphery and similarly a condition of compound hyperopia in the periphery where a strong hyperopia is present in the central field. Obviously it is also possible that the shape of the curve of the eyeball from the posterior pole towards the equator may be such as to cause either a compound myopia or a compound hyperopia even when there is only a slight error of refraction at the center. In this connection it should be remembered that in case of such a strong refractive system as is found in the eye, a very small difference in distance of the retina from the nodal point is sufficient to cause several diopters difference of myopia or hyperopia as the case may be.

Twenty-one eyes were examined in all, 15 without and 6 with a cycloplegic. The amount of refractive error at the center varied among these eyes from 0.25 diopter of astigmatism to several diopters of defect. In general, the effect of oblique incidence was an astigmatism which was variously modified in the eyes examined by the distances of the percipient elements from the nodal point of the refractive system. Classified as to peripheral refraction, 3 types of eye were found. In one type, A, the eye becomes more myopic in the horizontal plane as the periphery of the retina is approached, and more hyperopic in the vertical plane. That is, the curve representing the refractive errors for the horizontal plane tends downward as the periphery is approached and the curve for the vertical plane tends upward. In all the cases but one in which there was a high myopic astigmatism (central refraction), this resulted in a mixed astigmatism in the peripheral field, the interval of Sturm varying over a considerable range of diopters. In a second type, B, the eye becomes less myopic in the horizontal plane as the periphery of the field is approached and more hyperopic in the vertical plane; that is, the trend of the curve for both planes is upward. In the cases studied this resulted in a condition of compound hyperopia in the peripheral field for eyes that were hyperopic, emmetropic or moderately myopic in the central field. If strong myopia is present at the center it is obviously possible that a compound myopia might result over a greater part of the peripheral field. In a third type, C, the condition was asymmetrical, *i.e.* a considerable difference was found in the nasal and temporal halves of the field.

The results are shown in Figs. 2–6. In the charts shown in these figures the results for the horizontal plane, the plane of incidence of the light, are plotted as a solid line; for the vertical plane, the plane at right angles to the plane of incidence of the light, as a broken line. Degrees of eccentricity are plotted along the horizontal coördinate and diopters of refractive defect along the vertical coördinate. The condition of no refractive defect is represented by a horizontal line drawn through the center of the chart. Diopters of hyperopia are plotted above this line along the vertical coördinate; diopters of myopia below it. The diopters of hyperopia and myopia are expressed in terms of the correction needed, + and — respectively. The

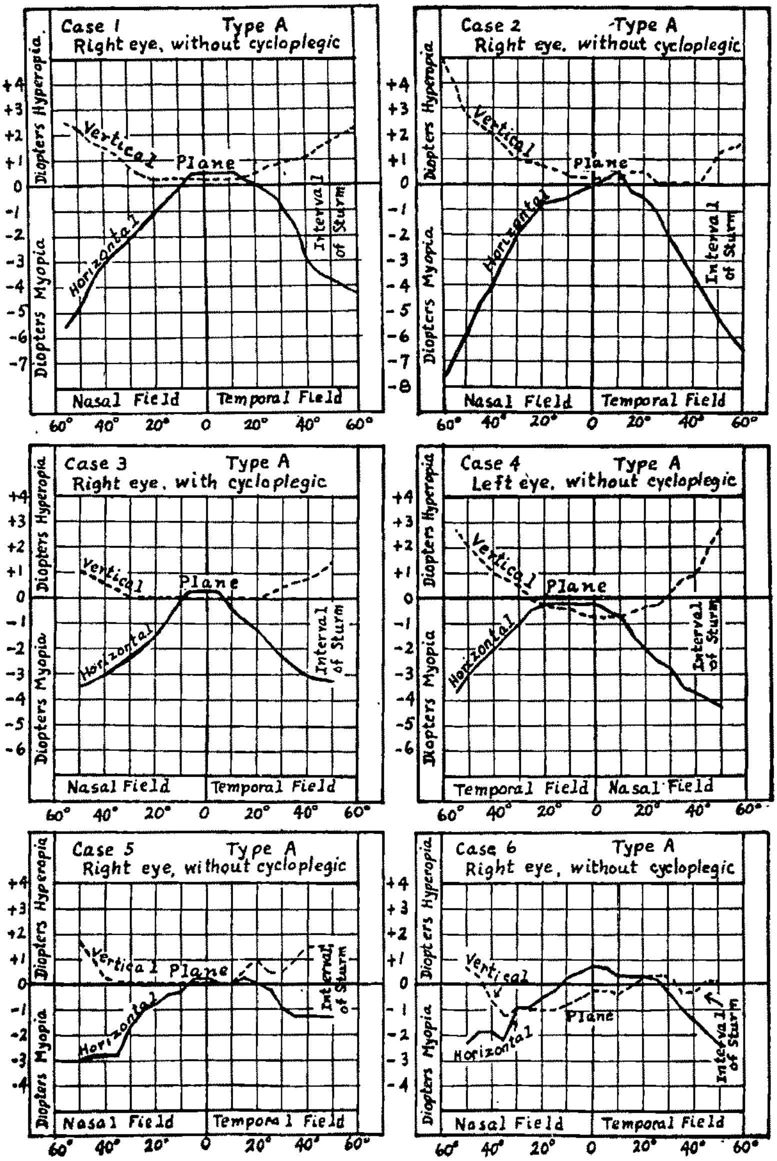

FIG. 2. REFRACTION OF PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE A THAT HAVE ONLY A SLIGHT ERROR OF REFRACTION AT THE CENTER

value of the interval of Sturm can be read along the vertical coördinate at corresponding points between the solid and broken lines.

From the statement of principles already given, it is obvious that the shape of the curve drawn as a broken line is determined dominantly by the shape of the eyeball or by the distance of the percipient elements from the nodal point of the refractive system. That is, this curve represents the refraction in the vertical plane at the various points examined and the refraction in this plane is not strongly affected by the angle at which the examination is made. The shape of the curve drawn as a solid line, however, is strongly affected by the angle at which the examination is made. This curve represents the refraction in the horizontal plane, the plane of oblique incidence; and the effect is, as already stated, to bring the focus progressively nearer to the nodal point of the refracting system as the angle at which the examination is made is increased. The former curve thus informs us, roughly at least, as to the symmetry of conformation of the retina; and the latter, together with its relation to the former, gives us information as to the symmetry of action of the refracting system.

In Fig. 2 are shown results for 6 eyes of the class we have called Type A. These eyes all have only a slight refractive error at the center. It will be noted that in general the defect in the vertical plane, the amount of which is shown by the displacement of the broken line from the line of reference or zero line, is much smaller than the defect in the horizontal plane, shown by the displacement of the solid line. That is, the defect due to the obliqueness of incidence of the light on the refractive system is greater than the defect due to the progressive approach of the retina to the nodal point as the distance of the point under examination from the center of the field becomes greater. In short, in most cases much the greater part of the interval of Sturm is due to the astigmatism of oblique incidence. Some asymmetry will be noticed also in Case 6 but not enough to warrant placing it in the class which we have called Type C.

In Fig. 3 are shown the results for 6 eyes also of the class we have called Type A. These eyes have, however, a much more pronounced error of refraction at the center. The following points may be noted in connection with the results for this chart: (a) the curves are less regular; (b) the temporal and nasal halves of the curves are less symmetrical—Case 8, for example, shows so much asymmetry as to deserve, perhaps, to be classed as Type C; and (c) the point at which the mixed astigmatism begins is farther from the center of the field than is the case in Fig. 2—that is, a smaller proportion of the field carries a mixed astigmatism and a greater proportion a compound myopic or a compound hyperopic astigmatism. In Case 10, a

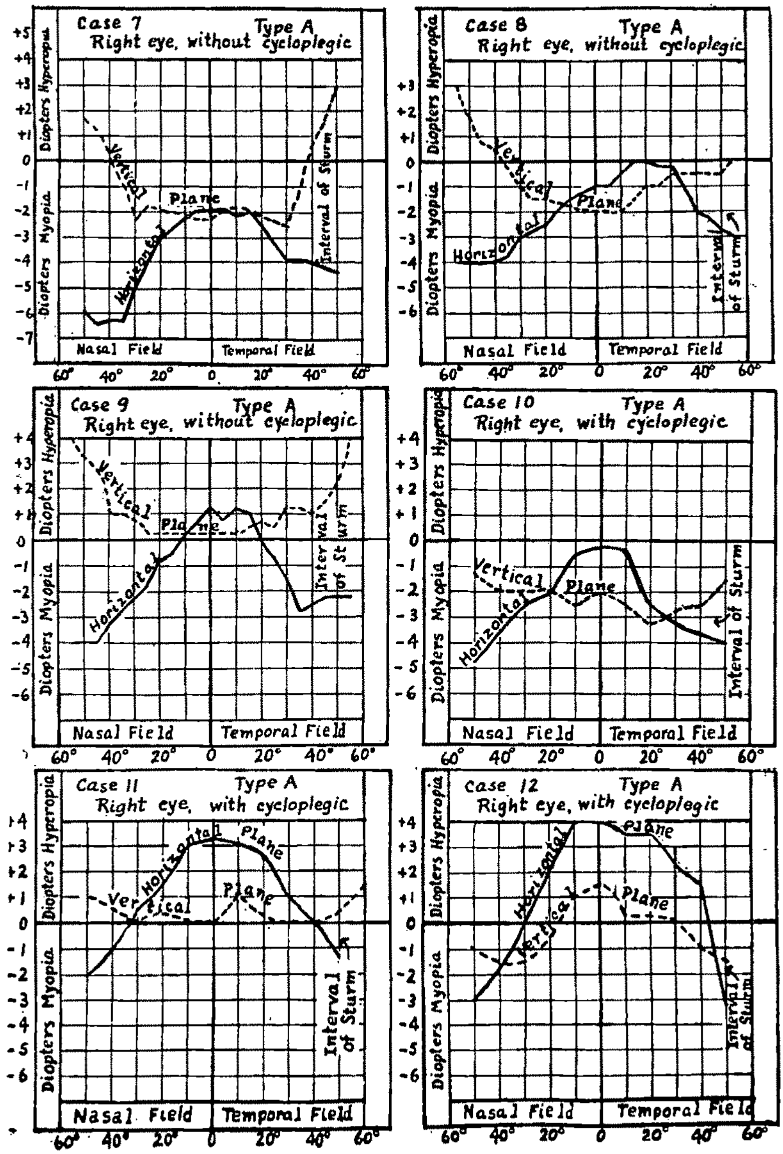

FIG. 3. REFRACTION OF PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE A THAT HAVE A PRONOUNCED ERROR OF REFRACTION AT THE CENTER

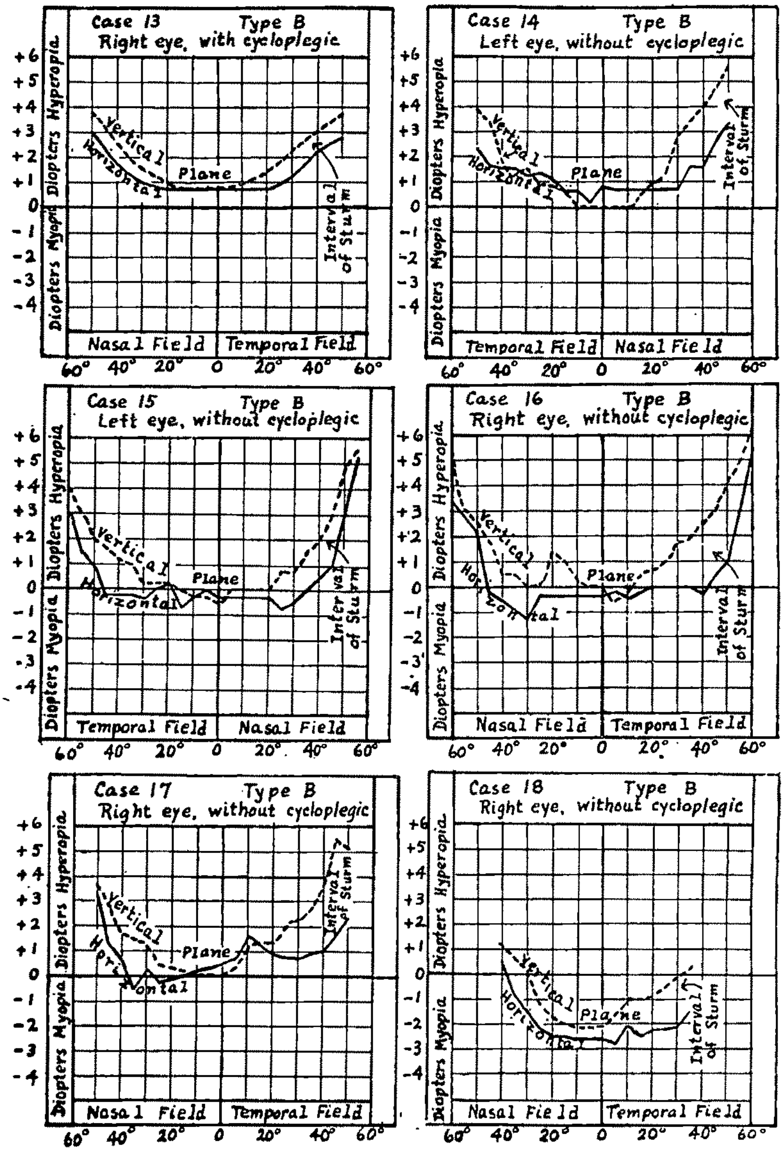

FIG. 4. REFRACTION OF PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE B
Five have only a slight error of refraction at the center, one has a pronounced error.

compound myopic astigmatism with the greater myopia in the vertical meridian, the curves for the two meridians cross at approximately 20° to 25° from the center of the field; *i.e.* beyond these points the eye becomes more myopic in the horizontal plane and less myopic in the vertical plane, but it retains a compound myopia as far out as the examination was made. The astigmatism changed, however, from compound myopia with the rule to compound myopia against the rule.

In Fig. 4 are shown the results for 6 eyes which belong to the class we have called Type B. The characteristic defects in the peripheral field for these eyes are compound hyperopia and compound myopia. Four of the eyes have a slight refractive error at the center of the field and two a more pronounced error. As is shown in the chart, a striking feature of these cases is the smallness of the interval of Sturm in the peripheral field. In Type A the general trend of the curve for the vertical plane from center towards periphery is upward and of the curve for the horizontal plane downward. This gives a progressively increasing value to the interval of Sturm from center towards the periphery. In the cases of Type B, however, the trend of both curves from center towards the periphery is upward, the trend for the curve for the horizontal plane being less sharply upward. There is thus an interval of Sturm, but it is of smaller value than for the cases of Type A. It would appear that the astigmatism of incidence does not play proportionately so strong a rôle in the eyes of Type B as it does in the eyes of Type A.

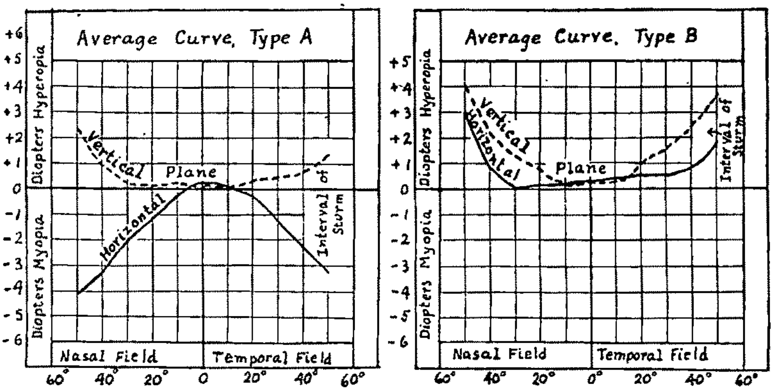

FIG. 5. COMPARISON OF THE RESULTS OF THE EYES OF TYPES A AND B
The chart at the left shows the average refraction of the eye for the peripheral field for 6 eyes of Type A having only a slight error of refraction at the center; that at the right, the average refraction for the peripheral field for 5 eyes of Type B having only a slight error of refraction at the center.

In the left half of Fig. 5 are given curves showing the average of the results in the nasal and temporal halves of the field for 6 cases of the class we have called Type A. We have selected for this average only the cases which have a slight defect in refraction in the central field. This chart shows a slight asymmetry; *i.e.* in the nasal half of the field there is a greater amount of myopia in the horizontal plane and a greater amount of hyperopia in the vertical plane than there is in the temporal half of the field. In the right half of Fig. 5 are given average curves for the nasal and temporal halves of the fields for 5 of the cases we have called Type B. We have selected for the average curve here, also, the cases which have only a slight defect of refraction in the central field. The average curves for the two halves of the field show less asymmetry, perhaps, than the curves in the left half of Fig. 5. Here too a greater refractive defect is shown in the nasal than in the temporal half of the field.

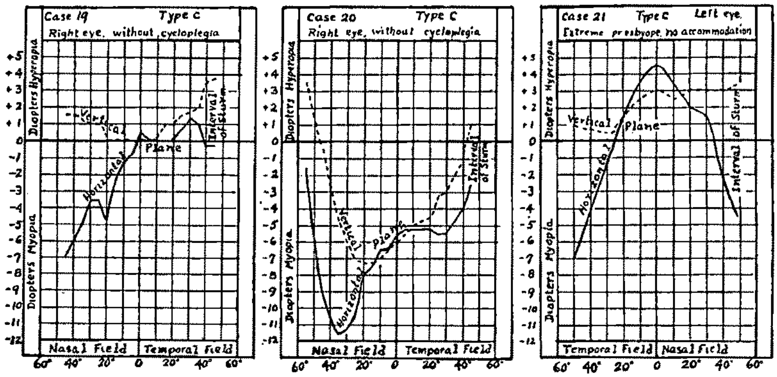

FIG. 6. REFRACTION OF THE PERIPHERAL FIELDS OF VISION FOR EYES OF TYPE C
These three eyes show marked asymmetry in the refraction of the nasal and temporal halves of the field.

In Fig. 6 are shown the results for 3 eyes of the class we have called Type C. These cases, which show a pronounced difference in the two halves of the field, suggest (a) that the lens may not be symmetrically positioned with reference to the anterior-posterior plane of the eye; (b) that there may be asymmetry in the shape of the nasal and temporal halves of the eyeball in the meridian examined or a difference in the distance of the percipient elements from the nodal point of the refracting system in the two halves of this meridian from any cause whatsoever; or (c) that there is a combination of these two conditions. In the three cases shown in Fig. 6 a pronounced asymmetry was found in the refractive condition of the temporal and nasal halves of the field and in one of these, Case 21, the asymmetry may sustain a causal relation to the ocular deviation which was found to be present. In the other two cases the character of the asymmetry is not such as to lead one to suspect any considerable effect on the muscle balance of the two eyes.

An inspection of Case 19 of this chart shows little asymmetry for refraction in the vertical plane; that is, the two halves of the curve for this plane (the broken line) show little difference in shape. For the refraction in the horizontal plane there is, however, a great deal of asymmetry. Largely because of the asymmetry in refraction in the horizontal plane there is also a pronounced asymmetry in the interval of Sturm in the two halves of the field. Under test this case showed practically no muscle imbalance.

An inspection of the chart shows no reason for expecting a muscle imbalance, so far as the refractive situation, central and peripheral, is concerned. That is, the eye is nearly emmetropic at the center of the field and no deviation in either direction could be expected to improve the refractive condition. The curves for this eye would seem to indicate a fairly symmetrical eyeball in the meridian under examination but an asymmetrical refracting system. The general shape of the curve for the refraction in the horizontal plane is such as might be expected for a lens rotated or tilted towards the temple. That is, the focus for objects in the nasal field is shorter than for objects in the temporal field as would be the case if the angle of incidence of light on the lens from objects in the nasal field was greater than from objects in the temporal field.

Case 20, Fig. 6, shows a pronounced asymmetry of the nasal and temporal halves of the field, but it appears to be an asymmetry due to a very considerable extent to the elongation of the eyeball in high myopia. An inspection of the curve representing the refraction in the vertical plane, the curve the shape of which is dominantly influenced by the shape of the eyeball, shows the maximum of myopia at about 15° to the nasal side of the center of the field or at a point on the retina about 15° to the temporal side of the fovea. The diopter value of the myopia here is about 7.25, at the center of the field it is 6. From the point of maximum value the curve rises more sharply towards the nasal than towards the temporal side of the field. However, at corresponding points in the two fields the myopia is greater in the nasal than in the temporal field. It is interesting to compare the shape of this curve with the shape of the eyeball in high myopia as is shown by the cross section of the eyeball given in Fig. 7. In the horizontal plane the value of the myopia at the center of the field was 5.5 diopters; at 35° it reached a maximum in the nasal field of 11.5 diopters; at 45° in the nasal field it was 9 diopters and at 55°, 1.5 diopters. In the temporal field the value of the myopia at 30° was 5.5 diopters and at 45°, 2.5 diopters. A comparison of the shape of the two curves and of the interval of Sturm in the temporal and nasal fields indicates an asymmetry in the refracting system as well as in the shape of the eyeball, such as might be produced by a rotation or tilting of the lens towards the temple. The test both with the Maddox rod and with prisms shows 10 prism diopters of exophoria. When fixation was taken with the right eye alone, a deviation of 20° towards the temple was shown. Measurements of the cornea showed no asymmetry and the value of the curvature in both meridians fell within the normal range.

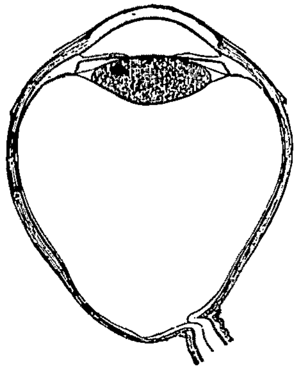

FIG. 7. CROSS-SECTION OF A MYOPIC EYE HAVING AN AXIAL LENGTH OF 28 MM. (From Fuchs: Textbook on Ophthalmology)

The color fields for red and blue, 1° stimuli, showed a contraction in the upper half. The field for 1° green was very small. The ocular deviation in this case was probably due to the high degree of myopia. Refractive asymmetry apparently had little or nothing to do with it. This may be inferred from the fact that when the myopia for the center of the field was corrected, the deviation practically disappeared and by the fact that a deviation of the eye either toward the temporal or the nasal side could not have been expected to render any very effective service in clearing up the defective imagery.

An inspection of Case 21, Fig. 6, left eye, shows asymmetry in the curves for both the vertical and horizontal planes. At the center of the field there was a high compound hyperopic astigmatism, 3 diopters in the vertical and 4.5 in the horizontal plane. In the nasal field in the vertical plane there was little change in the hyperopia as far from the center as 50°. In the temporal field it had decreased to 0.5 diopters at 25° and was 1 diopter at 50°. In the nasal field in the horizontal plane it decreased to zero at 35°; and at

50° there was 4.5 diopters of myopia. In the temporal field the hyperopia decreased to zero at 35°; and at 50° there was 7 diopters of myopia, just twice the amount present at the corresponding point in the nasal field. It is significant to note that the nearest approach to emmetropia in the eye came at 25° from the center in the nasal field. The defect here was 0.5 diopter of simple hyperopic astigmatism.

The vision in this eye was 20/200 and could not be improved substantially by correction. The patient stated that prior to 15 years ago there was, so far as he knew, no vision at all in this eye. An examination for central scotoma gave negative results. That is, while the test object was seen vaguely at the center it was seen equally clearly there and in the field surrounding the center for 15 degrees to 20 degrees in all directions. A test with the Haitz charts showed that when both eyes were open the vision in this eye was entirely suppressed. When the right eye was closed, however, the figure for the left eye in the chart was seen. When both eyes were open there was no observable deviation in either eye; but when the right eye was closed the left immediately turned inwards and took a fixation approximately 25° from the median plane. It will be remembered from Fig. 6 that the nearest approach to emmetropia in the left eye was at an eccentricity of 25° in the temporal field. When using this eye alone, the patient apparently turned the eye inwards about 25° in order that objects in front should be in the line of the least refractive error and the clearest image be formed on the retina.

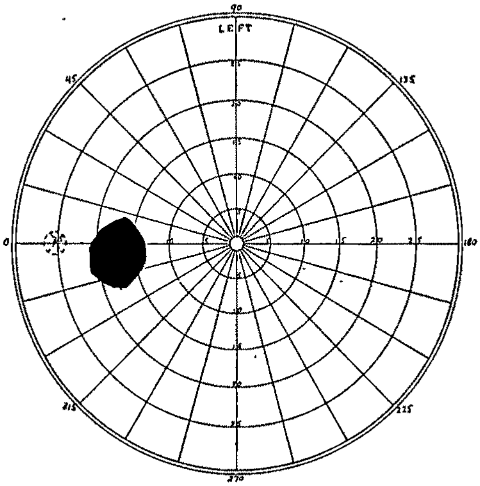

FIG. 8. BLIND-SPOT AND FIXATION-POINT FOR CASE 21

In the course of the examination of the eye the blind spot was mapped on the tangent screen of the Ferree-Rand perimeter with a 0.5° white stimulus-object on a black background. With the eye turned towards the center of the arc of the perimeter, the fixation-object could not be seen with sufficient clearness to give a satisfactory control of fixation. In order to secure a correct fixation, the left eye was lined up by means of the telescope inserted in the hollow axle of the perimeter. A large fixation-cross, 3 cm. in diam., was moved temporally along the horizontal meridian to the point at which it could be seen most clearly. The highest visibility was attained at a point approximately 25° from the center of the arc. This, it will be remembered, was the point in the field at which there was the smallest error refraction, 0.5 diopter simple hyperopic astigmatism. With the fixation-object at this point a fairly satisfactory control of fixation was secured and a map of the blind-spot was made. The location of the fixation-object and the map of the blind-spot are shown in Fig. 8.

A phenomenon of very great theoretical and practical interest observed during the course of the work may be noted here. During the early part of the work of the examination at the different point in the peripheral field was made by taking an eccentric fixation, the instrument remaining stationary. One of the reasons for abandoning this method of working was the fact that when the fixation was held for any appreciable length of time, the eyes of the class we have called Type A developed an increased myopia in the horizontal meridian with a corresponding decrease in the hyperopia for the vertical meridian. With prolonged fixation this change in some cases amounted to as much as 2.5 diopters. A similar tendency in the same direction was shown also by eyes of the class we have called Type B. Further, when the method was changed and a far fixation was taken in the median plane—the examination at the peripheral points in the field being made by rotating the apparatus about the fixed point described in the method of working—the phenomenon was still present in some eyes but to a lesser degree. There seem to be two possibilities of explaining the phenomenon: (a) a change in the convexity of the lens, and (b) the possibility that as the fixation is prolonged the muscles of the eye gradually produce an elongation of the eyeball.[3] The first of these explanations does not seem plausible because the rendering of the eye myopic by a change in the focus of the lens would operate against the clear seeing of the fixation-object. This would be in opposition to one of the eye's strongest reflex incentives; namely, the adjustment of its refracting mechanism to produce the clearest possible vision; while the gradual elongation of the eyeball because of the pressure exerted by the external muscles in maintaining fixation, falls entirely outside the reflex and is unavoidable.

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[3] It is assumed here that the eyes are under considerable pressure from the external muscles in a prolonged eccentric fixation, the amount depending upon the degree of eccentricity. It is also assumed that they are under some pressure in taking and holding far fixation in the median plane; that is, when relaxed and freed from all incentives to adjustment for clear seeing, as is the case with closed lids and in the dark, the eyes take a more converging position.

To test the point, the examination was made under as profound a cycloplegic as could be obtained with atropine. The performance of the Zeiss refractionometer is, it may be noted, not as satisfactory with as without a cycloplegic. With wide expansion of the pupil, produced by the cycloplegic, the test images that are formed by the refracting system of the eye are not nearly so clear and distinct as they are with smaller pupils. This affords another interesting example of the poor and unnatural performance of the refraction system of the eye with an expanded pupil. It was found possible, however, to make the examination under cycloplegic with a fair degree of satisfaction. The phenomenon was still present, but in a diminished amount.

In connection with the phenomenon two points of interest may be noted: (a) there is a bearing on the possibility of a permanent elongation of the eyeball by the pressure of the external muscles, one of the theories of the cause of myopia; and (b) the phenomenon strongly suggests that a temporary elongation may be produced in some eyes, presumably due to the action of the external muscles. This is of interest in connection with theories of accommodation and with the ability of certain aphacic eyes to see near objects with the same correction that is used for seeing distant objects.

SUMMARY AND CONCLUSIONS

(1) The refraction of the eye in the temporal and nasal peripheral field as far out as 60° has been studied by means of a Zeiss refractionometer modified to suit the purpose. Twenty-one eyes have been examined. Those selected included eyes with a slight central error of refraction and eyes with a pronounced central error of refraction.

(2) The eyes examined fall into three classes. In Type A, the eye becomes more myopic in the horizontal plane as the periphery of the field is approached and more hyperopic in the vertical plane. In all eyes examined but one (in which there was a high myopic astigmatism in the central field), this resulted in a mixed astigmatism in the peripheral field, the interval of Sturm varying over a considerable range of diopters. In Type B, the eye becomes less myopic in the horizontal plane and more hyperopic in the vertical plane. This results in a condition of compound hyperopic astigmatism in the peripheral field for eyes that are hyperopic, emmetropic

and moderately myopic in the central field. If there is strong myopia at the center of the field, it is obviously possible that a compound myopic astigmatism might result over the greater part of the peripheral field, weakening in amount as the periphery is approached. In Type C, the refractive condition is asymmetrical for the nasal and temporal halves of the field. The general trend of this asymmetry, as the periphery of the field is approached, is towards greater myopia and less hyperopia for one half of the field than for the other half.

(3) As fixation was prolonged, a tendency toward myopia was found; that is, if hyperopic, the eye tended to become less hyperopic; if emmetropic, it tended to become myopic; and if myopic, more myopic. This change was found to take place in all the eyes examined when an eccentric fixation was taken; also for some eyes when a far fixation was taken in the median plane. Further, the change was found to take place whether the examination was made with or without a cycloplegic. Two points of interest may be noted in connection with this phenomenon: (a) there is a bearing on the possibility that a permanent elongation of the eyeball may be produced by the pressure of the muscles, one of the theories of the cause of myopia; and (b) the phenomenon suggests that a temporary elongation may be produced in some eyes, presumably due to the action of the external muscles. This is of interest in connection with theories of accommodation and with the ability of some aphacic eyes to see near objects with the same correction that is used for seeing distant objects.

(4) The following additional points may be noted: (a) the important relation which the refractive conditions in the peripheral field sustain to acuity in the peripheral field, to achromatic and chromatic sensitivity in peripheral vision and the limits of the form and color fields, and to the anomalies and irregularities in peripheral space perception. The conditions for the formation of an image are so bad for objects in the peripheral field that one can only marvel that peripheral vision is as good as it is, and that the peripheral portions of the retina should have attained as high a development as they have.

(b) The possible bearing of asymmetrical refraction in the peripheral field on the explanation of cases in which central vision can not be substantially improved by correction in eyes which show no central scotoma.

(c) The rôle which asymmetrical refraction may play in cases of ocular deviation and the bearing that the demonstration of such a condition may have on the treatment of the case.

(d) The possibility of determining roughly the conformation of the retina and the shape of the posterior half of the eyeball by refracting the eye for the peripheral field; also of determining within rather wide limits in cases of refractive asymmetry whether the defect is in the refracting system, or in the conformation of the retina, or in both.

(e) The possibility and comparative practicability of using the Zeiss refractionometer, modified as described in this paper, for studying the refractive conditions in the peripheral field and in testing for refractive asymmetry. To give the instrument maximum serviceability for this purpose, however, a further modification should be devised to make the examination possible and feasible for the upper and lower as well as for the temporal and nasal halves of the field. Cases which might be tested to advantage for refractive asymmetry as clinic procedure are those in which a pronounced ocular deviation is combined with low central vision and there is no detectable scotoma.

It is stated above that one marvels that vision in the peripheral portions of the field is as good as it proves to be. In fact when one considers the type of image that is formed in case of an axial astigmatism, an astigmatism of incidence or a lens out of focus, in comparison with the power of vision that is actually present under these conditions, one is almost inclined to concede a supplementary resolving power to the rods and cones themselves. Some of the additional phenomena that seem to need explanation are: (1) the power of vision in general in ametropic eyes; (2) the power of vision in the peripheral field; (3) the power of seeing objects nearer and farther than the fixation-point; (4) the contention by Ames and Gliddon that the actual accommodation distance in viewing objects does not coincide with the apparent accommodation distance;[4] (5) the power which some aphacic eyes have of seeing objects at all distances with the same pair of glasses; (6) the effect of increase of intensity of illumination on acuity and the much greater effect on ametropic than on emmetropic eyes; and (7) the phenomenon of relative accommodation, on the power of separating accommodation from convergence without impairing vision, and the increase in this power with increase of intensity of illumination.

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[4] A. Ames, Jr., and G. H. Gliddon, Ocular measurements, *Trans. Amer. Med. Assoc., Section on Ophth.*, 1928, 102-169.

PITCH DISCRIMINATION NEAR THE UPPER AND LOWER THRESHOLDS OF AUDITION

By LAURENCE A. PETRAN, Peabody Conservatory of Music

This paper reports a study of pitch discrimination near the upper and lower thresholds of audition. Most previous work with extremely high and low tones has been concerned with determination of tonal stimulus-thresholds. This study was made in an endeavor to find out if there is a range of very low pitches which can be heard as tones but which cannot be discriminated as higher or lower than each other, and also if there is a like range near the upper threshold. It was undertaken as a result of a suggestion coming from Hardesty's work with a model designed to illustrate the probable action of the tectorial membrane.[1] He found that while different lengths of the elk-hide in his model vibrated for frequencies from *a* to *f′*, for frequencies below *a* the whole membrane vibrated. If the tectorial membrane in the ear should be the primary vibrating structure of the ear, and if it behaves in a fashion similar to Hardesty's model, then it might reasonably be expected that there would be a range at the lower end of the pitch series in which all frequencies would produce the sensation of the same pitch.

Values for the lower threshold tabulated by Titchener from nineteen references up to 1905 range from 7d.v. to "between 16 and 48."[2] Apparatus used includes large pipes, loaded reeds, rotating spokes, large tuning forks, and the Appunn lamella. Many authors mention that it is easy to mistake the first overtone of one of these sound generators for the fundamental. Dunlap and Wells outline the following method for avoiding this error in work with the lower threshold.[3] *O* is presented with a tone from a tuning fork set to 42 d.v.; if he hears this plainly as a tone he compares it with a tone from a 21 d.v. fork. If the tones from the second fork seem clearly lower than tones from the first, then lower pitches always an octave apart are to be compared until *O* judges the tones from both forks to be not different. If the tones from the forks set at 21 and 42 d.v. are judged the same, higher pairs are to be compared until a point is reached at which tones from the

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[1] I. Hardesty, A model to illustrate the probable action of the tectorial membrane, *Amer. J. Anat.*, 18, 1915, 471-514.

[2] E. B. Titchener, *Experimental Psychology, Quantitative Experiments, Instructor's Manual*, 1905, 24-31.

[3] K. Dunlap and G. R. Wells, *Laboratory Exercises in Psychology*, Part I, 159-161.

two forks are judged different. Dunlap and Wells suggest that when tones from forks set an octave apart seem the same in pitch "the tone of the slower fork is the second partial of its fundamental note; the partial being probably an artifactual one produced in the middle ear."[4]

Vance reports that 5 trained *Os* after practice could hear tones from 12 and 13 d.v. forks.[5] It is still a question whether these *Os* actually heard tones of 12 d.v. or an artifactual overtone. Fletcher emphasizes that intensity is an important factor in the determination of the lower threshold and that most previous work is inconclusive because this factor has been neglected.[6] The upper and lower thresholds in the audiograms of Fletcher[7] and Wegel[8] are obtained by extending the curves for minimum audible pressure and maximum audible pressure. Wegel says, "As the frequency is still decreased (below 60 d.v.) to the point of intersection of the feeling and hearing lines it is difficult to distinguish between the sense of hearing and that of feeling. . . . Below the point of intersection it is easier to feel than to hear the air vibrations. . . . At the lower limit the excursions of the diaphragm and ossicles are probably so large that the nerves feeding these movable parts are stimulated."[9] The resulting flutter sensation constitutes a practically inescapable secondary criterion in tests with low tones.

Stückert,[10] Gradenigo,[11] Vance,[12] and Kuehl[13] have all found difference sensitivity finest in the middle range and increasingly grosser towards the extremes. Maltzew found interval judgment much more faulty at very high and low ranges than at the central.[14] Achmatov found no differential adaptation effect for three widely scattered pitches.[15]

EXPERIMENT I

Apparatus and procedure. For the production of low tones as pure as are available at present three large tuning forks were used in Experiment I. The weights on the largest fork in Experiment I could be set so that it would give from 16 to 24 d.v., the middle sized fork could be set from 24 to 38, and the smallest from 36 to

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[4] *Ibid.*, 161.

[5] T. F. Vance, The lower limit of tonality, *Psychol. Monog.*, 16, 1914, (no. 69), 104-114.

[6] H. Fletcher, Physical measurements of audition and their bearing on the theory of hearing, *J. Franklin Inst.*, 196, 1923, 289-326.

[7] *Ibid.*

[8] E. P. Fowler and R. L. Wegel, Audiometric methods and their applications, *Trans. Amer. Laryng. Rhinol. and Otol. Soc.*, 1922, 98-131; R. L. Wegel, The physical examination of hearing and binaural aids for the deaf, *Proc. Nat. Acad. Sci.*, 8, 1922, 155-160.

[9] Wegel, *op. cit.*, 157.

[10] N. Stückert, Über die Unterschiedsempfindlichkeit für Tonhöhen in verschiedenen Tonregionen, *Zsch. f. Sinnesphysiol.*, 42, 1908, 392-408.

[11] G. Gradenigo, Über die Empfindlichkeit des Gehörorgans für die verscheidenen Regionen des Tonleiters, *Arch. f. Ohrenhlk.*, 96, 1914, 103-114.

[12] T. F. Vance, Variation in pitch discrimination within the tonal range, *Psychol. Monog.*, 16, 1914, (no. 69), 115-149.

[13] B. F. Kuehl, Measurements of auditory acuity with the Iowa pitch range audiometer, *Psychol. Monog.*, 31, 1922, (no. 140), 83-97.

[14] C. von Maltzew, Das Erkennen sukzessiv gegebener musikalischer Intervalle in den äusseren Tonregionen, *Zsch. f. Psychol.*, 64, 1913, 161-257.

[15] A. S. Achmatov, Über die akustische Adaptation, *J. f. Psychol. u. Neurol.*, 31, 1925, 428-442.

55. They were clamped to a heavy oak table at fixed positions 2 in. apart. The table was placed near a corner of the room.

A black cloth draped over a standard concealed the apparatus from the *O*s. A conical megaphone whose diameter at the large end was approximately 13 in., and whose long axis was a little less than 28 in. was hung so that the central part of the large end was about 1½ in. from the upper halves of the two forks in use, and the other end protruded through the curtain at the height of *O*'s ear. *O* sat on a stool the height of which could be adjusted so that he could comfortably hold his ear at the opening. The use of the megaphone enabled *O* to hear the tuning fork tones while at sufficient distance to obviate spatial cues from the positions of the forks.

The fundamental tone of the megaphone, obtained by blowing a jet of air across its mouth, was located at about *g*♯ by several *O*s comparing it with a tone obtained in the same manner from a Helmholtz resonator. This was much higher than the pitch of the highest fork tone used and hence, according to Miller,[16] this horn should not have distorted the tuning fork tones appreciably.

A set of wooden clips was used to set the forks in vibration. These were made from small wooden blocks by chiseling out in them shallow rectangular holes wide enough to hold the two prongs of a fork when these were pulled together. When *E* drew off the clip the forks would vibrate immediately. Several graduate students assisted in selecting the clips to be used for various frequencies. They sat with their ears to the end of the megaphone while the experimenter tried various clips for the octave pairs of forks, and reported when tones from the two forks seemed most alike in intensity. It was not possible to get equal intensities for all pairs of forks as the *O*s disagreed as to intensity differences. The clips used gave a tone of moderate to weak intensity; louder tones would have been desirable but could not be obtained without increasing a great deal the disturbance from forced vibration in the table and floor. Felt pads were placed under the table legs to minimize this forced vibration, and a heavy curtain several times folded was placed unevenly on the floor below the table to prevent the air space below the table from acting as a resonance chamber for any one frequency. *E* kept his recording material in clamps fastened to the wall so that no vibratory object of any kind was present on the table.

Until oscillographic or galvanometric records of these fork tones are made and analyzed it will not be known what tones they produced besides the fundamental. The 6.27 or another overtone was heard as a little, rather faint 'singing' tone in the middle range by some of the *O*s[17]; they were told to disregard this by *E*, who compromised his honesty to the extent of telling them that this tone would not help them one way or another in their judgments. It did not occur to him till later that this might have acted as a negative suggestion. Most of the *O*s claimed to have disregarded this overtone, though there is no telling how much it may have influenced them unconsciously. Whether the second harmonic partial was objectively present is another question. If present in one fork at one frequency it probably was present at all times with all frequencies, since the material of the fork and the manner of generating the tone was the same for all. However, the relative position of the weights on the forks varied. On the largest fork the weights were close to the top of the prongs for 16 d.v. and rather close to the center for 24.

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[16] D. C. Miller, *The Science of Musical Sounds*, 1916, 156-162, 186-188.

[17] S. H. Anderson, Overtones of large tuning forks, *Phys. Rev.*, 21, 1923, 692-694.

This technique for producing low tones, while adequate for a preliminary study such as this, would be imperfect and unsatisfactory for final results, but perhaps no more so than other means available at present. If the forks had been set in vibration by a narrower clip giving the prongs a greater original displacement the forced vibration in the floor would have been increased. Oscillatory electric apparatus if used with a telephone receiver diaphragm would give a tone containing upper partials due to the diaphragm as well as the cue from the flutter sensation.

At the beginning of the experiment *O* was seated with his ear to the small opening of the megaphone. He was told that he would hear a sound which he was to describe. Then one of the tuning forks was sounded. Most of the *O*s described the sound as a rumble, some as a flutter, one as a "moan," another as "like a train coming in." After tones of several different vibration rates had been sounded the *O*s were able to accept the sounds as tones having a certain amount of pitch. *O* was then told that he would be given a series of pairs of tones and that he was to judge the second tone of each pair as higher, lower, or the same in pitch as the first. Two forks were then set to frequencies an octave apart. Each series was made up of 12 pairs: 3 in which tones from the higher fork preceded those from the lower, 3 in which the lower preceded the higher, 3 in which the higher fork was sounded twice, and 3 in which the lower fork was sounded twice. *E* gave a ready signal, sounded one fork for 1½-2 sec., damped it with the gloved hand, and after a pause of about 1 sec. sounded the same fork or the other fork for 1½-2 sec. The order of the pairs within the series of 12 experiments followed lists of orders made up by chance. Nine series were given: one each for the octave pairs 16-32, 17-34, 18-36, 19-18, 20-40, 21-42, 22-44, 23-46, and 24-48 d.v. The succession in which these series were given was from high to low (or from low to high) but shuffled, as for example: 16-32, 19-38, 17-34, 18-36, 21-42, 20-40, 24-48, 23-46, 22-44. It was thought that this shuffling might help prevent the carrying over of secondary criteria from high overtones, etc., from one octave pair to the next given pair.

There were 12 *O*s in this experiment, all of them advanced students at the Peabody Conservatory of Music (except *Q*, who had previously studied there). They all had pitch discrimination thresholds within 3 d.v. at 435 d.v. The experimental periods were rather irregularly spaced: at first most *O*s found this experiment difficult and fatiguing, so not more than two series were given a day. Later, as the *O*s became more adapted to the experiment, 4 or 5 series were given a day. The time which the *O*s could give to experimental work varied from day to day so that on some days several series could be given and on others none at all. Distracting factors were the moan of the wind about the corner of the building, noises from automobiles, trucks, and sometimes from airplanes. In cases where one or both tones in a pair were not clearly heard the pair was repeated later in the series, so that for each series 12 comparatively clear judgments were obtained.

The *O*s (*A, B, C, D, F, G, H, L, M, N, P* and *Q*) gave 12 judgments each on the 9 octave pairs of forks from 16-32 to 24-48 d.v. Nine of the *O*s (all but *C, F,* and *Q*) repeated the experiment. Thus a total of about 2268 judgments was received.

TABLE I

Total Number of Judgments of All Os on Each Pair of Forks

		Sequence											
		HL			LH			HH			LL		
Forks	Section	L	H	S	L	H	S	L	H	S	L	H	S
16–32	1	18	13	5	9	22	5	6	6	24	6	10	20
	2	21	6	0	12	15	0	1	3	23	4	6	17
	T	39	19	5	21	37	5	7	9	47	10	16	37
17–34	1	14	13	9	9	17	10	2	8	26	9	7	20
	2	20	3	4	9	15	3	4	4	19	3	2	22
	T	34	16	13	18	32	13	6	12	45	12	9	42
18–36	1	8	19	9	16	14	6	4	5	27	6	10	20
	2	14	11	2	10	16	1	7	2	18	4	5	17
	T	22	30	11	26	30	7	11	7	45	10	15	37
19–38	1	14	19	3	19	12	5	4	10	22	4	12	20
	2	7	18	2	19	6	2	5	1	21	1	5	21
	T	21	37	5	38	18	7	9	11	43	5	17	41
20–40	1	10	22	4	24	9	3	7	4	25	7	8	21
	2	9	12	6	12	7	8	6	2	19	3	5	19
	T	19	34	10	36	16	11	13	6	44	10	13	40
21–42	1	10	18	8	21	7	8	9	5	22	8	9	19
	2	11	16	0	15	6	6	3	3	21	7	4	16
	T	21	34	8	36	13	14	12	8	43	15	13	35
22–42	1	10	20	6	16	11	7	7	6	22	1	14	20
	2	6	15	6	20	4	3	3	3	21	5	7	15
	T	16	35	12	36	15	10	10	9	43	6	21	35
23–46	1	14	18	4	16	13	7	7	5	24	4	10	22
	2	8	17	2	19	6	2	3	3	21	4	3	20
	T	22	35	6	35	19	9	10	8	45	8	13	42
24–48	1	15	20	1	18	12	6	6	4	26	4	4	28
	2	6	18	3	18	8	1	3	4	20	7	0	20
	T	21	38	4	36	20	7	9	8	46	11	4	48

Results. The total number of judgments of higher, lower, and same for each sequence on each fork pair is given in Table I. In this table judgments for each sequence for each fork pair total 36 for the first section and 27 for the second, since while 12 *O*s completed the first section only 9 repeated it. The total number of judgments on all fork pairs for every *O* are given in Table II.

From these tables three conclusions may be drawn. First, the judgments on the HL (high-low) and LH (low-high) pairs are very erroneous. The judgments on the LL (low-low) and HH (high-high) are somewhat better but there is still a great deal of uncertainty. Secondly, there is a tendency for the same fork tone to be judged 'same' (S) more often in the second

TABLE II

Total Number of Judgments on all Forks for Every *O*

		Sequence											
		HL			LH			HH			LL		
O	Section	L	H	S	L	H	S	L	H	S	L	H	S
A	1	7	12	8	13	8	6	3	6	18	4	11	12
	2	20	4	3	10	13	4	8	0	19	6	10	10
B	1	10	17	0	20	6	1	4	6	17	8	5	14
	2	8	18	1	19	7	1	0	1	26	1	1	25
C	1	9	15	3	6	13	8	1	3	23	1	6	20
D	1	6	15	6	19	4	4	4	3	20	3	5	19
	2	8	13	6	19	5	3	0	2	25	0	0	27
F	1	12	12	3	9	12	4	4	6	16	6	6	14
G	1	13	13	1	18	8	1	9	5	13	4	8	15
	2	9	18	0	18	5	4	2	1	24	4	1	22
H	1	12	9	6	10	14	3	4	3	20	4	9	14
	2	10	16	1	16	9	2	4	6	17	3	5	19
L	1	3	21	3	13	12	2	6	12	9	3	15	9
	2	10	16	1	16	10	1	1	5	21	3	10	14
M	1	18	6	3	6	13	8	12	2	13	13	7	7
	2	18	5	4	7	14	6	11	7	9	12	8	7
N	1	9	11	7	8	10	9	1	4	22	0	5	22
	2	9	12	6	11	8	8	2	1	24	1	0	26
P	1	10	15	2	17	7	3	4	2	21	3	2	22
	2	10	14	3	20	6	1	7	2	18	4	2	21
Q	1	4	16	7	9	10	8	0	1	26	2	5	20

section of the experiment than in the first. This tendency is more marked with the LL sequence: *B, N, D, H, L,* and *G* gave more judgments of 'same' on LL pairs in the second section than in the first, while *A, M,* and *P* gave about the same number in both. With the HH sequence *B, D, G,* and *L* gave more judgments of 'same' in the second section; *A* and *N* gave about the same number; and *H, M,* and *P* gave fewer. Thirdly, the HL and LH pairs on which the majority of judgments are correct are the two lowest pairs: 16-32 and 17-34; the other higher pairs received a majority of incorrect judgments. This is in direct contradiction to the natural expectation that the higher pairs would be the more correctly judged.

These results from *O*s with fine pitch discrimination at ordinary ranges are significant, because while Stumpf found some evidence that a tone could be confused with its octave, there is no reason for expecting a tone an octave higher than another tone, to be rather consistently reacted to as lower. It may be that intensity differences in the fork tones are somewhat to account for this and the other discrepancies, but if so, it is significant that the pitch discrimination of advanced music students should be so poor at this range as to allow an incorrect system of pitch judgments to be built up from the very moderate intensity differences present in this experiment. Our *O*s would never at ordinary pitch ranges judge a loud *c′* an octave higher than a softer *c*, or a faint *A* an octave higher than a louder *a*. Another possible explanation is that if from a 20 d.v. fork only an artifactual overtone of 40 d.v. is heard this overtone may sound lighter in quality or at least different from the objective vibrations of a 40 d.v. fork, which supposedly would also produce its own artifactual overtone of 80 d.v. If the 40 d.v. artifact sounds lighter than the objectively produced 40 d.v. tone it might come to be consistently regarded as higher. But the clips used were selected empirically to give approximately equal intensities, the *O*s were instructed to judge on the basis of pitch and all reported trying to do so, and while *O*s were only occasionally questioned as to intensity differences in the fork tones they sometimes reported the higher tone as louder than the lower, and sometimes the reverse. It is possible, though, that many of the correct judgments of 'same' were due not only to a feeling of sameness in pitch but also to a feeling of sameness in intensity and vibration as well.

*O*s were not questioned during the experiment as to whether they judged the tones with faster vibrations lower or higher unless they brought up the point themselves. Such questioning was avoided because it was not desirable to direct the attention to the vibration sensation as a criterion of pitch, but this may have been a useless precaution. *B* said she judged the tones vibrating faster as higher, those vibrating slower as lower. *Q* did the reverse. *M* on the 18-36 series and *N* on the 17-34 each reported that tones could seem to be the same pitch but "coming at a different rate;" *M* didn't see how this could be. *Q* reported much difficulty in judging the pitches of these tones and said he was forced to judge the "lighter quality higher." *H* reported of the 24-48, 21-42, 17-34, and 16-32 series that the higher tones (really lower for the first two pairs and higher for the last two) were harder to hear; maybe they had a low fundamental that he couldn't hear. Later he reported that in cases where one tone (23 d.v.) was judged a twelfth higher than the other (46 d.v.) that the interval really sounded like a fifth but that he felt that the fundamental was lacking in the second tone (23 d.v.).

The *O*s were fairly consistent among themselves in their reports of the intervals between different tones. *A, B, C,* and *L* reported that tones from the lower pairs seemed a semitone or whole tone apart; *N* reported hearing thirds and *P* sevenths or ninths at these points. The middle pairs were called a semitone or whole tone apart by *B, C, L,* and *N,* and a third apart by *A, D, L, N,* and *P.* The higher pairs were judged as a fourth, fifth, or more apart by *A, C, D, G, L,* and *P.* These interval judgments were not required but the *O*s were usually asked at the end of each period if they had anything to say about the intervals between notes judged as different. Some of the *O*s expressed dislike for this experiment because it caused a strain on the attention and they felt so much at sea in their judgments.

TABLE III

JUDGMENTS ON TONES FROM PAIRS OF LOW TUNING FORKS HELD SUCCESSIVELY TO O'S EAR

		Sequence											
Forks	*O*	HL			LH			HH			LL		
	D	L	L	L	H	H	H	S	S	S	S	S	S
	F	L	S	L	L	S	L	S	L	S	S	S	S
16–32	*G*	S	L	L	L	H	H	S	S	H	L	S	L
	K	S	L	L	H	H	L	L	S	L	L	L	S
	N	S	S	S	S	S	H	S	S	S	S	S	S
	D	L	L	L	H	H	H	S	S	S	S	S	S
	F	L	L	L	H	H	H	S	S	S	L	L	S
19–38	*G*	L	L	L	H	L	S	S	L	H	H	S	S
	K	L	L	L	H	H	H	S	L	L	L	S	S
	N	S	S	L	H	H	S	S	S	S	H	S	H
	D	L	L	L	S	S	S	S	S	S	S	S	S
	F	L	L	L	H	L	L	L	L	S	H	L	S
21–42	*G*	S	H	L	H	L	H	H	H	S	S	H	S
	K	L	L	L	H	H	H	S	S	S	S	S	S
	N	L	L	L	H	H	H	S	L	L	S	S	S

At the end of this experiment 5 *O*s were put through a series with the 16-32, 19-38, and 21-42 octave pairs. In this case the forks were struck on a sandbag and held to *O*'s ear. The higher forks were held from 5-6 in. from the ear and the lower forks much closer to minimize intensity differences. This technique does not exclude cues or disturbances from the flutter sensation, which was quite noticeable, nor from the striking: the lower fork had to be struck more strongly than the others. Table III gives the judgments on these pairs. The total number of judgments is too few for any thorough comparison of this method with the other, but it will be noted that the frequency of correct judgments is much greater. These tones had a greater intensity than those in the main experiment and it is possible that these tones, being stronger would have stronger artifactual overtones: what might be heard from the 16 d.v. fork would be a 32 d.v. artifact, and from a 32 d.v. fork the objective 32 d.v. tone and a 64 d.v. artifact; naturally the second would sound higher. There is also a possibility that tuning forks struck as hard as these would objectively produce the first overtone.

Summary and conclusions. To conclude, music students with pitch discrimination well above the average have great difficulty in comparing the pitches of octave pairs of tones from 16 to 48 d.v. produced in the manner described above, they cannot do so accurately, and are evidently often forced into the use of secondary criteria from intensity and vibratory sensations. If it should be true that discrimination is better for tones louder than those used in the main experiment, then our results point to a relation between intensity and discrimination at this pitch range, something for which there is absolutely no parallel at ordinary ranges for pitch differences so large and intensity differences so small.

EXPERIMENT II

Apparatus and procedure. A supplementary experiment on pitch discrimination at low ranges was carried out with notes from the Bourdon unit stop in a Moller pipe organ.[18] This unit consisted of stopped wooden pipes and gave comparatively pure tones, the purest available in the organ at as low a pitch range (about 32–44 d.v.).[19]

Eight *O*s took part in this experiment. Preliminary tests showed that all could correctly distinguish as higher or lower tones a half tone apart at ranges above 65 d.v. They were tested individually, sitting in the choir loft of the church with their backs to *E*, who sat at the console. They were told that they would hear a series of pairs of tones and that they were to judge each time whether the second tone of each pair was the same as, higher than, or lower than the first tone. The following tones were used in the test: CC = 32.7 d.v.; C♯C♯ = 34.6; DD = 36.7; D♯D♯ = 38.9; EE = 41.2; and FF = 43.6. These vibration numbers are approximate; they were computed by dividing those of the once accented octave by 8. The pairs were given in different chance orders and a series consisted of 36 pairs in which every note of the 6 followed itself and every other note once. The series was given 5 different times to every *O* on different days. Thus a total of 1620 judgments was taken, 180 for each *O*.

Distractions in this experiment consisted of street noise, mostly from street cars and trucks. Pairs which were not clearly heard on this account were repeated later in the series. There was also some accessory noise from the sounding of the pipes themselves but not of such a nature as would serve as a differential cue.

Before every experiment *E* gave a ready signal. Then the first tone was sounded for a period of 1½–2 sec., then after a pause of about 1 sec. the second tone was sounded for 1½–2 sec.

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[18] Opus 4991, installed in 1927 in the Wilson Memorial Methodist Episcopal Church, South, located at Charles Street and University Parkway in Baltimore.

[19] The organ had been recently tuned and the pipes were voiced so as to give within this range tones of approximately equal intensity.

All the *O*s knew that organ tones were being used for the test and hence that a discrete scale was being used. Hence there may have been some tendency to call tones that seemed less than a semitone apart the same, when a difference was actually perceived.

Results. The results from this experiment are shown in Tables IV–VII. Table IV gives for every *O* the total number of errors made each day, the total number of errors for the series, and the percentage that the total number of errors is of the total number of judgments. Every *O* gave 36 judgments a day, making a total of 180 for the series. There is not much variation in number of errors from day to day: the greatest is 9 (from 9 to 18 for *C*). *C* is the only one to show consistent improvement; *M* and *F* became more inaccurate and the others remain at about the same level from the first day to the fifth. *B*'s errors are so few that they may be ascribed to attention lapses but the percentages of error of the others are formidable; those of *A*, *H*, and *M* are better than chance (67%) but still very large.

TABLE IV

Number of Errors Made by Every O on Every Day of the Experiment

	Observers							
Day	*A*	*B*	*C*	*F*	*H*	*L*	*M*	*N*
1	12	0	18	5	18	12	16	10
2	19	5	17	6	19	7	18	9
3	16	3	11	10	17	8	23	7
4	18	7	10	10	17	7	24	10
5	15	3	9	12	15	6	22	9
Total	80	18	65	43	86	40	103	45
% of all	44	10	36	24	48	22	57	25

TABLE V

Number of Each Type of Errors Made by Every O

Type of Error	Observers							
	A	*B*	*C*	*F*	*H*	*L*	*M*	*N*
L judged as H	14	3	14	1	19	7	42	4
L judged as S	16	3	20	11	24	11	12	16
S judged as D	9	6	2	8	12	5	7	4
H judged as L	30	3	13	14	12	8	15	9
H judged as S	11	3	16	9	19	9	27	12

Table V shows the number of times each *O* judged the second tone of a HL pair to be higher or the same as the first, the number of times he judged the same notes to be different, and the number of times he judged the second tone of a LH pair to be the same or lower. Six *O*s (*A*, *C*, *F*, *H*, *L*, and *N*) judged the second note of a HL pair to be S a little more often than H, and there is a like tendency for the LH pair. Since there were a total of 75 each of the HL and LH pairs in the series and 30 in which the same tone was given twice, Table VI was made up to show the percentages of each type of error. This table shows that except for *B* (and *F* to a certain extent) pairs of the same tones were judged more accurately than pairs of different tones, yet it is very significant that the same tone should be so often judged to be different. Table VII gives the percentage of times that the same notes and notes one, two, three, four, and five semitones apart were judged erroneously. The percentages for the perfect prime are based on 30 judgments, for the minor second on 50 judgments, for the major second on 40 judgments, for the minor third on 30 judgments, for the major third on 20 judgments, and for the perfect fourth on 10 judgments.

TABLE VI

PERCENTAGES OF EACH TYPE OF ERROR MADE BY EVERY O

Type of Error	Observers							
	A	*B*	C	*F*	*H*	*L*	M	*N*
L judged S or H	40	8	45	16	57	24	72	27
S judged H or L	30	20	7	27	40	17	23	13
H judged S or L	55	8	39	31	41	23	56	28

TABLE VII

PERCENTAGE OF ERROR ON EACH INTERVAL MADE BY EVERY O

Interval	Observers							
	A	*B*	C	*F*	H	*L*	M	*N*
Perfect prime	30	20	7	27	40	17	23	13
Minor second	54	20	60	34	62	32	72	48
Major second	40	5	40	20	38	23	63	25
Minor third	43	0	33	17	50	23	60	13
Major third	50	0	25	20	40	5	45	10
Perfect fourth	50	0	20	10	50	20	80	10

B has a regular diminution of errors as the interval becomes larger; *C* and *N* have the most errors on the tones a semitone apart and a decrease as the interval becomes larger; the rest show little consistent change.

Summary and conclusions. This experiment shows that musically trained *O*s are much confused in their judgments of these low pitches. It reënforces the conclusion from Experiment I that pitch discrimination at these low ranges is much more difficult and erroneous than pitch discrimination in ranges above 64 d.v. The increase in difficulty and number of errors seems quite out of proportion to the short distance that the 32-44 range is below the range from about 64 upward, in which errors are never made in judging successive tones a half tone apart.

EXPERIMENT III

Experiment III is concerned with pitch discrimination near the upper tonal threshold.

Titchener gives a full account of the work done on the upper threshold up to 1905.[20] Since then studies on the upper threshold have been published by Helmholtz,[21] Gildemeister,[22] Pratt,[23] and Schlittler.[24] Lane computed the relation of intensity to audibility at extremely high pitches,[25] but Möller found variations in intensity above threshold intensity to have no regular effect on the tonal threshold.[26] Humes found that practice decreased the upper tonal limen and ascribed this to the *O*'s becoming more critical as the experiment progressed.[27] Apparatus for upper threshold work has usually been some form of the Galton whistle or Schulz monochord, both with their recognized limitations, and König's cylinders, which are difficult to calibrate.[28]

Apparatus and procedure. The procedure used was similar to that in Experiment II. The sixth accented octave on a set of König's cylinders was found to be within the threshold of audibility of all the *O*s and was used for the experiment. The bars used and the vibration numbers on them were: $c = 8132$, $d = 9216$, $e = 10240$, $f = 10922.67$, $g = 12288$, $a = 13653$, and $b = 15360$. *O* was seated on a stool 4 ft. from the cylinders; this was the distance used in the work of Möller, Humes, and others and was thought to be close enough for clear hearing and far enough away to eliminate cues from the spatial position of the cylinders. *O* was told that he would be given a series of tones and that he was to judge the second tone in each pair as higher, lower, or the same in pitch as the first. The pairs were made up so that each tone followed itself and each other tone once in each experimental period, and the pairs were given in chance orders. This amounted to 49 judgments a period, and as the test was given on 5 different days every *O* gave a total of 245 judgments. Twelve *O*s (*A, B, C, F, G, H, K, L, M, N, P* and *Q*) participated in this experiment.

E struck the bars with a steel hammer held in his right hand and damped them a little over a second later with a piece of felt held in his left hand so that the duration of tones from all cylinders might be kept relatively constant. He attempted to keep the intensity of the strokes as constant as possible.

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[20] Titchener, *op. cit.*, 41-46.

[21] W. Helmholtz, Vergleich zwischen der Galtonpfeife und dem Schulzeschen Monochord zur Bestimmung der oberen Hörgrenze, *Diss. Berlin,* 1910, 1-18.

[22] M. Gildemeister, Untersuchungen über die obere Hörgrenze, *Zsch. f. Sinnesphysiol.*, 50, 1919, 161-191.

[23] C. C. Pratt, Highest audible tones from steel cylinders, this JOURNAL, 31, 1920, 403-406.

[24] E. Schlittler, Die Bestimmung der oberen Tongrenze mittels der Galtonpfeife nach Prof. Karl Ludolf Schäfer, *Acta Oto-laryngol.*, 10, 1927, 81-89.

[25] C. E. Lane, Minimum sound energy for audition for tones of high frequency, *Phys. Rev.*, 19, 1922, 492-497.

[26] E. F. Möller, The effect of change of intensity upon the upper limit of hearing, this JOURNAL, 33, 1922, 570-577.

[27] J. F. Humes, The effect of practice upon the upper limen for tonal discrimination, *ibid.*, 42, 1930, 1-16.

[28] E. G. Boring, The construction and calibration of König cylinders, *ibid.*, 38, 1927, 125-127; S. W. Fernberger, An improved apparatus for actuating tonal cylinders of the König type, *idem,* 120-124.

All the *O*s but *A* said that they liked this experiment but that it required close attention. *A* said that the tones hurt his ears, but many others said they sounded like sleighbells, etc., and were interesting. All the *O*s without exception reported that they based their judgments on the 'ring,' not on the 'knock.'

Results. Table VIII shows for every *O* the number of errors made every day, the total number of errors for the experiment, and the percentage that this total was of the total number of judgments. In general there is a certain level in number of errors maintained by every *O* throughout the experiment. *A* and *Q* are the only ones to show consistent improvement. If attention lapses are allowed for, the scores of *L*, *F*, *C*, and *G* could be considered very good, but the scores of the other *O*s point to a genuine difficulty in discrimination. Table IX shows the number of errors of each type that every *O* made on the HL, LH, and same pairs; and since for each of the unlike pairs an *O* made 105 judgments and for the like pairs he made 35 judgments, in Table X is given the percentage that the number of errors of each type was of the total number of judgments on that type. Thus, 19% of the judgments *A* made on a HL pair were erroneous. This table shows

TABLE VIII

NUMBER OF ERRORS MADE BY EVERY *O* ON EVERY DAY OF THE EXPERIMENT

Day	Observers											
	A	*B*	*C*	*F*	*G*	*H*	*K*	*L*	*M*	*N*	*P*	*Q*
1	17	15	9	10	10	10	18	5	21	24	10	26
2	14	11	8	4	5	14	12	7	19	20	7	27
3	14	10	9	6	8	11	12	5	24	28	19	21
4	13	13	5	7	10	9	15	2	24	20	22	19
5	6	11	7	6	10	17	19	7	20	30	11	19
Total	64	60	38	33	43	61	76	26	108	122	69	112
% of all	26	24	16	13	18	25	31	11	44	50	28	46

TABLE IX

NUMBER OF EVERY TYPE OF ERROR MADE BY EVERY *O*

Type of Error	Observers											
	A	*B*	*C*	*F*	*G*	*H*	*K*	*L*	*M*	*N*	*P*	*Q*
L judged as H	4	24	5	8	11	23	6	7	28	48	39	21
L judged as S	16	15	11	4	17	9	1	7	41	22	2	33
S judged as H	0	2	2	2	1	1	3	0	1	2	13	0
S judged as L	6	3	0	4	0	0	16	0	1	1	4	1
H judged as L	25	7	5	7	3	21	48	6	12	28	9	28
H judged as S	13	9	15	8	11	7	2	6	25	21	2	29

TABLE X

PERCENTAGE OF EVERY TYPE OF ERROR MADE BY EVERY *O*

Type of Error	Observers											
	A	*B*	*C*	*F*	*G*	*H*	*K*	*L*	*M*	*N*	*P*	*Q*
L judged S or H	19	37	15	11	27	30	7	13	66	67	39	51
S judged H or L	17	14	6	17	3	3	54	0	6	9	49	3
H judged S or L	36	15	19	14	13	27	48	11	35	47	10	54

that except for *F, K,* and *P* pairs of same tones are more often correctly judged than pairs of different tones. The three *O*s showing the reverse tendency probably built up in the course of the experiment criteria differing from the criteria of the others.

Pitch discrimination in this high octave is inaccurate for all the *O*s. *C, F, L,* and *G* show such good scores, however, that the hypothesis becomes plausible that after proper training the only errors that would be made on this test would be those due to attention lapses. But it is another question whether or not reactors could be trained to discriminate accurately differences much less than 1000 d.v. in this pitch region.

GENERAL CONCLUSIONS

Below 50 d.v. *O*s with pitch discrimination better than the average have great difficulty in telling which of two tuning-fork tones an octave apart is the higher; they are often forced into the use of secondary criteria and build up therefrom a system of wrong judgments. They do better with organ pipe tones from 32–44 d.v., but these have some overtones. Further work should be done on this problem when apparatus is available for producing absolutely pure tones from say 10 to 50 d.v. and when secondary criteria can be either eliminated or made of neutral effect. Final determination of the lowest objective vibration rate which can stimulate the cochlea must of course wait until tests are made with absolutely pure tones on people who have neither the tympanic membrane nor the ossicles. (This assumes that neither the external auditory meatus nor the tympanum would contribute a perceptible overtone differing for each low objective tone.) In persons with normal hearing the rate of flutter in low tones would always give a secondary criterion in judging low pitches, whether or not the tones are distinguishable from each other by their pitch characteristic alone. The only other approach to the final determination of the lower pitch threshold would be experimentation with *O*s whose sensory nerves leading from the membrane and ossicles had been anaesthetized or blocked in some way or other. The artificial production of this in many *O*s seems hardly feasible.

Nevertheless, the results of these experiments indicate that it is very doubtful that the cochlea itself responds to vibration rates slower than about 30 per sec.

Experiment III should be followed up by a learning experiment. If *O*s with more than ordinarily fine pitch discrimination in the middle register cannot learn to discriminate pitches of the diatonic scale above 8000 d.v. or even above 4000, this result would be difficult to explain on any current theory of audition.

THE COMPARATIVE EFFECTS OF SOCIAL AND MECHANICAL STIMULATION ON MEMORIZING

By JOSEPH PESSIN, University of Wisconsin

There have been a number of studies seeking to investigate the effects of mechanical stimuli, often called distractors, on various functions. The experiments of Evans,[1] Cassel and Dallenbach,[2] Morgan,[3] Tinker,[4] Hovey,[5] and Ford[6] may be mentioned. In general these studies have found that the initial effect of mechanical stimuli is disturbing, interfering with the efficiency of the particular function investigated. Later, individuals become accustomed to the mechanical stimuli, habituation sets in, and the function tested is as efficient as it was prior to the introduction of the so-called distractors.

The influence of social stimulation on various activities has also been investigated. An excellent summary of the earlier work is given by Allport,[7] and of the more recent work by Murphy.[8] No general statement of social influences on human activity can be made, since these influences vary with the nature of the social stimulus (a passive audience, a co-working group, a single spectator, etc.), and the activity investigated (simple sensori-motor tasks, cancellation, free association, etc.).

It seemed valuable to attempt an experiment which would test the relative effects of mechanical and social stimuli on the memory process. With this in mind, the present study was undertaken. It sought to incorporate both problems discussed above. We wished to determine whether social and mechanical stimulation influenced the memory process, and whether this in-

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[1] J. E. Evans, The effect of distraction on reaction-time, *Arch. Psychol.*, 5, 1916, (no. 37), 1-106.

[2] E. E. Cassel and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 29, 1918, 129-143.

[3] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, (no. 35), 1-84.

[4] M. A. Tinker, Intelligence in an intelligence test with an auditory distractor, this JOURNAL, 36, 1926, 467-468.

[5] H. B. Hovey, Effects of general distraction on the higher thought processes, *ibid.*, 40, 1928, 585-591.

[6] A. Ford, Attention-automatization: an investigation of the transitional nature of mind, *ibid.*, 41, 1929, 1-32.

[7] F. H. Allport, *Social Psychology*, 1924, Chap. II.

[8] G. Murphy and L. B. Murphy, *Experimental Social Psychology*, 1931, Chap. 8.

fluence was favorable or unfavorable. Stated in terms of the function tested the purpose of this experiment is two-fold. It was undertaken to determine (1) the relative effects of social and mechanical stimulation on learning, and (2) the relative effects of social and mechanical stimulation on retention as measured by relearning after intervals of 1, 2, and 3 days.

THE EFFECT ON LEARNING

Material and apparatus. Three lists of nonsense syllables, seven 3-letter syllables in each list, were used. These syllables were presented one at a time for 1.5 sec. by means of Hull's exposure machine, improved model.[9]

The *Ss* worked in a partitioned booth (5 x 5 x 8 ft.) which contained a small table and a chair. Directly in front of *S* were the machine's exposure windows which fitted snugly into a slit in the wall about 4 ft. above the floor. Above *S*'s head and out of line of vision was a board containing a 150-watt light and an electric buzzer. The light and buzzer were connected to a mercury metronome outside the booth. A curtained window about 3 ft. square was situated at *S*'s left. It was here that the social stimulus in the form of a spectator was introduced by lifting the curtain.

Procedure. Each S memorized lists of nonsense syllables under 3 conditions which we shall designate as mechanical, social, and control.

The control condition was one in which *S* worked alone in a partitioned booth unobserved by any individual and relatively free from extraneous stimulation. This may be a condition some would desire when engaged in serious mental work.

The mechanical condition was one in which *S* worked in the presence of combined visual and auditory stimulation. The visual and auditory stimuli consisted of the stimultaneous flashing of a 150-watt light and the sounding of a buzzer, introduced by means of a mercury metronome that beat 54 times per min.

In the social condition a spectator was introduced while *S* was memorizing. The writer acted as spectator for all *Ss*. He remained entirely passive, making no attempt to offer verbal suggestion, encouragement, or reproach.

When *S* entered the experimental room he was conducted immediately to the partitioned booth and asked to be seated directly in front of the windows of the exposure machine. He was given the following typewritten sheet of instructions.

> This is not an intelligence test; we are not testing your capacity or ability. Our purpose in this investigation is to find some facts about learning. You are, therefore, requested to learn the material which will be presented on the exposure machine in front of you. You are to learn the syllables by anticipating them *aloud*. That is, before the syllable appears at the exposure window you are to pronounce it *out loud*. When the material is presented for the first time you will not be able to anticipate any syllables. But commencing with the second presentation you are expected to anticipate some syllables *out loud*. The material will be presented repeatedly and you are asked to anticipate the syllables *out loud*, until the exposure window is closed. Please do not read the syllable which is exposed; anticipate the syllable before it appears. If you do not understand the instructions, please read them again.

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[9] C. L. Hull, Quantitative aspects of the evolution of concepts, *Psychol. Monog.*, 28, 1920, (no. 123), 1-85.

Prior to actual experimentation, *S* was given a practice list of 7 syllables which he learned to the point of one perfect reproduction by the anticipation method. The results of the practice list were discarded and the material was not used in the experiment. *S* then learned one list of 7 nonsense syllables under control condition, a second under mechanical, and a third under social. The conditions were rotated so that none had any advantage because of temporal position. The lists were also rotated as is demonstrated by the learning program shown in Table I.

TABLE I

Learning Program Showing the Rotation of the Conditions and Lists of Nonsense Syllables

S	Learned		
	First	Second	Third
	List 1	List 2	List 3
1	Control	Mechanical	Social
2	Mechanical	Social	Control
3	Social	Control	Mechanical
	List 2	List 3	List 1
4	Control	Mechanical	Social
5	Mechanical	Social	Control
6	Social	Control	Mechanical
	List 3	List 1	List 2
7	Control	Mechanical	Social
8	Mechanical	Social	Control
9	Social	Control	Mechanical

The use of the above learning program may be explained by citing the first *S* as an example. *S 1* learned one practice list of 7 nonsense syllables to the point of one perfect reproduction. He then memorized List 1 under control conditions, List 2 in the presence of combined visual and auditory stimulation, and List 3 in the presence of a spectator. Each list was memorized first, second, and third by different *S*s and under all conditions of this experiment. The learning program controlled practice and fatigue effects, and balanced any inequality in the difficulty of the lists.

Subjects. Sixty college students recruited from the various classes were used. An attempt was made to obtain a random sampling of the university population. There were 36 men and 24 women.

Results. The results were scored in terms of errors and repetitions. The number of errors made and the repetitions required to reach the point of one perfect reproduction were computed. (An error consisted in failing to anticipate the correct syllable, anticipating the wrong syllable, or anticipating the correct syllable in the wrong order.) Table II shows the average number of errors and repetitions required to learn a list of nonsense syllables to the point of one perfect reproduction under the various conditions.

TABLE II

Average Number of Errors Made and Repetitions Required to Learn a List of 7 Nonsense Syllables by the Anticipation Method under Each Condition

(N = 60)

Condition	Errors	Repetitions
Mechanical	46.37	12.02
Social	41.10	11.27
Control	36.58	9.85

Table II shows that more errors were made in the mechanical situation than in either the control or social. The control situation was the most favorable for learning, inasmuch as fewer errors were made and fewer repetitions required when *S*s worked alone undisturbed by noise or by a spectator. Ranked in order we may say that the control situation was best, the social next best, and mechanical worst. This is true both in terms of errors and repetitions.

The reliability of the differences is shown in Table III.

TABLE III

A Comparison of the Various Conditions Showing Differences and Critical Ratios

(N = 60)

Conditions		Diff.	S.D.$_{\text{diff.}}$	D/σ diff.	Chances in 100
Mech.-Soc.	Errors	5.27	3.19	1.65	95
	Repetitions	0.75	0.79	0.95	83
Mech.-Con.	Errors	9.79	3.21	3.05	100
	Repetitions	2.17	0.77	2.82	99
Soc.-Con.	Errors	4.52	2.72	1.66	95
	Repetitions	1.42	0.74	1.92	97

We may omit, temporarily, a comparison of the social and mechanical conditions, since we are principally interested in contrasting the results of the latter conditions with those of the control.

The table above shows that the difference between social and control conditions is not completely reliable, although the chances are 95 in 100, in terms of errors, and 97 in 100, in terms of repetitions, that the true difference is greater than zero. There can be little doubt however, as to the reliability of the differences between the mechanical and control conditions, since the critical ratio, in terms of errors, is 3.05. In terms of repetitions, the critical ratio between mechanical and control conditions is 2.82, there being 9974 chances in 10,000 of a true difference greater than zero.

Not all differences existing between the experimental conditions (mechanical and social) and the control are large enough to insure complete reliability, yet they are consistently in favor of the control condition.

Such consistent differences are taken as evidence that the control condition was more favorable for learning.

The conclusion just drawn is corroborated by an analysis of individual records. Each *S*'s performance was studied separately to determine which condition was most favorable for him. Each condition was ranked best, second best, and worst, depending upon the number of errors committed. The best condition contained the fewest errors, and the worst the most. Table IV shows the results in terms of errors.[10]

TABLE IV

THE NUMBER OF *S*s WHOSE PERFORMANCE WAS BEST, NEXT BEST, AND WORST IN EACH OF THE 3 CONDITIONS INVESTIGATED, AS MEASURED BY THE NUMBER OF ERRORS

Condition	Best	Next Best	Worst	Equal	N
Control	31	19	8	2	60
Mechanical	13	16	28	3	60
Social	14	20	21	5	60

Of the 60 *S*s who participated in this experiment, 31 or slightly more than 50% did their best learning under control conditions, and 8—approximately 13%—did worst. The performance of 19 *S*s was next best.

Learning under mechanical conditions shows a trend in the opposite direction; namely, fewer *S*s learned best, and more poorest. The performance of 28 *S*s was worst, that of 16 next best, and that of only 13 best under mechanical conditions.

The social situation appears to be more favorable for learning than the mechanical, and less favorable than the control. Fourteen *S*s learned best, 20 next best, and 21 worst under social conditions. Of the 5 *S*s who did equally well in two conditions, 3 were matched with the mechanical, and 2 with the control.

Analysis of the data in terms of group averages (Tables II and III) and individual records (Table IV) shows that learning nonsense syllables was more efficient when the *S*s worked alone undisturbed by a spectator or noises and lights. These results do not agree entirely with the findings of Ford and Morgan. This may be due to the fact that the learning period in the experimental conditions was too short to permit the *S*s to become accustomed to the external stimuli. It is possible, then, that our results measure the initial effects of external stimuli. A second factor of importance is that the nature of the task set before *S* differed in our experiment. Our *S*s learned verbal material by the anticipation method. Ford's subjects added silently single digits from a row of mixed letters, and wrote the answers, and Morgan's *Ss* translated codes by pressing correct keys.

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[10] The data were analyzed also in terms of repetitions, but this analysis is not presented here, since the results are substantially the same as for errors. For the same reason the data in terms of repetitions were omitted from Tables V and VI.

THE EFFECT ON RETENTION

Procedure. To determine the effect on retention of material learned under the various conditions of this experiment, the *Ss* were divided into 3 groups and asked to return at intervals of 1, 2, and 3 days. All relearning occurred under the same condition as the original learning. Retention was measured by the Ebbinghaus Savings Method.

Results. In Table V the results of each group may be considered separately. Group 1 retained more of the material learned in the social and mechanical conditions than of that learned in the control condition. This is indicated by the greater average percentage saving during relearning. The various conditions may be ranked for retention value, first rank being assigned to the condition yielding the greatest average percentage saved during relearning. Such a ranking indicates that the social condition was first, the mechanical second, and the control third.[11]

TABLE V

THE AVERAGE PERCENTAGE SAVED DURING RELEARNING IN TERMS OF ERRORS

Condition	Group 1 After 1 day (N=20)	Group 2 After 2 days (N=20)	Group 3 After 3 days (N=20)
Mechanical	43.82	54.67	51.65
Social	46.39	46.59	53.06
Control	36.59	40.43	37.57

This ranking is slightly altered by Group 2, the mechanical being first, the social second, and the control third. Of greater significance, however, is the fact that material learned under the mechanical or the social conditions is again better retained than that learned under control conditions. The results of Group 3 are similar to those of Group 1. The social condition was first, the mechanical second, and the control third.

The reliabilities of the differences between the various conditions in retention tests are presented in Table VI.

As in the original learning (see Table III), the differences are not large enough to insure complete reliability. If the material had been relearned under more favorable conditions, as in our control condition, the differences in retention between the experimental conditions and the control would doubtless have been greater.[12] Yet differences are evident, and in each case there is greater percentage gain for material learned in the experimental conditions. This points to the conclusion that material learned in the presence of a spectator or in the presence of noise and light is retained better than that learned in quiet conditions.

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[11] Some retroactive inhibition doubtless affected the relearning, but it is considered a negligible factor since the retroactive inhibition affected all conditions approximately alike.

Also it is important to observe that the differences in percentage gained between the experimental and control conditions are greater after 3 days than after 1 day. This is seen by comparing Groups 1 and 3. For Group 3 the differences are: social minus control = 15.49%, and mechanical minus control = 14.08%. For Group 1 the differences are: social minus control = 9.8%, and mechanical minus control = 7.23%. Two possible explanations for the divergences in the two groups suggest themselves: (1) the fact that different *S*s were used in each group, so that the differences may be due to a sampling error. Such an explanation, however, does not seem plausible, since both groups showed approximately the same amount of saving in relearning under the control condition (Group 1 saved 36.59% and Group 3 saved 37.57%, a difference of less than 1%, which is negligible), indicating that there is some basis for comparing the two groups. (2) A second explanation is based on the fact that the influence of learning material under mechanical and social conditions becomes evident only after a lapse of time. Such influence is slightly evident in relearning after one day, but markedly noticeable after a lapse of 3 days. It is possible that some factors operate during learning in the experimental conditions which facilitate retention over a longer period. The better retention of material

TABLE VI

A COMPARISON OF THE VARIOUS CONDITIONS SHOWING DIFFERENCES AND CRITICAL RATIOS IN RETENTION TESTS

(In terms of errors)

Lapsed time	Conditions	Diff.	$\text{S.D.}_{\text{diff}}$	D/σ_{diff}	Chances in 100
1 day	Soc.-Mech.	2.57	6.86	.37	64
	Soc.-Con.	9.80	8.71	1.13	86
	Mech.-Con.	7.23	8.23	.88	81
2 days	Mech.-Soc.	8.08	6.82	1.19	88
	Mech.-Con.	14.24	8.88	1.60	94
	Soc.-Con.	6.16	7.49	.82	79
3 days	Soc.-Mech.	1.41	6.30	.22	59
	Soc.-Con.	15.49	6.32	2.45	99
	Mech.-Con.	14.08	6.66	2.11	98

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[12] J. Pessin, The effect of similar and dissimilar conditions upon learning and relearning, *J. Exper. Psychol.*, 15, 1932, 427-435.

learned in the social and mechanical conditions may be attributed in part to the fact that slightly more repetitions were required in the original learning under these conditions. That retention is dependent upon the number of repetitions in the original learning has been demonstrated by Ebbinghaus,[13] Luh,[14] and others. The number of repetitions in the original learning does not, however, seem entirely adequate to explain the superiority in retention of the mechanical and social conditions. Although the mechanical condition required slightly more repetitions in the original learning than the social, the latter shows slightly superior retention after 3 days (see Tables II and V). It is, therefore, believed that other factors plus increased repetitions in the original learning are responsible for better retention of material learned in the experimental conditions. Perhaps the greater expenditure of energy, as evidenced by louder responses in the mechanical condition and shuffling about in the social, exerted in learning the material in the experimental conditions resulted in better fixation and consequently better retention.

SUMMARY AND CONCLUSIONS

The relative effects of mechanical and social conditions on learning and retention of nonsense syllables were studied. The results obtained seem to justify the following conclusions.

(1) That learning is more efficient under relatively quiet conditions than under conditions of mechanical or social stimulation.

(2) That nonsense syllables learned under mechanical and social conditions are better retained than similar material learned under control conditions. Also, that the percentage retained is greater after a period of three days than after one day.

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[13] H. Ebbinghaus, *Memory*, Eng. Trans. by H. A. Ruger and C. E. Bussenius, 1913.

[14] C. W. Luh, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.

INDIVIDUAL DIFFERENCES IN THE REPRODUCTION OF TEMPORAL INTERVALS

By HERBERT WOODROW, University of Illinois

An attempt was recently made by the writer to determine by the method of reproduction what is known as the temporal indifference interval.[1] According to the existing experimental literature on the reproduction of empty intervals bounded by instantaneous sounds, when stimulus-intervals shorter than a certain length are reproduced, the reproduction-intervals should be longer than the standard or stimulus-intervals, or, in other words, the average error in the reproductions should be positive. On the other hand, when the stimulus-intervals are long, the reproductions should be shorter, that is, should show a negative average error. The indifference interval—the length of interval at which the average errors of reproduction change from positive to negative—has varied enormously with the investigator. The study made by the writer, though extensive and thorough and conducted throughout under what appeared to be a careful and adequate control of conditions, instead of resulting in an accurate determination of the indifference-interval, as had been hoped, led to the conclusion that every length of interval, no matter whether long or short, was overestimated (made too long in reproduction) by some *S*s and underestimated by others. It is true that rather definite results were obtained as regards the *variability* in the reproductions with variation in the length of the standard or stimulus-interval, results which revealed, in particular, a tendency towards a minimum relative variability at 0.6 sec.; but as regards the *sign* and magnitude of the *S*s' average errors, the so called constant errors, there was so little uniformity that it appeared unjustifiable to average the results obtained with the different *S*s, as is customary, in order to determine the prevailing general tendency.

This outcome was unsatisfactory, for at least two reasons. In the first place, the absence of any general trend in the constant errors is hard to reconcile with the obtained rather definite and regular trend in the variable errors; and secondly, it leaves unsettled the cause of the differences

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* Accepted for publication February 15, 1933.

[1] H. Woodrow, The reproduction of temporal intervals, *J. Exper. Psychol.*, 13, 1930, 473-499.

between individuals as also that of irregular changes noted in the constant errors of the same individual with different lengths of the stimulus-interval.

There are obviously a number of possibilities which should be further investigated. One of these is the possibility that groups of *S*s large enough to approximate adequate samples of the population might reveal uniformities not obvious in a group of only eight; another is the possibility that the effects of reproductions of one interval upon reproductions of another interval, whether such effects be those of practice, habituation, or contrast, carry over from one day to the next, but in different ways with different individuals.[2]

The present paper deals with two other possible influences upon the constant errors. The first of these is the production or non-production of a sound simultaneously with the taps by means of which *S* reproduces the stimulus-interval. The second factor, found to be of enormous importance, is the effect exerted by differences in the mental attitude of the *S*s, by which is meant differences in the way they went about their task of reproducing the intervals. This was studied by determining the effects of two sets of instructions which differed widely as regards the way in which the *S* was asked to attend to and to reproduce the intervals but which remained identical in calling upon the *S* to make his task that of accurate reproduction.

Apparatus and procedure. In the writer's former investigation of the reproduction of intervals, the apparatus was so arranged that, by means of certain electrical connections, at each tap of the *S* (made upon a key noiseless in itself) there occurred a sound exactly like the sounds used to bound the stimulus-interval. It seemed quite possible that the errors of reproduction would present a different picture if the *S*'s taps were unaccompanied by any noticeable sound. The apparatus and procedure, except for a slight change needed to render the taps noiseless, were the same as in the former investigation and therefore do not need detailed description.[3] The stimulus-intervals were bounded by instantaneous sounds. Each interval was preceded by a warning signal, the tone of a buzzer of 800 d.v. per sec., given approximately 2 sec. before the initial sound of the stimulus-interval and sounding for approximately 0.15 sec. Only one stimulus-interval was used on any one day. This interval was presented 50 times and reproduced once after each presentation. The time elapsing from the end of one reproduction to the warning signal of the next presentation of the stimulus-interval was usually about 12 sec., but varied over a range of from 12–16 sec. The stimulus-intervals varied from 0.4–6.0 sec. and were controlled, if not over 1.0 sec., by a Bergstrom large model chronoscope, and if 2.0 sec. or over, by a Bowditch-Baltzar contact-clock. The reproduction intervals were measured by means of a Hipp's chronoscope controlled before and after each sitting for the particular stimulus-interval employed. The intervals used were changed from sitting to sitting, in regular order, from the shortest, at the first sitting, to the longest and then from the longest back, at the last sitting, to the shortest.

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[2] A study which obviates both these possibilities by the use of nearly 300 *S*s divided into groups each of which groups is given but one interval, is now nearing completion. This study forms a part of a larger study under way in the Illinois laboratory for obtaining dependable, verifiable information concerning the fundamental facts about the errors in the comparison, absolute judgment, and reproduction of empty and filled temporal intervals.

[3] Cf. *op. cit.,* 476-484. In order to render the taps noiseless, key K5 was kept closed and relay R5 not used.

Instructions. The instructions were as follows.

> You will first hear a "ready" signal; namely, the sound of a buzzer, and shortly after this signal, you will hear two clicks, separated by an interval of time. As soon as you have heard the two clicks, you are to reproduce the interval between them by two taps upon the key. The same interval will be given throughout the sitting. Try to inhibit any tendency to count and avoid making any movements of a rhythmical nature with your hands, feet, or vocal organs. Pay as little attention as possible to your breathing. Reproduce each interval independently, that is, no matter how long or how short it seems, reproduce it as it seems.

After *S* had read the instructions, he was asked to repeat them to *E*. Any point forgotten or misunderstood was gone over again. A uniform manner of tapping, with the fore and middle fingers pressing down together upon a Scripture noiseless key, was demonstrated. The fingers were never lifted entirely off the key, so that when the key was pressed down there was no noise due to the impact of the fingers upon the key. The tap was experienced simply through the skin and the muscles. The *S*s were asked to tap a number of times until they became accustomed to the key and the tapping movement, but were not given practice in the reproduction of the interval. After instructing *S*, *E* retired to a room, two doors removed, which contained the Hipp's chronoscope and all of the control-apparatus. The *S*s were advanced students in psychology. They were all unaware of the type of errors they might be expected to make. They were, of course, never informed concerning either the length of the stimulus-interval or their errors.

Results. The results obtained are summarized in Table I. Each constant error (C.E.) which appears in the table is the difference between the mean length of the stimulus-interval, as measured 10 times before and 10 times after the sitting, and the mean length of the *S*'s 50 reproductions. The variation in the mean length of the stimulus-interval (usually only several sigma) was negligible compared with the variation in the length of the reproductions, so that in place of the P.E. of each constant error, which in reality is the P.E. of a difference between two means, there has been used simply the P.E. of the reproductions. As in the case of reproductions by means of sound-producing taps, there is no regularity in the sign of the constant errors. In the entire table there are 21 positive C.E. and 21 negative ones. Both positive and negative errors occur with every one of the stimulus-intervals and these two kinds of errors are apparently distributed quite in a chance order. This result agrees perfectly with that previously obtained in the case of reproductions by sound-producing taps.

TABLE I

Constant Errors of Silent Reproductions of Empty Intervals

All intervals, P.E., and C.E. are given in sigma. N for each cell is 50; total N is 3,500. Below each C.E. (mean or average error), is given its P.E., preceded by a ± sign. Each given P.E. is the P.E. of the distribution, which is 7.1 times as large as the corresponding $\text{P.E.}_{\text{C.E.}}$

Interval	Ss						Number of Values	
	Pn	*Ws*	*Fg*	*Sp*	*Bn*	*As*	+	−
Ascending Series								
400	−17 ±20	−40 ±25	−37 ±21	+29 ±20	+16 ±18	+16 ±22	3	3
600	+15 ±23	−18 ±25	−31 ±22	+81 ±45	+39 ±22	−4 ±22	3	3
800	+21 ±37	−3 ±40	−104 ±41	+40 ±39	+4 ±38	−55 ±33	3	3
1000	−60 ±52	+150 ±102	−80 ±51	+39 ±47	−46 ±70	−18 ±68	2	4
2000	−19 ±149	+107 ±334	−19 ±179	+96 ±174	−255 ±113	+137 ±221	3	3
4000	+673 ±390	−829 ±385	−869 ±375	−412 ±353	−1019 ±196	+274 ±501	2	4
6000	+1534 ±687	+269 ±779	+263 ±723	−762 ±794	−1237 ±324	+71 ±630	4	2
Descending Series								
6000	+1486 ±699	+972 ±909	−183 ±822	−187 ±747	−193 ±397	+33 ±682	3	3
4000	+1221 ±541	+379 ±423	−418 ±432	+58 ±437	−827 ±254	−16 ±412	3	3
2000	+43 ±185	−41 ±199	+17 ±155	−49 ±145	−2 ±97	−83 ±144	2	4
1000	+94 ±57	−45 ±92	−29 ±62	−34 ±40	+11 ±60	−63 ±66	2	4
800	+34 ±42	−62 ±37	−69 ±48	−102 ±41	+26 ±35	+3 ±44	3	3
600	−50 ±35	+32 ±30	−63 ±22	−31 ±35	+43 ±27	+1 ±29	3	3
400	−19 ±24	−32 ±16	−9 ±23	−33 ±30	+22 ±29	−14 ±17	1	5

The present results with silent reproductions also confirm the former ones, with reproductions by means of sound-producing taps, as regards the relation between variability and the length of the interval. In spite of the irregular shifts in the C.E., there is a marked and fairly regular relation between the variability, measured by the P.E., and the length of the interval. In general, individual differences in the P.E. are much less marked than those in the C.E., and there appears to be no good reason why the P.E. of different individuals should not be averaged, particularly as the general tendency revealed when they are so treated is substantially the same in the present study as in the preceding one, with 8 *S*s ,who reproduced the intervals by sound-producing taps. In Table II are given in the columns headed Av. $\text{P.E.}_{\text{dis.}}$ the averages of the absolute individual P.E. (shown in Table I), while in the columns headed Percent are given the relative variabilities, *i.e.* the averages of the individual P.E. expressed as percentages of the lengths of the stimulus-intervals. The results given in this table demonstrate that relative variability of reproductions is at a minimum when the interval reproduced has a length of 0.6 sec., and that this relative variability increases as the interval increases or decreases from this most favorable length. Of course the validity of this generalization is limited to the intervals studied. In particular, since the 0.6-sec. interval was the only one between 0.4 and 0.8 sec., it is impossible to determine whether the minimum relative variability occurs exactly at 0.6 sec., or at a slightly longer or shorter interval.

TABLE II

THE RELATION BETWEEN VARIABILITY IN REPRODUCTIONS AND LENGTH OF STIMULUS-INTERVAL

Interval	Ascending series		Descending series		Av. percent	
	Av. $\text{P.E.}_{\text{dis.}}$	Percent	Av. $\text{P.E.}_{\text{dis.}}$	Percent	this study	earlier study
400	20.9	5.2	23.1	5.8	5.5	5.7
600	25.3	4.2	29.6	4.8	4.5	5.3
800	37.9	4.7	41.0	5.1	4.9	5.7
1000	65.0	6.5	62.8	6.3	6.4	5.8
2000	194.9	9.8	154.1	7.7	8.8	6.8
4000	366.7	9.2	416.3	10.4	9.8	11.1
6000	655.1	10.9	709.2	11.8	11.4	11.8

Since all the relevant *external* conditions were the same for all our *S*s, the conclusion appears inescapable that the great variation between *S*s was due to variation in the *internal* operations of the *S*s—*i.e.* in the ways in which they went about their task. Significant change from time to time on the part of a given individual under supposedly constant external conditions is probably the rule in psychological experimentation. This fluctuation in performance is decidedly pronounced when results from sittings scattered over a number of days are considered.[4] Consequently a study was undertaken of the effect upon the errors of reproduction of the processes occurring within the *S*s, a study of what may be termed the effect of attitude upon performance. The only way in which it was possible readily to obtain information concerning the attitudes of the *S*s was to study their introspections.

Like most investigators, the writer followed psychological tradition and asked for frequent introspective reports, and so by the end of the experiments had accumulated a large sheaf of manuscripts written by the *S*s. It is something of a problem just what to do with such material, when obtained as *accessory* to the study of the *S*'s performances. How often, as a sort of appendix to a quantitative study of some aspect of performance, we find tacked on several pages of introspective material, material which may constitute only a small selection, made by *E,* from a much larger mass of reports. It is difficult to understand how these introspective reports can be regarded as relevant to the accompanying behavior, unless it is possible to establish a correlation between the reports and the behavior. Relationship has to be *proved,* mathematically—here, the same as elsewhere —and not simply assumed. We know very little about such relationships, and yet many a psychological discussion may be found which has as its basis an assumed perfect correlation between the characteristics of introspectively reportable processes and those of overt behavior.

In this work on the reproduction of intervals, *E* carefully scanned the introspective reports of the *S*s but could discover only faint suggestions of a correlation between any features of the reports and the type of errors shown by the *S*s' performances. In such a situation, which is not an uncommon one, the chief use that can be made of the introspections is to regard them simply as affording suggestions for varied types of instructions, which, when formulated, must be tried out to determine whether there is any relationship between the different types of instructions and the ensuing behavior. Such a procedure, constituting what may be termed the method of differential instruction, was adopted. At first it led to no positive conclusions. Either the *S*s did not all follow the instructions or else the instructions required the adoption of attitudes that were irrelevant to the errors. Finally, and only in the case of one pair of instructions, was there obtained a really significant change in performance. According to one set of instructions, *S* was to listen to the sounds bounding both the stimulus-interval and its reproduction interval as a pair of sounds and avoid strain; while according to the other set, he was to listen passively to the stimulus-interval but in reproducing he was to put his attention on the strain sensations which filled the interval between the bounding sounds. With change from one set of instructions to the other, there occurred an enormous change in the errors, a change which was not only of great magnitude but which was uniform in its direction. The 12 *S*s were each tested with both types of instructions and with two stimulus-intervals, 0.6 and 4.0 sec.

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[4] Woodrow, Quotidian variability, *Psychol. Rev.,* 39, 1932, 245-256.

The first set of instructions was as follows.

Auditory—unanalytical—attention on the sounds.

(1) Stimulus. Attention solely on the sounds (in case of both stimulus and response). Listen to them as 'out there' somewhere, *i.e.* as going on outside of you and some distance away. A *pair* of sounds. Avoid any effort or strain; take it easy.

(2) Response. In reproducing the interval pay no attention to your finger movements—try to make them so automatically that you will hardly be aware of them. You simply reproduce the pair of *sounds*. Of course in reproducing the stimulus pair of sounds you are to make the interval between your two sounds exactly the same as that between the stimulus-pair; but do not analyze the whole into sounds and interval; with your attention on the sounds, make the whole "gestalt" *sound* just like the stimulus-gestalt. Do not look at the key you tap on: forget about the taps. Avoid any muscular tension, in hands, fingers, vocal or breathing organs. Avoid any humming or motor accompaniments. Forget your *self*.

The second set of instructions was as follows.

Strain—interval separated from the bounding stimuli—attend to the interval as such.

(1) Stimulus. Attention to the *interval* between the sounds, rather than the sounds.

(2) Response. Motor, *i.e.* get your attention immediately on your fingers and the two pushes you are to make on the key. Put your fingers and your body, breathing and vocal muscles, on a strain while reproducing the interval. Say hm . . . m to yourself and keep your attention on these various movements. Be sure to tense your fingers. Try to keep your mind so occupied with attention to your muscle experiences that you are scarcely aware of the sounds resulting from your taps. Be sure to make the interval between the two pressings of the key exactly equal to the stimulus-interval.

Perhaps better terms than 'auditory' and 'strain' for distinguishing between these two sets of instructions would be the terms 'extrovert' and 'introvert.' Such designations would also have the merit of preventing these instructions from being confused with the 'sensory' and 'motor' instructions sometimes used in work on reaction-times.

On any one day only one interval and one set of instructions were used, and as a rule several days elapsed between any two sittings. The trials with one interval were completed before going to the other interval, but the order in which the two instructions and the two intervals followed each other varied among all the possible orders.

E of course had no way to make sure that the rather complex instructions were carried out. The *S*s, however, were either advanced students or members of the teaching staff in the department of psychology and intelligent coöperation could be assumed. All of the 12 *Ss* had had previous experience in introspection. They all stated that they understood the instructions and that they thought they had carried them out. Their reports indicate that, in the words of *Cr*, "factors mentioned in the instructions dominated the set;" but additional factors not mentioned in the instructions, the influence of which, if any, is unknown, also came into play. Many of these additional factors are probably inconsequential. On the other hand it is quite possible that some of them greatly modified the effect which the instructions would otherwise have exerted. One such factor, for example, may be that indicated by reference on the part of a few of the *Ss* to patterns and groupings, to swings of a pendulum followed by the eyes, and the use of the word rhythm, usually in a vague manner. Such references are well illustrated in the reports of *Em*, extracts from which are as follows.

Auditory instructions, 0.6 sec. "The two sounds were similar to time-beats in music and their reproduction a matter of continuing the rhythm. The whole four sounds formed one phrase, including the time between the two pairs of sounds."

Strain instructions, 0.6 sec. "Concentrating on the finger-movement, the tick-tocks were not so clear, and a definite finger pattern became evident. In reproducing the interval I was conscious of the same experience as in practicing five-finger exercises upon the piano—a definite pattern of down and up movements in the fore-finger."

Auditory instructions, 4.0 sec. "These longer intervals were not so rhythmical as the shorter ones and did not form a pattern of stimulus-wait-response."

Strain instructions, 4.0. sec. "The intervals tended to form a pattern of stimulus and response, but not so clearly as with the shorter intervals."

These introspections are in accord with the well-known fact that widely separated sounds do not readily enter into a rhythmical grouping, but their bearing on the sign of the C.E. at any given interval, is an exceedingly difficult and entirely open question.

The *Ss*' reports under these instructions afford positive information on several interesting matters. One of these is the question what strains were used in complying with the "strain" instructions. The strains reported varied with the *S*, but were most frequently derived from the fingers, hand or arm, or from the vocal or respiratory organs. Other parts of the body mentioned as the seat of strain were "the back of the throat," "the vocal cords," "the wrist," "muscles involved in gritting the teeth," the "whole body," and the "viscera."

Em wrote that "although there was strain throughout the whole body, it was particularly noticeable in breathing and the right arm;" at another time, "the strain was localized in the fore-finger rather than throughout the arm." Two *Ss* mentioned an increase in the intensity of the strain towards the end of the interval; 4 *Ss* said the strain was stronger or more vivid during the response interval than during the stimulus-interval, while none reported the reverse to be true. Two *Ss* said that the motor pattern was not the same during the stimulus and the response intervals. Thus *Cr* reported: "I exhale in one and inhale in the other." *Dg* (4.0 sec. interval, strain instructions) referred to extreme nervousness of his fingers and stated that "just before the time to give the second sound my fingers shook considerably until all at once they became relaxed and I pushed the key." Several of the *Ss* reported that it was harder to fill the short interval with strain than to do so in the case of the long one. *Cr* (0.6. sec. interval, strain instructions) wrote: "I often failed to notice any organic quality until both the [stimulus] clicks were over, so I tried to notice my breathing while the clicks occurred and then tried to reproduce in terms of breathing. Easiest was to say click-click and then reproduce the same muscular pattern. That seemed the 'natural' way for me. Unconsciously I seem to beat time somewhere in the body of these quick clicks and to reproduce that pattern." Similarly, *Mr* found the strain instructions harder with the short interval. He wrote that "the short interval does not have the 'timeyness' of the longer one. There is, however, a short going-on, although I can't say what it is that goes on. The body is on a strain, but very little happens to that strain. There is strain and that is all, but in the longer interval there is quite a pattern to the strain—increase in intensity and pain that means fatigue. When the going-on is there, there is a downward pressure which is neither bright nor dull nor heavy nor light, localized where I believe my stomach is. There is very little of this down-pressure with this short interval." *Rk* described his experience of the 0.6-sec. interval, under strain instructions, in the following vivid manner: "Between the clicks there was a dull thud as though a man were making a short jump in between two jumps."

From these introspections it appears that an attempt to attend to the interval as separate from the sounds leads only to the observation of strain. *Mr,* however, referred to a quality designated by the neologism "timeyness"; and several *Ss* referred to experiences of waiting and of expectation without observed strain.

Even with auditory instructions, the *Ss* sometimes mentioned strain or motor experiences, but in this case, they tried not to attend to them.

Thus *Cr* wrote as follows, after reproducing the 4.0-sec. interval with auditory instructions: "Hard to keep attention off the organic factors. I tried out various things such as taking one posture in listening and another in reproducing or changing my breathing voluntarily, so as to make it difficult to be guided by organic factors. After a while, however, I took it more casually and in many cases didn't notice at all the organic factor. The two sounds were the dominant factor." On the other hand such reports as the following were made concerning the effect of the auditory instructions. *Em,* (0.6 sec.) wrote that "there was no strain of any sort, nor any effort of listening."

One other point is perhaps worthy of mention; namely, the involuntary character of the *S*'s control of the length of the reproduction interval. Scores of reports on this matter, distributed throughout all parts of this work on the reproduction of intervals, have been obtained. The following are some illustrations.

Rb (0.6. sec., strain instructions): "My arm seems to give the second tap of its own accord. I set off the first tap, but the second takes care of itself."

Dg (4.0 sec., auditory): "There was a feeling of a growing nearness of action, then it was as if something said 'now' and I pushed the key the second time."

Cr (4.0 sec., auditory): "The second tap seemed to come inevitably at a certain point."

Em (0.6. sec., auditory): "The intervals were more or less unconsciously reproduced."

Of course it might be said that we have no sense of initiating most of our movements; they simply take place. However that may be, many of the *S*s seemed impressed by the fact that once they set themselves to reproduce an interval, the reproduction took place in a satisfactory manner without their knowing why or how. What particularly impressed them was that while the reproduction-interval seemed to be initiated at an intended instant, the completion of the interval took care of itself.

The effect of the two sets of instructions upon the errors of reproduction are summarized in Table III. It will be observed that the reproductions with strain instructions averaged much longer than those with auditory instruc-

TABLE III

THE EFFECT OF DIFFERENCE IN INSTRUCTION UPON THE CONSTANT ERROR OF REPRODUCTION

Intervals, C.E., and P.E. are given in sigma. The P.E. given is the P.E. of the distribution (7.1 times the P.E.$_{\text{C.E.}}$). N for each C.E. is 50; total N is 2,400.

Stimulus-Interval	S	Instructions				Difference in C.E. (S−A)	D/P.E.$_{\text{diff.}}$
		Auditory (A)		Strain (S)			
		C.E.	P.E.	C.E.	P.E.		
600	*Cr*	−2.3	27	+71.4	64	+73.7	+7.5
	Mr	−30.6	25	+15.8	24	+46.4	+9.4
	Rk	−22.8	32	+55.3	78	+78.1	+6.6
	Mn	−36.6	41	+21.2	32	+57.8	+7.6
	Ln	+0.4	33	−10.0	24	−10.4	−1.8
	Em	−14.4	18	+18.9	26	+33.3	+8.9
	Kp	−15.6	35	+36.0	50	+51.6	+7.3
	Ds	−94.2	77	−20.1	58	+74.1	+5.1
	Dg	−12.2	30	+9.5	26	+21.7	+3.8
	Fl	−7.0	34	+51.0	35	+58.0	+8.3
	Ke	−20.4	25	+24.3	30	+44.7	+6.2
	Ht	+17.7	28	+33.1	39	+15.4	+3.6
	Av.	−19.8	33.8	+28.3	39.5	+48.1	+6.0
4000	*Cr*	−129.8	429	+373.7	287	+503.5	+6.9
	Mr	−1787.6	213	−196.1	527	+1591.5	+19.7
	Rk	−515.7	444	+246.7	479	+762.4	+8.2
	Mn	−109.0	297	+1155.9	674	+1264.9	+12.1
	Ln	+49.0	406	+338.3	294	+289.3	+4.0
	Em	−766.5	198	−243.1	279	+523.4	+10.0
	Kp	+117.6	479	+408.0	307	+290.4	+3.6
	Ds	−472.6	696	−135.6	493	+337.0	+2.8
	Dg	−809.1	327	−512.1	296	+297.0	+4.8
	Fl	−619.1	398	+461.8	409	+1080.9	+13.4
	Ke	−166.1	327	+689.2	394	+855.3	+11.5
	Ht	−290.0	421	+475.5	478	+765.5	+8.2
	Av.	−472.1	386.3	+274.0	410.0	+746.1	+8.8

tions. In the case of the 0.6-sec. interval, the strain reproductions averaged 48.1 σ longer than the auditory reproductions, and in the case of the 4.0-sec. interval they averaged no less than 746.1 σ longer. It is true that the strain instructions did not always cause 'overestimation' nor the auditory instructions 'underestimation.' In 23 out of the 24 cases, however (the only exception being *Ln* with the 0.6-sec. standard), the strain reproductions were longer than the auditory reproductions—shown in the table by the fact that the values in the column headed 'Difference in C.E. (S-A)' were positive in all cases but one. Furthermore, the difference in the length of the reproduction was, in the case of the great majority of the *S*s, statistically significant, averaging for the individual *S*s over 6 times the P.E. of the difference in the case of the 0.6-sec. interval and over 8 times the P.E. of the difference in the case of the 4.0-sec. interval. If the results for all *S*s were pooled into one distribution, in the case of each interval the difference between the two averages (in terms of the $\text{P.E.}_{\text{diff.}}$) would be even greater.

This result is, of course, only a beginning. The two sets of instructions were admittedly complex. To determine what were the really effective elements in them would require much further work. Moreover, from the mass of introspections obtained, it is obvious that the varieties and subvarieties of attitudes which *S*s may take in even such a simple experiment as the reproduction of the interval between two clicks is bewildering. Probably fluctuation from one to another of many of these attitudes has no bearing upon the nature of the errors of reproduction. On the other hand, it is not contended that the difference in attitude produced by the difference between the instructions described above is the sole difference in the attitude between *S*s who 'overestimate' and those who 'underestimate.' Indeed, it seems certain that this could not be the case, since the one set of instructions did not always cause overestimation and the other underestimation. Moreover, if the average errors (C.E.) of a large group of *S*s are plotted, the distribution will be smooth, with a considerable frequency (though not usually the modal frequency) at zero error. What the present experiment proves is simply that variation in attitudes is extremely influential and may be sufficient in some cases to determine whether an *S*'s reproductions will show great overestimation or great underestimation. It affords experimental support of the conclusion that in the reproduction of empty intervals the nature of the errors depends in large part upon the attitude of the *S*s toward their task. To determine to what extent the nature of these errors depends also upon the length of the interval requires an investigation planned in other fashion than those hitherto made.

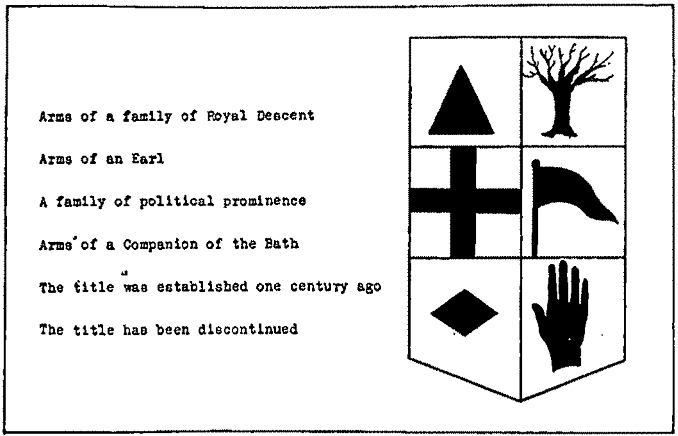
AN OBJECTIVE EXPERIMENT ON REASONING

By S. N. F. CHANT, University of Toronto

The purpose of this investigation was to analyze by a method of continuous observation some of the characteristics of the reasoning process.

Method and procedure. The materials were eleven 5 x 8 cards on each of which was drawn a fictitious coat of arms. Each coat of arms was composed of 6 separate drawings. Beside these were printed 6 items of information concerning the family to which the coat of arms supposedly belonged, and each drawing in any particular coat of arms stood for one of the items of family information printed on the same card. No rules of heraldry were followed. In all there were 14 drawings which were repeated in different groupings, along with their respective items of family information, throughout the series of 11 cards. The complete series, with Card 1 shown as a sample and the remainder with the drawings indicated by name instead of diagrammatically, was as follows:

Card 1



Arms of a family of Royal Descent

Arms of an Earl

A family of political prominence

Arms of a Companion of the Bath

The title was established one century ago

The title has been discontinued

FIG. I. CARD 1.
(A sample of the material used in the experiment.)

Card 2

Sword	2 Triangles	Arms of a family of Royal Descent.
		Arms of a family of Scotch origin.
Tree	Bar	Arms of a Baron.
		A family of scientific prominence.
Flag	Snake	The title was established 2 centuries ago.
		The title has been discontinued.

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Card 3

Snake	Flag	Arms of a family of Scotch origin. Arms of a family of noble descent.
Cross	Rose	Arms of a Baron. A family of political prominence.
3 Triangles	Sword	The title was established 3 centuries ago. The title has been discontinued.

Card 4

Sword	Cross	A family of noble descent. Arms of a Baron.
Anchor	Flag	A family of seamen. A family of political prominence.
Rose	4 Triangles	The title was established 4 centuries ago. The title has been discontinued.

Card 5

Fish	4 Triangles	Arms of a family of Scotch origin. Arms of a family of noble descent.
Snake	Cross	Arms of a Viscount. A family of political prominence.
Rose	Flag	The title was established 4 centuries ago. The title has been discontinued.

Card 6

Snake	Hand	Arms of a family of Scotch origin. Arms of an Earl.
Flag	Diamond	A family of commercial prominence. A Companion of the Bath.
Elephant	Triangle	The title was established one century ago. The title has been discontinued.

Card 7

Cross	Snake	Arms of a family of Scotch origin. A family of noble descent.
Hand	4 Triangles	Arms of an Earl. A family of commercial prominence.
Flag	Rose	The title was established 4 centuries ago. The title has been discontinued.

Card 8

Cross	Sword	Arms of a family of noble descent. Arms of a Baron.
Flag	2 Triangles	A family of political prominence. A family of literary prominence.
Rose	Bell	The title was established 2 centuries ago. The title has been discontinued.

Card 9

Rose	Hand	Arms of a family of noble descent. Arms of an Earl.
Elephant	Anchor	A family of commercial prominence. A family of seamen.
Triangle	Flag	The title was established 1 century ago. The title has been discontinued.

Card 10

Elephant	Cross	Arms of a family of noble descent. Arms of a Baron.

Rose	Flag	A family of political prominence.
		A family of commercial prominence.
Sword	3 Triangles	The title was established 3 centuries ago.
		The title has been discontinued.
Card 11		
Bell	Flag	Arms of a Baron.
		Arms of a Companion of the Bath.
Sword	Diamond	A family of literary prominence.
		A family of seamen.
Anchor	3 Triangles	The title was established 3 centuries ago.
		The title has been discontinued.

The *Ss*, 69 in number, were all undergraduates who had completed an introductory course in psychology. Each *S* was observed individually while he worked at the problem for one half hour. The 11 cards, an answer sheet, and the following printed instructions were provided each *S*.

> On each of these cards is a coat of arms composed of 6 separate drawings. On each card are also 6 items of information concerning the family to which the coat of arms belongs. Each drawing on any particular coat of arms stands for one of the items of family information given on the same card. You are to determine which item of information each drawing stands for. You may use the cards in any manner you wish.
>
> On the record sheet in column 1, name or make a rough sketch of the drawing from the coat of arms. In column 2 record opposite each drawing the particular item of family information it stands for. In column 3 record at the same time as you answer your reason for giving your answer. You may correct any answer at any time by marking it "w," and recording the correct answer.

Two *Ss* out of 69 misunderstood the instructions and their results are not included.

A continuous record in 15-sec. intervals was made of the *S*'s behavior while working at the problem. This record was made on a sheet that was ruled into 120 columns to serve as time divisions, and into 7 horizontal divisions to provide sections for recording 7 characteristic types of behavior in this setting, which were determined by preliminary experimentation. The 7 possible records were as follows: Recording by number (1) any cards examined individually; (2) any two or more cards compared; (3) any cards examined in serial order; (4) any cards and answers compared; (5) the answers as they were written; and recording (6) any apparently random examination of the cards; and (7) any apparent periods of inaction.

The timing of those records was arranged as follows. A rack and pinion, operated by the ratchet wheel of the line-spacer of a typewriter, moved a vertical metal strip across the record sheet the distance of 1 time division whenever the spacing lever was pressed. In *E*'s line of regard when observing the *S* was an Eastman Timer. The record was made by placing a number or check in the appropriate behavior division of the time column, as indicated by the metal strip. Every 15 sec., as shown by the timer, the spacing lever was pressed and the metal strip moved forward one time division. After some practice the record could be made without diverting *E*'s attention from *S*.

Results. The method of recording described above made possible an analysis of the time spent by *S* in each of the 7 types of behavior recorded, as well as a detailed analysis of the behavior that preceded each answer given. Only 3 of the several possible time-analyses of these data are required for purposes of this study, namely: (1) the time devoted to the overt comparison of cards with cards, or of cards with answers, including sections 2, 3, and 4 of the record outlined above; (2) the time spent in the examination of individual cards, *i.e.* section 1 of the record; and (3) the time spent in recording answers, *i.e.* section 5 on the record.

Fig. 2 shows the distribution of cases for time spent in comparison in 15-sec. time-units. Note that the first class interval extends only from 0 to 1.

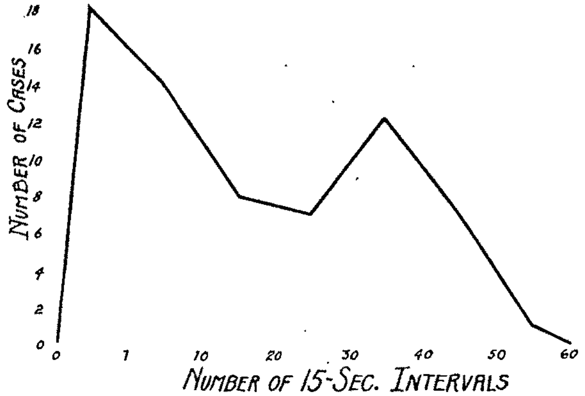

FIG. 2. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN COMPARING THE CARDS AND THE ANSWERS

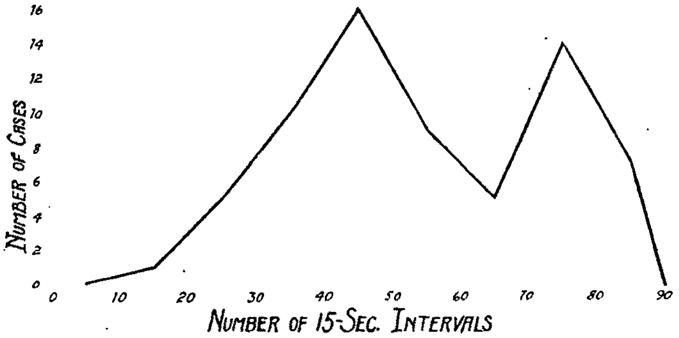

FIG. 3. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN EXAMINING THE INDIVIDUAL CARDS

Fig. 3 gives a similar distribution of cases for time spent in the examining of individual cards.

Fig. 4 gives a similar distribution of cases for time spent in recording answers.

The bimodal character of the distributions shown in Figs. 2 and 3, as compared with the more normal shape of that shown in Fig. 4, indicates that with reference to both time spent in comparison and time spent in the examination of individual cards, the *Ss* can be divided into an upper and a lower group. That this division is a function of the variables dealt with, and not common to all time studies in this experiment, is shown by the fairly normal nature of the distribution for the time spent in recording answers.

That the upper modal group in one distribution corresponds roughly to the lower modal group of the other distribution is evidenced by the negative coefficient of correlation, –0.86, P.E. 0.02, between time spent in comparison and time spent examining individual cards. The Ss therefore can be divided into two groups: (1) those who devote considerable time to the comparison of cards or of cards and answers and little time to the examination of individual cards; and (2) those who devote little or no time to comparison and relatively much time to the examination of individual cards.

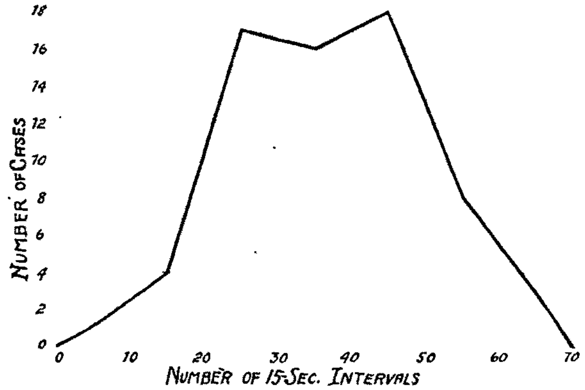

FIG. 4. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN RECORDING ANSWERS

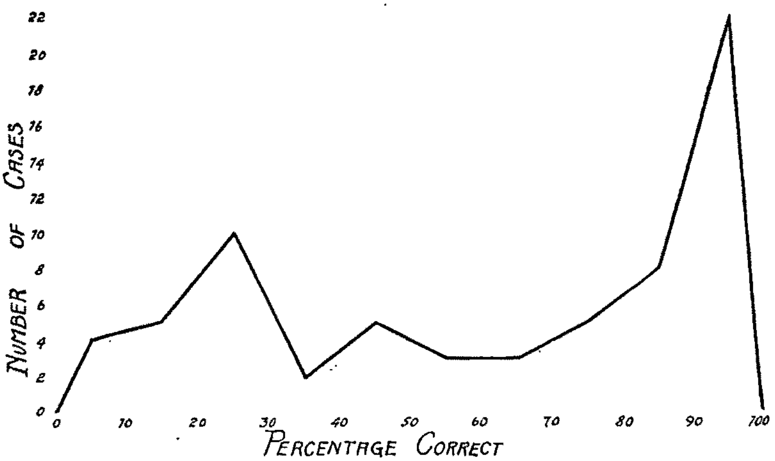

FIG. 5. DISTRIBUTION OF THE REPORTS ACCORDING TO THE PERCENTAGE OF CORRECT ANSWERS

Fig. 5, giving the distribution of scores in terms of the percentage of correct answers to the number of answers given, shows a bimodality similar to that seen in Fig. 2 and 3. This suggests a relation between the answers given and the distribution of time.

It is obvious from the nature of the experimental material that correct answers can be obtained rather easily by comparison. Answers obtained in any other manner may not be correct.

TABLE I

Showing the Answers Given for the Drawings on the First Card by 21 Ss

Drawings	Answers					
	Royal Descent	Arms of an Earl	Political Prominence	Companion of Bath	Antiquity of Title	Title Discontinued
Triangle	2	8	0	2	3*	6
Tree	17*	0	0	0	2	2
Cross	2	9	2*	4	1	3
Flag	0	3	11	3	1	3*
Diamond	0	1	4	1*	11	4
Hand	0	0*	4	11	3	3

* Indicates correct answers in each instance.

Table I, showing the answers given for each drawing of the first coat of arms by *S*s who had examined no other card previously to recording these answers, indicates that these answers are not merely a chance arrangement. Accordingly there is some factor other than chance that is operative in determining answers not derived by comparison. The basis for these answers can be found in the explanations given by the *S*s. To illustrate, the reasons are listed below for the 4 instances shown in Table I in which over half of the *S*s agree in assigning the same answer to a particular drawing, namely: (1) the 17 answers in agreement that are correct for tree; (2) the 11 answers in agreement that are incorrect for flag; (3) the 11 answers in agreement that are incorrect for diamond; (4) the 11 answers in agreement that are incorrect for hand.

(1) Reasons given for assigning, 'Arms of a family of royal descent' to tree, which is correct:

It is a family tree. (5 times).
The tree is a royal oak (3 times).
A tree stands for descent (twice).
A family tree representing descent (twice).
Royal Families can trace their descent, hence a tree.
The tree has many branches like the Royal Family.
The tree is deeply rooted like the Royal Family.
The tree is firmly rooted in the land like the Royal Family.
The tree is an oak indicating uprightness, strength and age; the Royal Family would be stronger, older and above any other family.

(2) Reasons given for assigning, 'A family of political prominence' to flag, which is incorrect:

Flag stands for patriotism (3 times).
Flag stands for the country (3 times).
Banner signifies leaders of political party.
Banner is a political symbol.

The banner would be waving for everyone to see.
The flag is the symbol of the country.
The only inference left.

(3) Reasons given for assigning, 'The title was established one century ago' to diamond, which is incorrect:

The diamond stands for a century (3 times).
Only answer left (3 times).
Conventional sign for 100.
Diamond sign of age.
The diamond has 4 quarters like a century.
The diamond represents a complete whole like a century.
The diamond has completed sides like a century.

(4) Reasons given for assigning, 'Companion of the Bath,' to hand, which is incorrect:

The hand stands for comradeship (4 times).
The hand suggests washing (twice).
The hand indicates a companionable order.
It is the hand of comradeship.
It is the helping hand of comradeship.
The hand is used in bathing.
It is the mailed fist of a military order.

These examples, which could be multiplied many times, indicate that in the great majority of instances answers derived without comparison are based upon previously established associations.

For purposes of comparison the following reasons are listed which were given by all *S*s assigning the correct answer to hand, which is 'arms of an Earl.'

By comparing cards 1, 6, 7 (22 times).
By comparison (5 times).
By comparing cards 1, 6, 7, 9 (3 times).
By elimination.
A hand stands for the power of an Earl.

Here only one answer out of 32 is based upon a previously established association, the other 31 being based upon an analysis of the experimental material by means of comparing various cards.

The answers, therefore, on the basis of the reasons assigned for giving them, can be divided into (1) those based upon previously established associations; and (2) those derived by an analysis of the experimental material by means of comparison. Similarly the *S*s can be divided into 2 groups: (a) 32 *S*s who gave predominantly answers based upon previously established associations. In this group are 26 *S*s who gave only answers based upon previously established associations, and the smallest percentage of such answers given by any one *S* in this group was 64% of all the answers given by that *S*; and (b) 35 *S*s who gave predominantly answers based upon the analysis of the experimental materials by comparison. Twenty-five members of this group gave not more than one answer each based upon previously established associations, and the smallest percentage of answers derived by comparison given by any one *S* in this group was 78% of the total answers given by that *S*.

That this division of *S* agrees remarkably well with the time analysis given above in Figs. 2 and 3 is shown by the superimposed graphs shown in Figs. 6 and 7.

Fig. 8 shows the 67 *Ss* in an array based upon the time spent in the examination of individual cards, shown by the strokes. The time spent by each *S* in the comparison of cards and answers is shown by the dashes. The numbers along the base line show the division of *Ss* based upon the nature of their answers: 1 indicates that the *S* belongs to the group which derives answers from previously established associations; 2 indicates that the *S* belongs to the group which derives answers by comparison.

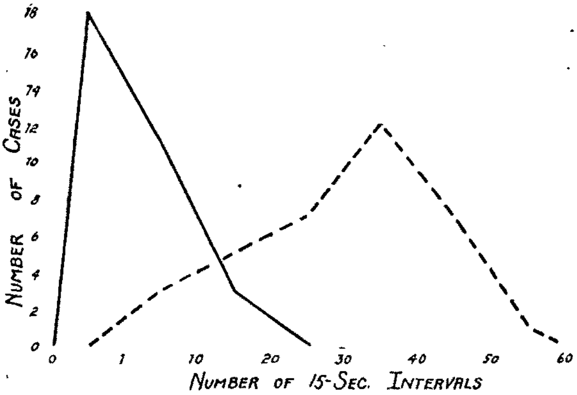

FIG. 6. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN COMPARING CARDS AND ANSWERS

——— = Answers based upon previously established associations.

– – – – = Answers based upon comparisons.

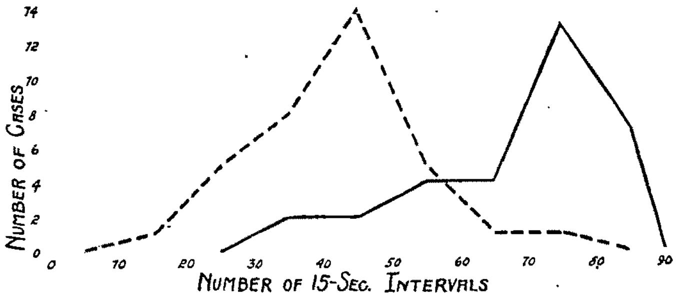

FIG. 7. DISTRIBUTION OF THE REPORTS ACCORDING TO THE TIME SPENT IN EXAMINING THE INDIVIDUAL CARDS

——— = Answers based upon previously established associations.

– – – – = Answers based upon comparisons.

The pronounced agreement between the division of the *Ss* on the basis of their answers and the time studies justifies the division of the *Ss* into (1) those who emphasize almost entirely the examination of individual cards and derive their answers from previously established associations, and (2) those who emphasize the comparison of cards or cards and answers and derive their answers through an analysis of the experimental materials.

Conclusions. The above division of *S*s clearly supports the conclusion that there are two well-defined methods of reasoning exemplified in this setting. The first is an interpretative approach, by means of which answers are derived from previously established associations that are remote from the experimental setting itself. The second is analytical, by means of which answers are derived by comparing cards or cards and answers and therefore within the experimental setting.

The first of these, the interpretative, appears to be the more elementary or basic type, while the second, the analytical, appears to be largely a product of training, as indicated by the three following evidences:

(1) One drawing was intentionally introduced into the experimental material which obviously was related to its particular item of information by a very common previously established association; namely, an anchor standing for a family of seamen. This drawing appeared on cards 4, 9, and 11. The time-records show that 77% of the group employing comparison forsook the method of comparison in this case and derived the answer from some previously established association, whereas an examination of the records shows only 31% of this group giving even one other answer that is not preceded by comparison. Hence there is a tendency to revert from the analytical to the interpretative approach when the previously established association is so obvious.

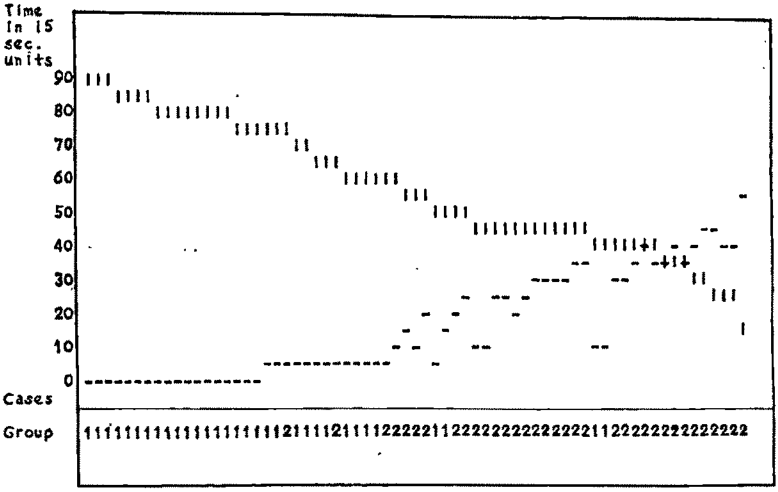

FIG. 8. SCATTERGRAM OF 67 CASES

Time spent in examining individual cards indicated by vertical lines; time spent in comparing cards and answers indicated by horizontal lines; answers based upon previously established associations designated by 1; answers based upon comparisons designated by 2.

(2) The errors made by the group employing comparison show the influence of previously established association in that, as a rule, they are in agreement with the answers given by the first group. For example, assigning 'Order of the Bath' to hand and a 'Family of Scotch origin' to sword accounts for 45% of all the errors

of the group employing comparison, and these are the most frequent answers given by the group employing previously established associations, in both instances.

(3) The third evidence for this conclusion is that 87% of the group employing comparison have studied some experimental science in addition to psychology, whereas only 42% of the group depending upon previously established associations have done so, which indicates that the analytical approach is probably the product of academic training along scientific lines. This is the only striking difference to be found with reference to the content of courses taken by these *S*s, nor did one group appear to obtain better grades than the other in any courses with any degree of consistency.

Since this experiment was undertaken as an initial attempt to investigate by objective methods the nature of scientific inference, the writer feels justified in adding the following brief note to be considered more thoroughly after a further study of the entire problem. Both of the approaches indicated in this investigation find a place in scientific inference. The analytical approach, which establishes relationships within the experimental setting itself, is representative of that type of inference which is characteristic of Mill's Canons of Induction. The interpretative approach, which goes beyond the experimental setting and relates the problem to previously established associations, is representative of that type of inference which characterizes scientific theories of a general nature, as for example some of the theories of nerve action which employ previously acquired information concerning electrical phenomena.

THE TIME-ERROR IN PSYCHOPHYSICAL JUDGMENTS

By CARROLL C. PRATT, Harvard University

If two equal stimuli are presented successively for purposes of comparison with respect to some psychological dimension, more judgments will be secured in the direction of increase (*greater, louder, higher, heavier, longer,* as the case may be) than in the direction of decrease. If several comparison-stimuli are used, a similar preponderance of *greater* judgments will result, provided the comparison stimuli are symmetrically distributed about the standard and the standard is always presented first. In the dimension of intensity this fact, generally referred to, after Fechner, as the negative time-error, was firmly established during the first days of psychophysical experimentation.

Various attempts were made to account for the phenomenon. Müller, working chiefly with lifted weights, was inclined to regard the falling-off in effective comparison-value of the first stimulus as due to fatigue.[1] This view would be plausible enough for intensity of weights, but the time-error appears in other dimensions and modalities and is presumably to be ascribed to some more general factor. In weights fatigue would cause the second member of a pair to appear heavier. In sounds, however, where the time-error is equally marked, fatigue, if it operated at all, would cause the second member to appear softer. Another frequent contention was that the physiological effect of the first stimulus does not completely disappear between the standard and comparison, with the result that an effective increment is added to the second stimulus which favors the judgment in the direction of increase. Borak was able, however, to dispose of this view.[2] Since the physiological effect presumably decreases with the passage of time, a longer interval between standard and comparison should tend to return the judgments more nearly to symmetrical distribution. The exact opposite was found. With increase of time-interval the preponderance of *greater* judgments becomes even more pronounced.

The most frequent explanation of the time-error remained at a more common-sense psychological level. When a judgment is made upon the second stimulus in terms of the first, the standard is already past and gone. The comparison must therefore be based upon some sort of imaginal revival of the first impression. But, if this is true, it is only natural to suppose that the revived image is less intense than was the original impression. The comparison-stimuli are therefore always judged against a lowered standard, so that the *greater* judgments outnumber the *less* and the points of subjective equality uniformly occur below the level of objective equality. The explanation seemed to fit the facts perfectly until Köhler re-

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[1] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit,* 1899, 117.

[2] J. Borak, Über die Empfindlichkeit für Gewichtsunterschiede bei abnehmender Reizstärke, *Psychol. Forsch.,* 1, 1922, 385 f.

minded psychophysicists that the literature of introspection furnishes abundant evidence that successive comparison-judgments are frequently made without any trace of an image whatsoever.[3]

Köhler therefore preferred to state the matter again in terms of physiology. Borak and others had suggested that the physiological effect or trace of the first stimulus combines with the neural processes set into operation by the incoming second stimulus, thus conferring upon the comparison-impressions an enhancement in the direction of increase. Borak's own experiments indicated that this view was hardly tenable. There is another alternative, however, with respect to the neural trace of the first stimulus. It may not combine with the second stimulus. It may instead retain relative independence and furnish the physiological level *against* which the second stimulus acquires effective value for comparison. Köhler defended this view chiefly because, among other things,[4] it accords well with the increase in the amount of the negative time-error with the lengthening of the time-interval between standard and comparison beyond the initial two or three seconds. If the trace is sinking, and if furthermore the trace is the primary condition for touching off the comparison-judgment when the second stimulus comes along, then the degree of preponderance of *greater* judgments at any point along the temporal line after the three-second interval is easily accounted for. Explanation in terms of a sinking physiological trace does service for a non-existent fading image.

Lauenstein now proposes a modification or enlargement of Köhler's trace-theory.[5] The theory as stated by Köhler is susceptible to two different interpretations. "Wir können erstens annehmen, dass alle Spuren durch den Stoffwechsel allmählich abgebaut werden, und dass darauf der negative Zeitfehler bei längeren Zwischenzeiten zurückzuführen ist. Oder wir können zweitens voraussetzen, dass sich unter den Umständen dieser Versuche benachbarte Spuren auf die Dauer aneinander angleichen. Dann wäre der negative Zeitfehler durch die Annäherung der Spur des ersten Prozesses an die Spur des Zustandes der Reizlosigkeit zu erklären. Zwischen diesen beiden Hypothesen lässt sich nun leicht eine experimentelle Entscheidung herbeiführen. Wenn nämlich die zweite Hypothese richtig ist, so muss sich der Zeitfehler umkehren, wenn ich dem ersten Reiz statt der Reizlosigkeit einen noch 'kräftigeren' Reiz unmittelbar folgen lasse. Die Angleichung an den zugehörigen 'stärkeren' Prozess wird eine 'Verstärkung' der Spur bewirken, und es muss sich ein positiver Zeitfehler statt eines negativen ergeben. Trifft aber die erste Annahme zu, so kann diese Veränderung des Verlaufes keinen Einfluss auf den Zeitfehler haben."[6]

The experimental attack suggested in the preceding quotation was carried out by Lauenstein in the Berlin laboratory, with results which were regarded as inconsistent with Köhler's interpretation of the trace. When the interval between the two stimuli presented for comparison was filled by a stimulus of the same modality considerably stronger than the standard, then, instead of a negative time-error, a positive time-error appeared. When a weaker intervening stimulus was used, the usual negative time-error returned. Lauenstein therefore assumed that, since the introduction of a strong background-stimulus produces a preponderance of judgments in the direction of decrease, the physiologically effective trace must be regarded at any one moment during its course not as a mere matter of *sinking,* but rather as a process of *assimilation* going on between the trace and the neural effects of surrounding stimuli.

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[3] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175.

[4] I am not concerned in the present context with the elaborate physico-chemical speculations with which Köhler fortifies his arguments. They are irrelevant to the present problem.

[5] O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 17, 1932, 130-177.

[6] *Ibid.*, 152.

Unfortunately Lauenstein's own theory is also subject to equivocal interpretation, and must be regarded as too hasty a generalization. Within the very special conditions of his experiment his hypothesis is reasonable enough; but the conditions are very special indeed. If an hypothesis is tenable in one set of circumstances, it is not necessarily upset when a different set of circumstances yields different results. The proposition, if A then B, does not mean, if A' then also B. When two objects are presented to a person for comparison with respect to some characteristic, it is not to be expected that the comparison will come off in the same manner no matter on what background the objects are placed. The relative pleasantness of two odors might well be different in a fish-glue factory from what it would be in an otherwise odorless room.

Can it be assumed, moreover, that absence of an intervening stimulus is the limiting case in a descending series of sensory impressions? How is one to conceive "die Spur des Zustandes der Reizlosigkeit?" Does the trace which results from the presence of no stimulus at all also sink with time or does it assimilate itself to its neighbors? If one is to assume, as Lauenstein apparently does, that the trace from stimulus-absence is to be treated in the same manner as the traces from actual present stimuli, then it would follow that the negative time-error from no intervening stimulus must be greater than that from any comparison-judgments in which intervening stimuli are introduced. Assimilation to zero, in other words, must produce a lower trace, and hence a greater preponderance of greater judgments, than assimilation to any value above zero. Evidence can be found, however, which indicates that such is not the case.

The practice of presenting three and even four stimuli in a temporal group for the purpose of studying various types of comparison-judgment has been adopted on several occasions in the Harvard laboratory.[7] The only results comparable to those reported by Lauenstein which at the moment lie ready at hand are presented in Table I. In these experiments the background-stimulus was introduced and then removed between each standard and comparison, whereas Lauenstein used a continuous background. At the level of the results themselves, however, this difference makes no change in the direction of the time-errors. The impressions on which the judgments were made were produced by various heights of fall of a sound-pendulum. The standard stimulus in all cases was set at 45°. The variable stimuli were 37, 41, 45, 49, and 53°, except in one or two cases in Series II and III, when it was necessary to shift them up or down to offset too marked a preponderance of judgments in the direction of decrease or increase. The slight correction necessary to produce a linear series of intensities on a sound-pendulum, $i = \sin^2 (\theta/2)$, was not made, inasmuch as an accurate determination of limens was not involved. Fifty judgments on each stimulus in each series were secured, making therefore 2250 judgments in all. The data were treated by Urban's phi-gamma process of calculation.

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[7] See, for example, W. D. Turner, Intra-serial effects with lifted weights, this JOURNAL, 43, 1931, 1-25.

Preliminary experiments made it clear that the time-error became negative for all three *Os* at about a three-second interval between standard and comparison. The pause between these two stimuli was therefore kept constant at 4 sec. for all judgments. In Series I (Normal) no stimulus was interposed between standard and comparison. The background was phenomenally empty. In Series II (Loud) an intermediate stimulus, produced by a fall of the pendulum from 70°, was inserted between the standard and comparison. In Series III (Soft) a fall of the pendulum from 20° was interposed.

It is clear from an inspection of the table that the results are similar to those secured by Lauenstein. The table presents, for each series, the upper and lower limens in columns 1 and 2, the interval of uncertainty in column 3, and the point of subjective equality in column 4. In Series II (Loud), in which a strong intermediate stimulus comes onto the background, the PSE for each observer is above 45°, indicating therefore a *positive* time-error. The trace of the standard stimulus, according to Lauenstein, assimilates to the very much louder sound in the background and hence produces a preponderance of judgments in the direction of decrease. In Series III (Soft) the reverse situation obtains. The trace assimilates to a very much softer sound, so that judgments in the direction of increase predominate. So far the results agree perfectly with Lauenstein's hypothesis. But how about results made upon a silent background? In Series I (Normal) the trace must assimilate to a still softer sound than in Series III, namely, to none at all (*Reizlosigkeit*). The PSE should therefore be lower in I than in III. For each *O*, however, the PSE in I is higher than the PSE in III. This fact consequently makes it questionable whether Lauenstein is justified in extending his hypothesis to include the normal comparison-judgment in which the background is phenomenally empty.

TABLE I

Upper and Lower Limens, Intervals of Uncertainty, and Points of Subjective Equality for Three Series of Judgments on Auditory Intensities

	I (Normal)				II (Loud)				III (Soft)			
	U	L	Un	PSE	U	L	Un	PSE	U	L	Un	PSE
G	46.79	42.53	4.26	44.66	50.25	42.73	7.52	46.49	46.59	39.00	7.59	42.79
H	45.38	41.68	3.70	43.53	52.80	45.19	7.61	48.99	44.80	38.57	6.23	41.68
F	46.28	42.17	4.11	44.22	55.54	46.43	9.11	50.98	44.74	39.46	5.28	42.10

The size of the intervals of uncertainty is also worth noting. With the possible exception of *F* in Series III, these values are all considerably larger in Series II and III than they are in Series I, indicating that, although the intermediate stimuli are not included within the *Aufgabe* of comparison, they nevertheless obtrude themselves as very real phenomenal distractors. It is not at all unlikely, as a matter of fact, that this very distortion of the normal comparison-judgment represents the basic difference between Lauenstein's results and the results usually secured in psychophysical judgments.

Similar findings with lifted weights have been reported by Guilford and Park.[8]

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[8] J. P. Guilford and D. G. Park, The effect of interpolated weights upon comparative judgments, this JOURNAL, 43, 1931, 589-599.

When weights considerably heavier or lighter than the standard were interpolated, the limens, with one exception, were all larger. In the normal series a third weight was also interpolated, but it had the same value as the standard and presumably acted therefore only as a very slight distraction. The PSE of the normal series were also, with one exception, higher than those from judgments in which a light weight was interpolated, but since the normal series had an interpolated weight as well, this difference in amount of negative time-error is inconclusive with respect to the question of assimilation.

Since the course of the time-error with the passage of time and with different time-intervals is anything but a simple function, the results in Table I must not, of course, be regarded as final. They are merely suggestive and are reported as evidence that the issue between *assimilation* and *sinking* is still open. Since the amount of the time-error varies with the number of preceding judgments, a few judgments with lifted weights were taken in order to see what the outcome would be before the effect of accumulated judgments could be serious. The real issue centers about the relative sizes of the negative time-error when there is a very weak interpolated stimulus as contrasted with judgments in which no intermediate stimulus is introduced. It was therefore unnecessary to run through a series with a heavier interpolated weight.

TABLE II

NUMBER OF JUDGMENTS OF THREE CATEGORIES IN TWO SERIES OF EXPERIMENTS WITH LIFTED WEIGHTS

D% = difference in percent between judgments of *greater* and of *less*.

Series	Stimulus (grm.)	B			D%	M			D%	C			D%
		g	*e*	*l*		*g*	*e*	*l*		*g*	*e*	*l*	
I (Light Weight)	108	8	1	1	−33.4	9	1	0	−43.4	8	2	0	−40.0
	100	5	3	2		5	3	2		6	4	0	
	92	2	6	2		4	3	3		2	4	4	
II (Normal)	108	8	1	1	−6.6	9	1	0	−23.4	9	1	0	−20.0
	100	2	4	4		4	5	1		5	3	2	
	92	1	5	4		2	1	7		0	4	6	

These results are presented in Table II. The standard weight was 100 grm., the variable weights, 92, 100, and 108. In Series I the intermediate light weight was 15 grm. In Series II no intermediate weight was used. The interval between standard and comparison, for both series, was 4 sec. The number of *heavier, equal,* and *lighter* judgments is given under the columns *g, e,* and *l.* D% refers to the difference, in percentage, between the *greater* and *less* judgments and is used by Lauenstein as a measure of the size of the time-error. According to Lauenstein the size of D% should be greater in Series II than in Series I. It will be noted that the reverse is the case.

The conclusion which these results suggest is obvious. When the stimulus which sets off a comparison-judgment comes after an intermediate stimulus or a background considerably greater than the standard, then the preponderance of judgments in the direction of decrease makes it reasonable to argue that the trace of the standard has, by means of assimilation, moved upward. When the background or intermediate stimulus is considerably less than the standard, the process of assimilation is reversed, thus producing preponderance in the direction of increase. When, however, there is no intermediate stimulus nor appreciable background, the trace merely *sinks*. With the exception of vision, this last case is the normal situation for comparison-judgments in psychophysics. Whatever of assimilation exists is brought about either by intra-serial effects[9] or by a general level or equilibrium of judgment[10] created by preceding comparisons either near or remote in time.

Lauenstein insists that in nearly all judgments there is some sort of effective background. "In fast jedem Sukzessivvergleich ist dieser Fall verwirklicht, da in der Pause zwischen den Wahrnehmungen 1 und 2 gewöhnlich irgendwelche anderen Wahrnehmungen liegen."[11] A bit later, however, he seems less certain, and suggests that only a limited number of items can possibly be effective for assimilation. "Die Angleichung der Spuren an ihre Umgebung gilt deshalb nur für ganz bestimmte einfache Verhältnisse. Auf jeden Fall werden sich überhaupt nur die Spuren aneinander angleichen können, die in einem konkreten Strukturzusammenhang miteinander stehen, und man kann wohl erwarten, dass die Angleichung um so stärker sein wird, je enger dieser Zusammenhang ist."[12] The latter suggestion seems by far the more reasonable one. Certainly in judgments of sounds and weights "irgendwelche anderen Wahrnehmungen" do not generally clutter the perceptual field. For sounds the background is silence, and silence seems not to stand in a close enough structural connection with the auditory impressions to serve as a source of assimilation.

Since the earliest days of psychophysical work the preponderance of *greater* judgments in the normal situation of comparison has been ascribed to the falling-off or sinking, with the lapse of time, of the effective value of the first stimulus. What the exact mechanism of this depreciation is, no one knows. Köhler seeks to lend it clarity by placing it within the framework of physico-chemical dynamics. Lauenstein's attempt to replace Köhler's theory is effective, so far, only for judgments made upon artificially induced backgrounds and possibly for vision. He has not yet demonstrated that phenomenally empty backgrounds set up neural processes which combine with the trace of the standard stimulus.

The psychophysical generalization which at present most nearly fits the principal data considered in the preceding discussion can be stated in the following manner. *The effective comparison-value of the first member of a pair of stimuli presented for judgment with respect to intensity decreases in magnitude after the first three seconds, unless the impression from some secondary stimulus constitutes an integral part of the comparison-field, in which case the comparison-value of the first stimulus becomes a function both of time and of the magnitude of the secondary stimulus relative to the first comparison-stimulus.* Experiments are now in progress which are designed to throw more light on certain other factors which influence the course of the time-error.

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[9] S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-150; Turner, *op. cit.*

[10] H. L. Hollingworth, The central tendency of judgment, *J. Philos.*, 7, 1910, 461-468; J. G. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness*, 1932, 222-237.

[11] *Op. cit.*, 135.

[12] *Ibid.*, 170.

AESTHETIC PROPORTION

By FRANK C. DAVIS, University of California at Los Angeles

Students in the sections of the course in Elementary General Psychology at the University of California were asked to make an attempt to visualize what seemed to them, at the moment, the rectangle of most pleasing proportions, and then to draw it freehand on blanks provided them.[1] About 40 min. later they were asked once more to visualize what seemed to them at *that* moment the most pleasing rectangle, and to draw it freehand as before. The blanks were 5 in. square. The *S*s were requested to draw the base of the rectangle longer than the height, except in case they chose the square as being "most pleasing" in its proportions.

The *E* wished to determine (1) what the general reaction of these *S*s would be to such an *Aufgabe;* in case there were fairly widespread acceptance of the task, he wanted to find out (2) whether those who claimed to be lacking in visual imagery were reliably differentiated (with respect either to deviations from a central tendency, or with respect to the degree of consistency of their two preferences) from those who claimed to be able to visualize clearly their most preferred rectangle or rectangles.

Concerning (1) it may be said that philosophers sometimes comment caustically upon the attempts of Fechner to lead the way in the psychologizing of aesthetics, with aesthetic proportionality his starting-point. It should be patent to all, they argue, that proportionality is always *of something,* and hence there can be no "most pleasing proportions" *per se.* This they hold in spite of the fact that most people will confess to having a favorite color, or a favorite odor, whereas those individuals who are (relatively) musically unsophisticated may even claim that they like a pitch of specified vibration rate, or a given musical interval, better than other pitches and intervals. At any rate, the one way to find out whether people find proportionality pleasing in its own right is to put the question to experimental test, as Fechner did. If it be found that they can and do express such a preference, then the question is not psychologically an idle one as to the source or sources of the preference.

Those psychologists who stress the rôle played by the environment in determining our responses insist that preference for this or that proportion, for this or that rhythm, or for any other datum whatsoever, is primarily a matter of habituation. "We like what we are used to," is the simplest statement of this formula.[2] The

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[1] The data referred to in this note were collected at the University of California at Berkeley as part of an investigation dealing with the functional significance of individual differences in imagery.

[2] White and Landis believe that they have good grounds for the conclusion that "with one type of material (the human profile) and with one type of observers (children and college students), there is a tendency for familiarity to produce pleasure and for unfamiliarity to produce displeasure or dislike." R. K. White and C. Landis, A reply to Dr. Murray's concept of aesthetics, this JOURNAL, 43, 1931, 290.

proportions with which we are most familiar, the ones to which we have been most often exposed, will *ipso facto,* according to this view, be the proportions which we prefer. A preference for green or blue is readily accounted for on such a basis, since these are the colors most prevalent in 'nature'—the colors of grass and trees, of sky, lake, and ocean; and so on.

There are many persons, however, to whom this manner of explaining our preferences seems very definitely to be an oversimplification of the facts. In the matter of aesthetic proportion, for example, it would seem that if the factors of ubiquity and propinquity were the only or chief ones operating to determine preference, then the Ford Model-T motor vehicle should have been esteemed an aesthetic gem, since it undeniably outnumbered every other make of automobile on the highways for a span of years longer than one now cares to contemplate. But as a matter of fact, the ugliness of its proportions had as unanimous agreement as did its utility and its stamina.

The Gestaltists, of course, will seek a different explanation of preference. While presumably not denying that something like 'habituation,' or 'conditioning' in the relatively loose, less sharply defined sense in which Watson often uses it, plays a certain part in behavior-phenomena, they will characteristically place much more emphasis upon the rôle played by the organism itself in the organization of its responses. For Watson and other behaviorists of the "end-term" variety the nervous system "conducts;" for the Gestaltists it also "configures," and for them this configuring property is the *sine qua non* in the explanation of psychological phenomena.

During the course of the present experiment only 4 out of a total of 310 *Ss* refused outright to express a preference. These four, it developed, had been forewarned in a class in the philosophy of aesthetics, in which they were then enrolled, that they might expect to be asked by experimental psychologists to do just such an illogical thing, and they were primed with all the possible arguments against such a procedure. When, however, the *E* drew on the blackboard two rectangles, one 2 ft. long by 2 in. wide, the other much shorter and somewhat wider, all four agreed that for some reason the latter seemed more pleasing than the former.

Instead of averaging the two ratios which each *S* yielded, on the assumption that the 'most pleasing rectangle' would necessarily remain stable for most *Ss* over a 40-min. interval, with the differences in the rectangles-as-measured due either to inaccuracies in the drawing or in the measuring, or both together, the two were treated as independent preferences, on the assumption that a given *S* might very well have several, or many, approximately equal preferences, one of which would come out on the first occasion and another on the second. It was found that the ratios (base over height) of the most pleasing rectangles drawn by these *Ss* ranged from 1.00 (the square) to 9.75, with a highest mode at 2.00, but with other modes, almost equally high, at 1.75 and at 2.25. See Fig. 1. It is interesting to note that in the interval from 1.55 to 1.64, which includes the so-called 'golden section,' slightly less than 3% of the total number of preferred proportions are found. Between the limits, 1.60 to 2.29, are included 54% of the 620 preferences.

Fig. 1 gives the sort of picture which one might expect if the experimental group were markedly heterogeneous with respect to whatever the factors may be which determine "aesthetic preferences" as recorded under the conditions of this experiment. If these *Ss* differed considerably with respect to (1) their interest in the task, (2) their conscientiousness in attempting to follow directions, (3) their ability to visualize a "most preferred" rectangle, (4) their ability to draw freehand the rectangles which they "imagined," and (5) the proportion or proportions preferred by them, then the distribution to be looked for would be something like the one found. The task itself might be expected to interest *Ss* of more or less "general" aesthetic sensibility (if such general sensibility be for the moment hypothetized) to a greater extent than it would *Ss* of predominantly scientific or legalistic turns of mind, for example. If some *Ss* were aware of no clearly defined preferences, but

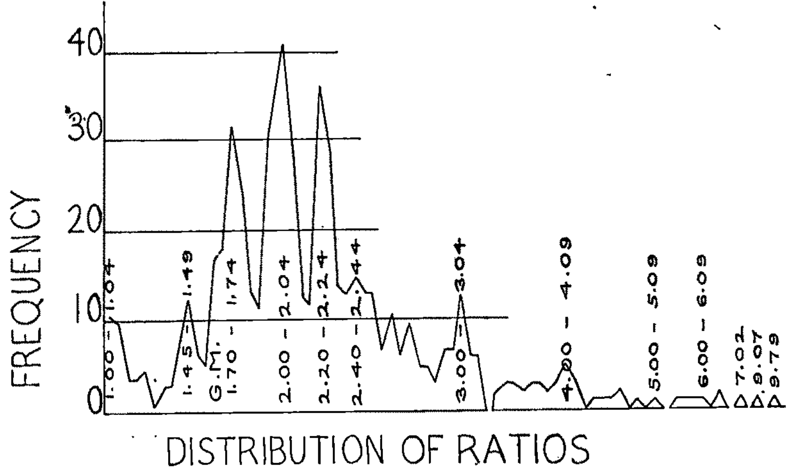

FIG. 1. DISTRIBUTION OF RATIOS OF THE DIMENSIONS OF MOST PREFERRED RECTANGLES.

(310 *Ss*, 2 preferences each, 620 ratios. The ratios are distributed in class intervals of 0.05 to the point where the abscissa is first broken. Beyond that point they are grouped in class intervals of 0.10, for convenience of presentation.)

coöperated nevertheless to the extent of drawing their two rectangles in more or less haphazard fashion, their rectangles would tend to be random in their proportions, and also with respect to the degree of similarity which the second bore to the first. But if *all* the *Ss* had approached the task with this attitude, the chances would seem to be remote (1) that any pronounced central tendency would appear, (2) that any appreciable number of *Ss* would duplicate the proportions of their first rectangle in another drawing 40 min. later.

As a matter of fact, some 31% of the paired preferred-ratios differed by less than 0.10, indicating for the *Ss* concerned a high degree of consistency. Two out of every three of these 'consistent'. preferences were for one or another of the modal ratios, 1.75, 2.00, or 2.25. In the case of some of the *Ss* in this group, the absolute dimensions were three or more times as large in the case of one of the rectangles as they were in the other, with the two ratios nevertheless identical. The total range of the ratio-differences was from 0.00 to 3.21, the greatest frequency being that just noted of 31% for differences between 0.00 and 0.10; the next highest frequency for any one class interval was 17% for differences between 0.20 and 0.30. However, ratio-differences from 1.00 to 3.21, although scattering, totalled 20% of the entire number. This means that while nearly a third of the *S*s in this experiment have so well in mind some particular most-preferred rectangle that their two drawings, though separated 40 min. in time, approach identity, nevertheless one out of every five of the total number of *S*s manifests very unstable preferences indeed under these same experimental conditions. A coefficient of correlation, under the circumstances, may be a somewhat ambiguous index to the degree of 'reliability,' or better, consistency, of such preferences. It was computed, however, and proved to be 0.67.

Whatever factors may be at the root of the individual differences noted, imagery is not one of them. The writer's 'hunch,' that those reporting weak visual images or none at all would be found at the extremes of the distribution, was unsupported by the experimental results. 'Intelligence,' also, as measured by Army Alpha test, proved not to go with a tendency either toward consistency of preferences, under such conditions, or toward variability, as is indicated by a correlation coefficient of —0.23 ± .06 between size of the ratio-difference and Alpha score.

These results are of interest in two connections. First, the wide range of the rectangle-ratios "most preferred," and the pronounced modes, lend emphasis once again to the conclusions reached by Angier,[3] that it is fallacious to look for a "golden section" in the average of the preferences of a number of individuals. Furthermore, since so many *S*s express a preference for one proportion on one occasion, and then register a preference for a markedly different proportion on another occasion, it seems obvious that the psycho-physical method of average error would yield ambiguous results with such *S*s unless intelligently applied. If, for example, the *S* were one who happened to like rectangles of the proportions of 3:1, 2.25:1, and 1.62:1 equally well, and if his 'out' determinations started well beyond the 3:1 proportion while his 'in' determinations started well inside the 1.62:1 proportion, he would presumably get a series of 'out' determinations fluctuating around the 3:1 ratio and a series of 'in' determinations fluctuating around the 1.62:1 ratio, and to take the mean of the means of the two series as indicating his truly '*most*-preferred' proportion would be an unintelligent procedure of which perhaps no competent experimentalist would be guilty.

The above line of reasoning is of course contingent upon (1) the distribution of preferences being genuinely multi-modal, and (2) the existence of *S*s who can be accurately described as 'balanced' in their proportional preferences between several distinct ratios. There is, naturally, no *a priori* reason to expect a distribution of preferred-proportions to be unimodal and 'normal' in shape, although it may be admitted that that was the writer's preëxperimental bias. (On the other hand, it is possible, though it seems hardly likely, that the curve shown in Fig. 1 would eventually smooth out, with the addition of many more cases, with a resultant mean probably somewhere between 1.70 and 2.29.)

The second connection in which the present data are of interest has to do with the results reported by Hambidge[4] from his measurements of Greek vases and temples. Hambidge reports the prevalence in ancient Greek art and architecture of "root-ratios." Rectangles having these ratios have what he has termed "dynamic symmetry"—the total surface of such a rectangle is divisible into a number of exactly similar rectangles. Especially prevalent in Greek works of art are the ratios 1.72 + $(3)^{1/2}$, 2.00 $(4)^{1/2}$, and 2.24 — $(5)^{1/2}$. These approximate the modal ratios around which cluster the majority of the preferences indicated by the *S*s in the present experiment.

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[3] R. P. Angier, The aesthetics of unequal division, *Psychol. Monog.*, 4, 1905, (no. 17), 541-561.

[4] J. Hambidge, *The Parthenon and other Greek Temples: Their Dynamic Symmetry,* 1924.

The interpretation of these results is far from obvious. Hambidge's "dynamic symmetry" has not been favorably received by those philosophical aestheticians who resent both the application of measurement and the attempt to educe general principles in the field of aesthetic preference. But, while the theory may perhaps be 'laughed off,' the measurements cannot be, and the accuracy of Hambidge's measurements seems not to have been challenged.

To the writer's knowledge, Langfeld is the only psychologist who has taken note of Hambidge's data and of his conclusions. He notes with apparent approval the latter's emphasis upon areal, rather than linear, symmetries.[5] From the standpoint of the *Gestalt* psychology Hambidge's findings, and the results from the present experiment, might be taken to indicate that for some reason (at present obscure) rectangles having approximately these proportions effect better 'closure' for most *S*s than do other rectangles. On this hypothesis the basis for the preference would presumably be the resolution of tension which accompanies, or which is, closure. The tensions, in this instance, might be peripheral, cortical, or both.

SUMMARY

(1) Contrary to the *a priori* judgment of certain philosophically-minded aestheticians, college students in general find it neither absurd nor impossible to imagine a "most-preferred" rectangle.

(2) When the base-to-height ratios of 620 rectangles (2 each from 310 *S*s) were computed and tabulated, the resulting distribution showed the preferences to be grouped around several modal ratios.

(3) College students differ greatly with respect to the stability of their preferences under the given experimental conditions (freehand drawing of two rectangles, 40 min. apart).

(4) The facts presented in the results of this and former experiments indicate the fallaciousness of attempts to establish *a* 'golden section' based upon averages, either of the preferences of a group of *S*s or of a number of preferences from a single *S*.

(5) No relation was found between the imagery reported and the *S*'s performance in the drawing of preferred rectangles.

(6) No significant relationship was found between 'consistency of preference,' under the conditions of this experiment, and Alpha scores.

(7) The modal ratios for the most preferred rectangle in the case of the present investigation approximate closely the ratios reported by Hambidge as those characteristically employed by the Greeks in their planning of the proportions of temples and vases.

(8) The *Gestalt* principle of 'closure' seems to supply a possible psychological basis for aesthetic preferences of this sort.

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[5] H. Langfeld, *The Aesthetic Attitude,* 1920, 129.

IMPROVED NORMS FOR FOUR PERSONALITY TESTS

By Ross Stagner, University of Wisconsin

While psychiatric analysis of personality in terms of individual interviews is no doubt the most thorough and accurate method of study available at the present time, the cost in both time and money makes its use prohibitive. An institution desiring to make a survey of its students in developing a mental hygiene program would find such a method quite impracticable. The only alternative offered by psychology at the moment is the use of group tests, which more than compensate in utility for their loss in validity.[1]

A number of useful group tests are available for the college level. These have been developed on the basis of the best knowledge available with reference to the trait tested; they have fairly high reliabilities (high enough for group testing) and some measure of validity. One fact tends to reduce their utility, and this is the lack of adequate norms.

The individual developing a test usually does so during his tenure at a single educational institution. For normal groups he naturally uses the students available there. As we propose to show, the personality traits average in a given institution may deviate considerably from those prevalent elsewhere, and consequently the testing of a small group at another institution may lead to quite erroneous conclusions concerning the relative quality of the students, since the standard group does not represent a true standard.

The best method for the development of nation-wide norms for a given test would be that of a cumulative frequency distribution, based on the administration of the test to students in colleges taken at random over the entire country. This method works admirably with the American Council Psychological Examination.

Another method, less desirable, is the determination of the average score and standard deviation for a large number of groups. This does not offer the possibilities of interpretation which one has with the frequency distribution, but it insures a stable average and a known degree of variability. Garvey[2] has recently published a formula which makes it easy for anyone using a calculating machine to determine the standard deviation for a large group from the means, sigmas and frequencies of the component groups.[3] This formula has been employed in the present case to determine the 'true' means and standard deviations from the studies reported in the literature.

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* Accepted for publication November 5, 1931.

[1] It should be understood that we refer to their use in surveys, selecting individuals who require further analysis by the interview method.

[2] C. R. Garvey, Sigmas of combined distribution calculated from sigmas, means and frequencies of component distributions, *J. Educ. Psychol.*, 22, 1931, 307-310.

[3] The formula is: $\sigma^2_{12} = [[(M^2_1 + \sigma^2_1)\ N_1 + (M^2_2 + \sigma^3_2)\ N_2] / N_1 + N_2)] - M^2_{12}$. It may, of course, be extended to any desired length.

In the course of administering various personality tests, we have come to feel the need for norms which would not be influenced by the type of students common to a single university. The task of collecting them, however, has been rendered difficult by the practice, common in articles on testing, of presenting correlations or other data without the measures of central tendency and variability on which they are based. Frequency distributions are even more rare. The expense of publishing tables is causally associated with this situation. Of the large number of articles using the Pressey X-O test, for example, we have not found one giving a frequency distribution of scores, and relatively few giving means and sigmas.

TABLE I

IMPROVED NORMS OF CENTRAL TENDENCY AND VARIABILITY FOR FOUR WELL-KNOWN PERSONALITY TESTS

Test	N	M	S.D.
Laird Inventory C 2	1021	15.96	5.03
Social Intelligence (Form 2)	1235	99.18	17.79
Thurstone Neurotic Inventory	1257	43.63	23.67
Pressey X-O Affectivity (Form A)	340	189.24	55.71

We have attempted to derive improved norms for four well known and widely used personality tests: the Colgate inventory C 2 for introversion; the Thurstone neurotic inventory; the Pressey X-O test (Form A);[4] and the Moss Social Intelligence test, Form 2. For the first two, we present frequency distributions from different studies; for the latter two, we have the frequencies only for tests immediately available to us, as we could find no others published. We give means and sigmas based on a large number of cases, for each of the four. Studies on the Allport A-S test offer a large number of cases, but none of the studies are in proper form to be used for norms. The Allport test is separately standardized for men and women, and the same score on a different form has quite a changed

TABLE II

FREQUENCY DISTRIBUTION ON THE LAIRD INVENTORY C2

Score	Frequency		
	Men	Women	Total
37–39	0	1	1
34–36	0	1	1
31–33	1	3	4
28–30	1	6	7
25–27	5	27	32
22–24	10	42	52
19–21	17	64	81
16–18	33	97	130
13–15	41	82	123
10–12	28	66	94
7– 9	6	24	30
4– 6	4	7	11
Total	146	420	566

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[4] Only affectivity scores have been compiled, as the idiosyncrasy score should properly be developed anew for each group studied.

TABLE III

Frequency Distrubution on the Thurstone Neurotic Inventory

Score	Frequency		
	Men	Women	Total
150–164	0	1	1
135–149	0	0	0
120–134	0	0	0
105–119	5	12	17
90–104	14	13	27
75– 89	32	28	60
60– 74	64	69	133
45– 59	125	120	245
30– 44	177	106	283
15– 29	198	107	305
0– 14	105	33	138
Total	720	489	1209

TABLE IV

Frequency Distribution for the Pressey X-O Test

Score	Frequency		
	Men	Women	Total
310–329	0	1	1
290–309	1	0	1
270–289	0	0	0
250–269	0	3	3
230–249	4	8	12
210–229	11	8	19
190–209	6	12	18
170–189	6	12	18
150–169	14	6	20
130–149	13	10	23
110–129	10	8	18
90–109	9	4	13
70– 89	2	3	5
50– 69	1	1	2
30– 49	1	0	1
Total	78	76	154

TABLE V

Frequency Distribution for the Neymann-Kohlstedt Test

Score	Frequency		
	Men	Women	Total
+36–+42	2	0	2
+29–+35	1	1	2
+22–+28	3	4	7
+15–+21	2	8	10
+ 8–+14	20	11	31
+ 1–+ 7	25	24	49
− 6– 0	18	22	40
−13–− 7	7	10	17
−20–−14	2	9	11
−27–−21	0	3	3
Total	80	92	172

significance. Yet several studies report the scores for the two combined, as though this were a perfectly legitimate procedure. Until this condition is improved, we cannot collect norms.

We are also presenting a frequency distribution for the Neymann-Kohlstedt extroversion-introversion test, which has been published without statistical data. The measures of central tendency and variability on our group for this test are published elsewhere.[5]

Sources. As a matter of perhaps considerable importance to anyone using these norms, we wish to specify the source of each item. By tables, they are as follows:

Table I. Test C 2: Broom,[6] San Diego Teachers College; Whitman,[7] Skidmore College, Colgate University; Stagner,[8] Gustavus Adolphus College.

Social Intelligence test: Broom,[9] Garrett and Kellogg,[10] Columbia; Jersild,[11] Barnard; Stagner.[12]

Thurstone test: Allport,[13] Dartmouth; Barrett, Jacobs and White,[14] Wisconsin; Mears,[15] Wisconsin; Stagner; [16] Thurstone and Thurstone,[17] Chicago.

Pressey test: McGeoch and Whitely,[18] Washington University; Stagner,[19] Wissonsin.

Table II. Broom,[20] and Stagner.[21]

Table III. Allport,[22] Barrett, Jacobs and White,[23] Mears,[24] Stagner,[25] Thurstone and Thurstone.[26]

Table IV. Stagner.[27]

Table V. Stagner.[28]

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[5] R. Stagner. Intercorrelations of some standardized personality tests, *J. Appl. Psychol.*, 16, 1932, 453-464.

[6] M. E. Broom, Critical study of a test of introversion-extroversion traits, *J. Juven. Res.*, 13, 1929, 104-23.

[7] R. H. Whitman, Sex and age differences in introversion-extroversion, *J. Abn. & Soc. Psychol.*, 24, 1929, 207-11.

[8] Stagner, *op. cit.*

[9] M. E. Broom. Note on the validity of a test of social intelligence, *J. Appl. Psychol.*, 12, 1928, 426-28; Further study of the validity of a test of social intelligence, *J. Educ. Res.*, 22, 1930, 403-05.

[10] H. E. Garrett and W. N. Kellogg, Relation of physical constitution to general intelligence, social intelligence and emotional stability, *J. Exper. Psychol.*, 11, 1928, 113-29.

[11] A. T. Jersild, A study of personality, *J. Abn. & Soc. Psychol.*, 25, 1930, 115-20.

[12] Stagner, *op. cit.*

[13] G. W. Allport, Neurotic personality and traits of self-expression, *J. Soc. Psychol.*, 1, 1930, 524-27.

[14] D. Barrett, L. B. Jacobs, and E. M. White, Correlations between four personality tests and certain physical measurements, Thesis, University of Wisconsin, 1931.

[15] C. E. Mears, Interrelationships between academic success, intelligence and social adjustment, M.A. Thesis, University of Wisconsin, 1931.

[16] Stagner, *op. cit.*

[17] L. L. Thurstone and T. G. Thurstone, A neurotic inventory, *J. Soc. Psychol.*, 1, 1930, 3-30.

[18] J. A. McGeoch and P. L. Whitely, Reliability of the Pressey X-O tests for investigating the emotions, *J. Genet. Psychol.*, 34, 1927, 255-70.

[19] Stagner, Emotional factors influencing retention, M.A. Thesis, University of Wisconsin, 1930.

[20] M. E. Broom, *op. cit.*, *J. Juven. Res.*, 13, 1929, 104-23. [21] Stagner, *op. cit.*

[22] G. W. Allport, *op. cit.* [23] D. Barrett, L. B. Jacobs, and E. M. White, *op. cit.*

[24] C. E. Mears, *op. cit.* [25] Stagner, *op. cit.*

[26] L. L. Thurstone and T. G. Thurstone, *op. cit.*

[27] Stagner, *op. cit.*

[28] Stagner, *op. cit.*, Thesis, University of Wisconsin, 1930.

Utility value. The value of the frequency distributions for the Thurstone and Laird tests needs no discussion. These contain a sufficient number of cases to render interpretation of individual scores fairly dependable, and they are based on groups from different college environments, thus eliminating the constant error due to this factor. The frequencies for Social Intelligence, X-O and Neymann-Kohlstedt are based on results from a single group, with too small a number of cases, and can be used only for approximations.

The value of the measures of central tendency and variability presented lies chiefly in the determination of the typicality of groups. (1) Groups from different colleges may be compared with respect to a given trait, and the statistical reliability of the deviations from these norms computed accurately. (2) Groups within an institution may be compared with the norms more accurately than with each other, if the number of cases in each group is small. (3) Extreme groups selected for special study may be picked from cases scoring plus or minus one standard deviation from the mean, using these values, which are more likely to be dependable than those determined from a smaller sample.

As an illustration of the usefulness of such norms, we may analyze the results of the Gustavus Adolphus tests, since this is the only group taking as many as three of the four tests. The mean for this group on the C 2 test was 15.95, and we may therefore feel quite certain that this group is normal in respect to introversion, as the norm value is 15.96.

The mean for this group on the Social Intelligence test is 96.16, and is the lowest group mean reported. It is 3.02 points below the mean norm, but this difference is less than three times its standard error. The difference is about 70% of what it should be to insure a statistically true difference.

When we consider the results for the Thurstone test, we find that this group has an average score of 51.95. This is 8.32 points more than the norm, and is the highest group mean reported. It is more than three times the standard error of the difference, and we may legitimately conclude that there is a true difference present, that is, that this group is actually more neurotic than the average college group, in so far as this test is a valid measure of the neurotic temperament. This analysis also shows that while introversion and neurotic traits are positively correlated, in a group it is quite possible to find an extreme amount of one and perfect normality of the other.

It is to be hoped that individuals using these personality tests will add to the number of cases represented in these norms, by combining their data with those here published. This is especially needed in the case of frequency distributions, as these are necessary for the adequate interpretation of the score of a single individual.

SUMMARY

(1) Means and sigmas for four standard personality tests have been calculated from the data of other investigators and the author.

(2) Frequency distributions have been combined to give more valuable and useful interpretative devices.

(3) The utility of these norms is discussed and illustrated.

THE INFLUENCE OF A SOCIAL FACTOR UPON THE APPRECIATION OF HUMOR

By RUTH EASTWOOD PERL, Columbia University

The specific problem under consideration in the research here reported is whether jokes presented under certain social conditions are judged to be more or less funny than equally humorous jokes presented under different social conditions; the investigation tries to obtain some quantitative measure of this difference.

Allport says that "social environment is necessary in the rôle of a contributory as well as a direct stimulus for our mirthful response."[1] He feels that there are three causes which underlie the hilarity of the crowd; (1) the large number of facilitating stimuli present, such as the sight and sound of others laughing; (2) the release of common restrained impulses; and (3) the feeling of moral sanction for their release.

H. T. Moore performed an experiment slightly similar to the one about to be discussed but using other material.[2] His experiment consisted of measuring a suggestive influence, such as expert opinion, or group judgment, in terms of the number of reversals of individual judgment occasioned by it as compared with the number that might have been expected by chance. He worked with three types of material; namely, linguistic usage, ethical judgments, and musical judgments.

Wheeler and Jordan made an experiment along these same lines.[3] They used questions on economic issues, politics, and campus affairs. They counted the reversals of opinion which took place before and after group opinion was made known to the subjects, or rather, scored the percentages of opportunities to change that each individual accepted.

Preliminary experiments. In the present experiment, the first task was to compile three equated lists of jokes. To do this, a preliminary experiment was performed using as subjects 19 women undergraduates who were members of the class in Experimental Psychology at Barnard College and 12 men undergraduates of the class in Experimental Psychology at Columbia University. Every *S* was given a mimeographed copy of a list of 160 jokes, divided into 4 sets of 40 each. These jokes were as varied as possible. They were gathered from American and English humorous magazines, joke books, vaudeville acts, and by asking people for the best joke they could think of at the moment. With the lists of jokes, the *S*s were given directions as follows.

You are being handed herewith 4 sets of about 40 jokes each. Kindly work on them in absolute privacy, and do not discuss your results until you have handed

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* Accepted for publication November 16, 1931.

[1] F. H. Allport, *Social Psychology*, 1924, 257 f.

[2] H. T. Moore, The comparative influence of majority and expert opinion, this JOURNAL, 32, 1921, 16-20.

[3] D. Wheeler and H. Jordan, Change of individual opinion to accord with group opinion. *J. Abn. Psychol. & Soc. Psychol.*, 24, 1929, 203-207.

> them in. You are to work on one of the 4 sets each evening for 4 evenings (so that you do them all).
>
> We are asking you to read the jokes over to yourselves one at a time and grade them. Be sure to grade each joke as you read it, and do not work back over them. You are to grade the jokes on a 5-point scale: A = excellent or most funny, or one of the 20 best jokes out of 100 that you have ever heard; B = good or very funny; C = fairly funny; D = poor or just slightly funny; and E = very bad, not at all funny or equal to one of the worst 20 jokes out of a 100 that you have ever heard.

From these results three lists of equated jokes were compiled in the following manner. First, all the jokes the judgments of which did not show a strong central tendency, were discarded. If 4/5 of the judgments of a joke did not fall within one grade each side of the median, the joke was not used. For example, if the median of the judgments of joke No. 1 was B, then 4/5 of all the *Ss'* grades would have to be A, B, or C for the reactions to the joke to be considered consistent. Also, if one vote more or less would shift the median so that the joke would not pass the 4/5 test, it was discarded. The use of this criterion left 75 jokes which were given quantitative scores by multiplying the frequency of a joke's A votes by 4, its B by 3, its C by 2, its D by 1, and its E by zero, and then adding these together. If, for example, the judgments of joke No. 4 were arranged as follows, 6 A, 13 B, 8 C, 2 D, and 2 E, then it numerical score would be 6 x 4, plus 13 x 3, plus 8 x 2, plus 2 x 1, plus 2 x 0, which equals 81. From these scores the jokes were arranged in an order of merit and finally three sta-

TABLE I

SHOWING AVERAGE RATING OF EVERY JOKE ON THE THREE LISTS

Joke No.	List		
	A	*B*	C
1	1 7	1.8	2.0
2	0.6	0.7	2.1
3	1.3	0.8	0.6
4	1.4	1.1	1.5
5	1.9	1.7	2.2
6	1.8	1.7	2.9
7	1.1	3.0	1.9
8	2.1	1.6	1.4
9	1.5	1.3	2.4
10	1.9	1.6	1.3
11	1.9	1.3	1.7
12	1.9	2.2	2.6
13	1.9	1.8	2.3
14	1.6	1.8	1.6
15	1.8	0.9	1.7
16	1.8	2.6	2.2
17	1.6	2.9	1.9
18	1.3	1.7	1.2
19	0.5	0.6	1.2
20	2.0	2.2	3.2
21	1.3	1.3	2.8
22	1.8	1.1	2.2
23	1.3	1.4	1.3
24	2.1	2.7	2.6
25	2.0	1.8	1.7

tistically equated lists of 25 jokes each were made. The jokes in one of the three lists were arranged in random order and the other two lists were matched against it, so that there were three equated lists of jokes arranged in equated order.

Main experiments. The Ss for the main experiments were 40 members of a graduate course in Social Psychology. They were asked to rate the three equated lists, which were arbitrarily called Lists A, B, and C, under different conditions.

TABLE II

SHOWING FOR EVERY LIST THE SCORE OF THE POOREST AND OF THE BEST JOKE, THE RANGE BETWEEN THESE EXTREMES, THE MEAN, STANDARD DEVIATION, AND COEFFICIENT OF VARIATION

List	Score on		Range	Mean	S.D.	Coef. var.
	poorest	best				
A	.5	2.1	1.6	1.57	.42	26.75
B	.6	3.0	2.4	1.64	.67	40.85
C	.6	3.2	2.6	1.92	.60	31.25

A 5-point scale was used. List A was presented in mimeographed form, and the *Ss* were asked to rate the jokes in absolute privacy, following directions similar to those given in the preliminary experiment. List B was read to the group as a whole, Professor Murphy and the writer alternating in reading the jokes while each member of the group scored them. List C was presented to the group visually by means of a lantern and slides.[4]

Then each joke was given a total weighted score as in the preliminary experiment, and each of these scores was divided by the frequency or number of *Ss*, (40), in order to get the average score for each joke (see Table I). A crude estimate of the fullness of the laughter at each joke in the group situations was also made.

TABLE III

SHOWING THE DIFFERENCES BETWEEN THE VARIOUS LISTS IN MEAN SCORES AND STANDARD DEVIATIONS, AND THE RELIABILITY AND PROBABILITY OF THESE DIFFERENCES

List	Mean			Standard Deviation		
	Diff.	D/σ diff.	Prob.	Diff.	D/σ diff.	Prob.
B to A	.07	.56	71	.25	2.83	99
C to A	.35	3.02	100	.18	2.20	99
C to B	.28	1.97	98	—.07	.70	76

As Kambouropoulou noted,[5] jokes attended to with a view to grading are not considered funny. Table II shows that the lowest score on List A (the jokes rated in private) is 0.5; on List B (the ones presented vocally) 0.6; and on List C (the visual ones) also 0.6. The highest scores for jokes on Lists A, B, and C are 2.1, 3.0, and 3.2 respectively, giving a range of 1.6, 2.4, and 2.6. The average score of the jokes on List A is 1.57, on List B 1.64, and on List C 1.92, indicating that vocal jokes are considered slightly funnier than private jokes, and visual ones much funnier than either.

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[4] To check up on this experiment a similar group of *Ss* ought to be used and the lists reversed; perhaps List C being rated in private, List B presented visually, and List A read to the group. The lists of jokes used may be found in the writer's unpublished Master's Essay, filed in manuscript in the Columbia University Library.

[5] P. Kambouropoulou, Individual differences in the sense of humor and their relation to temperamental differences, *Arch. Psychol.,* 19, 1931, (no. 121), 1-83.

Table III shows that the reliability of the differences of the averages between B and A is 0.56, meaning that the chances are 71 out of 100 that the true difference is greater than zero. This figure for the difference between C and A is 3.02, showing the chances to be 100 in 100, and that for C and B is 1.97, giving 98 chances out of 100 that the difference between the true averages is greater than zero.[6] These figures were obtained by using the formula for uncorrelated material, as it was assumed that our three equated lists of jokes were three lists picked at random from an infinite number of equally correlated lists of jokes.

The S.D. for List A is 0.42; that for List B 0.67, and that for List C 0.60, with coefficients of variation of 26.75, 40.85, and 31.25 respectively. The reliability of the difference of the dispersions between B and A is 2.83, meaning that the chances are 99 in 100 that the difference between the true S.D. is greater than zero. Between C and A this figure is 2.20 representing 99 chances in 100, and between C and B it is 0.70, representing 76 chances in 100.

TABLE IV

Showing the Average Score of the 8 Poorest and 8 Best Jokes on List A and the Scores of the Equivalent Jokes on Lists B and C

List	Average score of 8	
	poorest	best
A	.88	1.58
B	1.06	1.51
C	1.26	1.67
Av. B and C	1.16	1.59
Av. soc. improv.	.28	.01

The average score of the 8 poorest jokes on List A is 0.88. The average score for the equivalent jokes is on List B 1.06; and on List C 1.26; combining B and C, it is 1.16. This shows an approximate improvement under social conditions of 0.28. The average of the 8 best jokes on List A is 1.58; for the equivalent jokes on Lists B and C it is 1.51 and 1.67 respectively. Combining B and C the score is 1.59, indicating that the social improvement of better jokes is, in this case, only 0.01 as against 0.28 for the poorer ones. The poor jokes, of course, have more room to improve than the good ones, but the scores of all the jokes in this experiment are so low that the tremendous difference between the facilitation of the poor and the good jokes cannot all be convincingly explained by this fact.

All the jokes that were laughed at considerably under the social conditions were rated from 2.2 to 3.2, which means that they were in the highest third of the lists. The scores do not show any appreciable sex differences, although most of the laughter seemed to have been initiated by the women *Ss*.

Conclusions. These results suggest the following conclusions:

(1) Jokes presented to a group vocally are judged to be slightly but not reliably funnier than jokes judged in private.

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[6] If the unreliability of the tests had been taken into account, the significance of the differences would have been even larger.

(2) Jokes presented to a group visually are judged to be funnier than jokes rated in private; and this difference is statistically reliable.

(3) Jokes presented visually seem to be funnier than jokes presented vocally, at least under the conditions of our experiment, which included telling the jokes (List B) in a rather uniform and undemonstrative manner.

(4) The ranges of scores obtained under the two social conditions are much wider than those of jokes rated in private; that is, the funniest and the least funny jokes are much farther apart under the social conditions.

(5) Social facilitation has much greater influence in raising the scores of the poor jokes than it has in raising the scores of the good ones. Perhaps the fact is that all of the poor jokes are facilitated while some of the good jokes are facilitated and others inhibited, thus not affecting the average; but this is just a suggestion and not a conclusion.

THE PSYCHOPHYSICAL DETERMINATION OF THE LIMITS OF PURE GRAY

By Forrest L. Dimmick and George McMichael, Hobart College

The status of gray as an unique or fundamental color was first proposed by Dimmick,[1] and has been further justified experimentally by Dimmick and Holt[2] with a procedure that emphasizes the psychological or qualitative aspects but does not establish the region of pure gray. In the present experiment we have undertaken the determination of the limits as suggested by Rich.[3] We obtained the thresholds for black from gray and white from gray with 3 *O*s by the method of constant stimuli. Our gray was produced by the mixture of 'baryta' white and 'velvet' black papers. The black paper had a white valency of 3% as measured with the Kirschmann photometer. In order to maintain constant light conditions, the mixer was set up in a dark room and illuminated by an Ivanhoe daylight lamp (500 w. on 110 v. D.C.) hung one meter above and one meter in front of the mixer disk.[4] In front of the mixer was a medium gray screen which concealed the disk from *O* except when he was making a judgment. He was instructed as follows: "After a ready signal you will be shown a color disk. You are to report whether it shows any likeness to or hint of white or black. A judgment of 'neither' is permitted." After the signal, *E* raised the screen for 2 sec., exposing the stimulus to *O* who made his judgment of 'black' or 'white.' *E* immediately lowered the screen, shifted the mixer setting and gave a second exposure, and so on. Limens were obtained from 3 *O*s (undergraduate but with some practice in observation), who were given a long course of training in this type of judgment prior to the final series. This training consisted of making a number of similar liminal determinations for colors without, however, having pointed out to them the systematic implications of their judgments.

Results. Table I gives the limits in degrees of white of the region within which

TABLE I

White-Gray and Black-Gray Limens in Degrees of White

Baryta white paper standard. Numbers in parentheses include a correction of 3% for the white valency of the 'velvet' black paper as measured by the Kirschmann photometer.

	White	h	Black	h
G	95.12 (103.07)	.116	73.92 (82.5)	.1006
M	97.4 (105.28)	.0457	73.27 (81.87)	.0852
H	90.01 (98.11)	.0715	75.88 (84.4)	.078
Av.	94.18 (102.15)±2.79		74.35 (82.91)±1.35	

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* Accepted for publication October 19, 1932.

[1] F. L. Dimmick, A note on the series of blacks, greys, and whites, this Journal, 31, 1920, 301.

[2] Dimmick and C. H. Holt, Gray and the color pyramid, *ibid.*, 41, 1939, 284.

[3] G. J. Rich, Black and grey in visual theory, *ibid.*, 37, 1926, 123.

[4] Dimmick, An accurate differential color mixer, *ibid.*, 45, 1932, 798.

neither white nor black were reported. We may take the middle of this region, 80° (92°) white + 276° (268°) black, as the proper proportions for the best 'middle' or unique gray. This is slightly lighter than the gray used by Dimmick in his earlier work. Also, our region is narrower than that calculated from his results, viz., 20° as against 29°. This difference is probably due to the differences in the methods employed and in the practice of the *O*s in the particular type of judgment. For similar reasons it is not possible to make a close comparison of our results with the limens Rich gives, which were offered in substantiation of the earlier contention that quantitative determinations would show a region of gray that did not involve either black or white.

Summary. Our results agree with the contention that gray is an unique color quality, not a black-white composite, and establish 84° (±10°) white + 276° (±10°) black as the equation for neutral gray under our conditions of illumination.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LXII. MOTOR FACTORS IN VOLUNTARY CONTROL OF CUBE PERSPECTIVE FLUCTUATIONS AND RETINAL RIVALRY FLUCTUATIONS

By M. F. WASHBURN and ANNETTE GILLETTE

This study formed part of an experimental investigation comparing the alternations in perspective interpretations of an outline cube with the alternations in colored fields in retinal rivalry, both as regards their rate and as regards their controllability. It proved impossible to compare rates, owing to the factor of monocular dominance in rivalry. The present paper offers the results on relative controllability.

Method. The experimental conditions were as follows. The observers were 35 young women college students. The observations were made in a dark room: the *O* sat with her head in a rest; light came from a 50-watt ground glass bulb about 30 cm. above her head. For the experiments with the cube, a card carrying a drawing, in white lines on a dark ground, of a cube 1 in. a side (2.7 cm.) was placed against the wall on a table to which the headrest was attached; *O*'s eyes were about 60 cm. from this figure. During the periods of observation a metronome beat at 120 strokes per min., and *E* made a short pencil mark on paper with each beat. When *O* said, 'Down,' meaning that the cube was perceived as lying on the ground, *E* made the marks horizontal, and continued to do so until *O* said 'Up,' when *E* made the marks vertical until the next 'Down' signal. A period of observation lasted 2 min., after which *O* closed her eyes and watched the negative after-image of the cube. She reported appearances and disappearances of the after-image, and shifts of its up and down phases. *E* meanwhile continued to mark ½-sec. intervals in time with the metronome, in dots when no after-image was present, in horizontal marks while it appeared in the down phase, and in vertical marks while it appeared in the up phase, until the after-image finally disappeared. In the first of these observations *O* was instructed to make every effort (no method being suggested) to keep the cube in the down phase during the observation of the card. The after-image however was observed without effort to control it. In the next observation she was instructed to keep the cube as much as possible in the up phase; the after-image was uncontrolled as before. In the next observation the instruction was to observe the cube without effort to control it, but to try and see the after-image in the down phase. The next observation was also made with no attempt to control the phase of the cube, but with the effort to see the after-image in the up phase. Finally two observations were made, in one of which the instruction was to hold the down phase throughout both original and after image, while in the other the up phase was to be held throughout.

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* Accepted for publication July 30, 1932.

Observations corresponding to these were made on rivalry. Upon a white card fitting an ordinary stereoscope a piece of standard red paper and a piece of standard blue paper of the same shape and size as the outline cube were pasted in positions so that they appeared to coincide when seen through the stereoscope. *O,* with her head in the rest, held the stereoscope to her eyes, which were closed while she waited *E*'s signal. *E* after giving *O* the signal to begin observing marked the half-seconds through the 2 min. interval, using horizontal marks when *O* said 'Red,' meaning that the figure looked red; vertical marks when *O* said 'Blue,' and dots when binocular color-mixture occurred and *O* said 'Purple.' When the 2 min. were over, at a signal from *E, O* closed her eyes and reported the appearances, disappearances, and colors of the after-images, *E* marking the ½-sec. intervals by vertical marks when the color was yellow and horizontal marks when it was green, and the half-seconds while no after-image was present, by dots. *O*'s instruction at the beginning of the first experiment on rivalry control was to hold the red phase in observing the card but make no effort to control the color of the after-image; in the second experiment to hold the blue phase but leave the after-image uncontrolled; in the third experiment to leave the observation of the card uncontrolled but try to see the after-image as green; in the fourth experiment to leave the observation of the card uncontrolled but try to see the after-image as yellow. In the next observation, as with the cube, the effort was to hold the blue phase in the original and the yellow phase in the after-image: in the last observation the red and green phases were to be held.

The method by which the success of these efforts at control was measured was as follows. As we have seen, there were two observations in which the effort was to hold the down phase of the cube while looking at the card. The average duration of the down phases under such down control was found. There were two observations in which the effort was to hold the up phase in the original. The average duration of the down phase under this up control was found, and the amount by which the duration of the down phases under down control exceeded the duration of the down phase under up control measured the efficiency of the control. This difference was expressed in terms of a percentage of the time of observation. The same method was used to measure efficiency of cube phase control in the cube after-image. In the rivalry experiments, the phases of binocular color-mixture giving purple had to be equally divided between the red and blue phases; otherwise the method of measuring control was the same as for the cube.

Results. The following results were obtained. *(1) Comparative controllability.* The average percentage of success in control was for the cube original 44.46; for the rivalry original 23.78, for the cube after-image 17.37, and for the rivalry after-image 13.07. The superiority of cube original control over rivalry original control was thoroughly reliable statistically, the difference being 5 times its standard error. Considering the results of individuals, we find that 83% of our *O*s controlled the cube original better than the rivalry original. The superiority of cube original control over cube after-image control also was statistically reliable, the difference being 3 times its standard error. The superiority of rivalry original control over rivalry after-image control was fairly reliable, the difference being such as to give between 97 and 98 out of 100 chances of a true difference greater than zero. There was no reliable difference between control of the cube after-image and of the rivalry after-image.

(2) Constancy of methods of control. It may be recalled that each *O* made two experiments where her task was to control the phases of the cube, and two where it was to control rivalry phases. The order of the *O*s in success of controlling the cube in the first observation corresponded with that in the second observation by a correlation of +0.67 ± .066; their order of success in controlling rivalry phases in the first observation corresponded with that in the second observation by a correlation of +0.66 ± .07. This indicates that most of the *O*s used approximately the same method of control in the second experiment as in the first.

(3) Individual differences. The standard error of the average cube original control for the group was 3.47; that of the average rivalry original control for the group was 2.37. The range of individual scores was for the cube original from 7.4% to 77%; for the rivalry original from —4.9% to 51.85%. This greater variability from one individual to another may be plausibly conjectured to be due to a greater number and variety of methods open for controlling the cube as compared with rivalry. The standard error of the average cube after-image control for the group was 8.5; that for rivalry after-image control was 4.9. The shorter duration of the after-image as compared with the two minutes' observation of the originals will account for the greater variability of after-image control measured in percentages of the total time; it was possible for after-image control to vary from plus 100% to minus 100% when the total duration of the image was short.

(4) Absence of a significant common factor. If these two cases of voluntary control had involved to any marked extent a common conative or will factor, there should have been a reliable positive correlation between an individual's success in cube control and her success in rivalry control. This correlation was, however, +0.07 ± .119, indicating that the operating causes were different for the cube and for rivalry.

(5) Monocular dominance. There is one factor present in retinal rivalry that is absent from cube phase fluctuations. This is monocular dominance. For certain individuals the field of one eye habitually predominates over that of the other eye, and this might conceivably make rivalry phase control more difficult than cube phase control. To test such a supposition the *O*s were arranged in order of the degree by which their control of the cube surpassed their control of rivalry, and also in order of their amount of monocular dominance, this being measured by the size of the difference between the total time one color was seen during all 3 of the *O*'s uncontrolled rivalry observations, plus one-half the total time during which she saw purple; and the total time the other color was seen, plus one-half the purple time as before. The correlation found between these arrays was —0.06; hence it appears that the difficulty in controlling rivalry does not arise from resistance offered to such control by ocular dominance.

Explanation of results. In order to investigate the causes of the phenomena just described, we must consider the processes which naturally accompany them. *O*s were given no instruction as to how to control what they perceived: we wished to avoid all external suggestions. They were not asked at the end of a control period to report introspections as to their methods of control: we had no great confidence that the methods of which they were most conscious were those actually most effective in their control processes, and we wished to avoid autosuggestion. After the entire investigation was finished we sent questionaries to our *O*s asking simply, "When you were asked to control the cube so as to prolong the up or down phases, what methods of control did you use? What methods did you use in the experiments with rivalry?" Twenty-five of our 35 *O*s answered these questions. Of course little dependence can be placed on their replies.

Our experimental results indicate that our *O*s used for control of the cube original phases some method or methods different from and much more effective than the methods they used for controlling rivalry original phases. Now there is one process accompanying observation of cube perspective that has obviously much less significance in the observation of rivalry changes. This is *actual movement of the eyes, shift of fixation.* It has been held that shifting fixation is the main factor in determining the change of perspective interpretation. The point one looks at appears nearer than when it is not directly looked at. If one moves one's eyes along an edge of the cube, that edge seems to come nearer. Our *O*s were at the outset of the experiments told to fixate a certain corner of the cube, but the instruction was not repeated with emphasis and probably could not have been followed in any case. It is only in control of the cube original that fixation or eye-movements can favor a particular phase. Obviously they cannot do so in rivalry or in any after-image, for the after-images move with the eyes. We have certainly here a factor capable of explaining the marked superiority of cube original control over other controls.

Our results indicate further that more methods are open for control of the cube original than for control of rivalry or after-images. One factor in cube control is apparent to ordinary introspection. *Movement ideas* are present during observation of cube fluctuations: the ideas of sitting down on the cube, of reaching down to handle it, of passing the hand over the surface in the "down" phase; or of standing under it and looking up, of reaching up to touch its under surface in the "up" phase. These movement ideas, as has now been experimentally demonstrated by Jacobson,[1] are accompanied by slight actual contractions of the muscles concerned. No movement ideas have any such tendency to occur in connection with rivalry.

The cube then has two methods which favor the prolongation of a specific phase and which are not available for rivalry control. Another motor process which occurs in the effort to control phases is *making eye movements,* especially *blinking,* when the undesired phase is present; this always tends to change the phase; conversely, *preventing eye-movements* helps to hold a phase. This was the method mentioned oftenest in the questionary answers. It is available for rivalry as well as for the cube.

Subvocal verbal reactions, saying to oneself 'up,' 'up,' or 'blue,' 'blue,' while trying to restore or maintain a given phase, undoubtedly occur and might be effective through the association of the movements they involve with the muscular contractions peculiar to the phase desired. Only 3 of the 25 *O*s who answered the questionary on methods of control mentioned such verbal reactions in connection with cube control; 2 of these reported them also in connection with rivalry control. But it seems probable that all the control in our experiments depended for its origin on verbal reactions, since it was derived from the *O*'s acceptance of verbal instructions.

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[1] E. Jacobson, Electrophysiology and mental activities, this JOURNAL, 44, 1932, 677-694.

General muscular tension, which is probably what our *O*s meant when they mentioned "concentration" as an aid, may quite conceivably help to maintain a desired phase against change by checking disturbing movements.

There are then 5 muscular processes that assist control of the cube: eye movements, the slight muscular contractions underlying movement ideas, avoidance of blinking when a desired phase is to be held and blinking to get rid of an undesired phase, verbal reactions, and general muscular tension. The first two of these, eye movements and movement ideas, are absent in rivalry control. Eye movements cannot be used for control of the cube after-image; blinking cannot be effectively used to change the phase of an after-image because it tends to make the after-image itself disappear. Thus it is clear that cube control has as its service more methods than has rivalry control; which would explain its greater variability in different individuals. Cube after-image control can use movement ideas, since one can think of reaching down, sitting down, and so forth when one sees the cube after-image in the down phase, but the tendency of the after-image to disappear at any full eye movement diminishes the efficacy of control. The same factor of instability of the after-image must explain the inferiority of control of the rivalry after-image to control of the rivalry original. And the fact that cube after-image control, though it has the factor of movement ideas at its service, is so little better than rivalry after-image control, can be explained by the inferiority in clearness and duration of the cube after-image as compared with the rivalry after-image. The white lines of the cube offered a weaker stimulus than the colored areas on the rivalry card.

These experiments throw no direct light on the cause of perspective and rivalry fluctuations. Since the control processes appear to be motor, like all other control processes, it would appear that the inevitable relaxing of control must be due to muscular fatigue, but why this relaxing should be periodic is another problem.

A motor psychologist would describe the whole process involved in control of these phenomena of visual perception as follows. There is present in the observer at the outset the drive of submission to the directions of the experimenter. Physiology cannot yet explain to us in terms of glandular secretions and hormones this social drive; it is certainly the opposite of the fighting attitude and may be supposed to involve low adrenal activity. At any rate it produces a condition of relaxed muscular tension; the *O* has no tendency to set up attitudes antagonistic to the movements, full or tentative, suggested by the words of the *E*.

The nature of these movements and the differences among those available for cube original control, rivalry original control, cube after-image control, and rivalry after-image control have been sufficiently described. By certain philosophers and psychologists the existence of mind as a dynamic entity capable of directly controlling its own processes has been postulated. The absence of any correlation between cube perspective control and rivalry phase control, and the marked superiority of that form of control which is accompanied by the most various motor processes, suggest strongly that in these phenomena no other causal agency operates except the familiar physical one of the interaction and interference of movements.

LXIII. STEREOSCOPIC BINOCULAR FUSION IN THE ORIGINAL IMPRESSION AND IN THE NEGATIVE AFTER-IMAGE

By M. F. WASHBURN and DOROTHY LUCILE SMITH

During some experiments reported in Number LXII of these Studies, where observations were made upon retinal rivalry of small colored fields and upon the ensuing negative after-images, it was noticed that while binocular mixture of the original red and blue fields, resulting in a purple field, often occurred, there was not a single report of binocular mixture of the green and yellow negative after-images. It seems remarkable that while with the eyes open the processes on corresponding points should fuse part of the time, this fusion should be almost if not quite absent on the part of the after-image processes on the same points. The difficulty of observing it in the faint and unstable after-image must of course be reckoned with. The problem suggested itself of finding whether the binocular fusion of dissimilar images which occurs in stereoscopic vision is also diminished in the negative after-image. The present paper describes an investigation of this problem.

In a dark room the *O* sat with her head in a rest, light coming from a 50-watt ground-glass bulb about 30 cm. above. The stereoscopic slide used was one which when looked at through the stereoscope appeared as a truncated cone with its smaller end directed towards the *O*. The right-hand design showed a larger circle with a small circle within it towards the left; the left-hand design showed the same larger circle with a similar small circle within it towards the right. The design was in black lines on a white ground; we experimented also with this design in white on a black ground, and with one showing a truncated pyramid in white on black and in black on white; but the black lines of the truncated cone gave us the clearest after-images.

In the experments where the stereoscopic slide itself was observed, *O* was directed, after placing the slide in the stereoscope, to adjust its position until it "looked like a cone projecting out toward" her; then while holding the stereoscope directed slightly downward to get the best light from above, and keeping her head fixed in the rest, she was instructed to fixate the small circle which represented the truncated end of the cone, and to watch for any shifts in its position. When they occurred she was to say, "Right" when the small circle looked displaced towards the right (dominance of the left-eye image), "Left" when the small circle looked displaced towards the left (dominance of the right-eye image), and "Center" when the small circle appeared to be in the center of the large one (binocular stereoscopic vision). *E* recorded the time of these changes by making short marks on paper in time with a metronome ticking at 120 per min.; when *O* said, "Left," *E* made the marks horizontal; when *O* said, "Right," *E* made vertical marks; when she said, "Center," horizontal marks one under another were made.

In the experiments where the negative after-image was observed, *O* looked steadily at the small circle for approximately 2 min., and then at a signal from *E* closed and covered her eyes and watched the after-image, reporting its appearances and disappearances and whether the small circle appeared displaced towards the right or left, or in the center. *E* recorded these changes as in the experiments where the original card was observed, using dots to mark the half-seconds when no after-image was present. In these experiments the period during which *O* looked through the stereoscope at the card was varied from one *O* to another, in order to find what exposure time gave the *O* clearest after-images: these times varied from one to five minutes.

In both the original and after-image experiments four observations were usually made by an *O* during an experimental period. There were 19 *O*s, all young women college students.

Results. The total time in seconds during which the small circle appeared in the center of the large one, showing that binocular vision was present, was found for observation of the original and of the after-image in the case of each *O*, and was expressed as a percent. of the total time during which the *O* observed the original in the one case and the after-image in the other. These percentages were as follows:

Observer	*Original*	*After-Image*
H	95.0%	.9%
T	90.5%	14.6%
A	66.0%	10.8%
R	85.2%	40.4%
N	100.0%	54.0%
M	88.0%	16.2%
B	68.0%	0.0%
G	87.0%	29.0%
F	47.8%	49.0%
L	99.5%	46.4%
S	77.0%	95.0%
Z	41.8%	17.6%
Ch	15.0%	38.0%
Ca	100.0%	20.9%
Se	88.6%	0.0%
Le	58.0%	0.0%
Ga	85.0%	56.5%
Oa	92.8%	35.0%
Oe	63.8%	90.0%

Thus for 15 of our 19 *O*s or 79%, binocular fusion occurred for a longer time in the original than in the negative after-image, by differences ranging from 94.1 to 24.2%, with the median at about 58%. The other 4 *O*s showed better fusion in the after-image than in the original, by differences ranging from 26.2 to 1.8%.

These figures so far as they go indicate that *the after-image of a stereoscopic impression shows decidedly less tendency to binocular fusion of the two dissimilar retinal images than does the original impression.* Yet the original images and the after-images occur in the same retinal regions. If binocular fusion is secured merely by having the images of an object fall on corresponding points in the two retinas, then it should occur as readily in the negative after-images as in the original images. It is impossible to avoid the conclusion that the situation in binocular vision is not so simple as has been supposed. May the fusion of unlike images into one in stereoscopy depend partly on *conditions which suggest unity of motor response,* for instance of the response of reaching towards and grasping the single solid object from which the images are caused? This response is lacking under the general conditions of after-image observation: the image appears to float in the air, either in the dark field of the closed eyes, unassociated in our experience with any successful grasping responses, or as a faint vision in the field of the open eyes, unreal by comparison with the surrounding objects.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

VI. COLOR PREFERENCES OF 1279 UNIVERSITY STUDENTS

By WM. E. WALTON, RUTH B. GUILFORD, and J. P. GUILFORD

In common with many other laboratories of psychology, the University of Nebraska laboratory gives in its elementary experimental course a study of color preferences using the method of paired comparisons. Since procedures have been devised for computing scale values in the method of paired comparisons, the experiment has taken on added significance. It is possible to show the student how his likes and dislikes can be measured scientifically. As in many elementary experiments where quantitative results are obtained, however, there are very few available norms with which to compare the results of a new subject or group of subjects. It is the main purpose of this study to supply such norms based upon a large number of subjects used under a variety of conditions. Since the Milton Bradley colored papers, which were used throughout these experiments, are rather generally used for studying color preferences, the norms should be of value to other laboratories, provided conditions other than the color stimuli used, are comparable. During the course of preparing the norms, certain sex differences, yearly variations, and other constant tendencies appeared, and so the study was broadened in scope in order to take into consideration these secondary problems.

The stimulus-material that was used throughout the 14 years during which our results were obtained, consisted of the best chromas of the 18 spectral hues in the Milton Bradley series. These colored papers were cut into 9 cm. squares and glued side by side in pairs, 2 cm. between squares, on gray cardboards 20 x 30 cm. The gray background had a dull finish with a brightness value of 5 on the Munsell color scheme. There were 153 such pairs of stimuli since each one was paired with every other one. The order of presentation of the pairs was the same as that worked out by Titchener.[1] Every color appeared equally often on the right and on the left. The stimulus-cards were set up in daylight illumination in front of a large gray cloth background. The *S*s were given the experiment in groups ranging from 5 to about 15 in number. All *S*s were seated as near to the stimulus-cards as space would permit. Every card was exposed for 5 sec. and *S* was instructed to record on a prepared blank which of the two colors (designated by a key) was more pleasing to him.

The results from these experiments had been carefully saved from the school year 1910-11 through the year 1920-21, and again from 1928-29 through 1930-31. From 1910-11 through the year 1918-19 they were given under the supervision of

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* Accepted for publication December 10, 1931.

[1] E. B. Titchener, *Experimental Psychology*, I, Pt. 1, 93.

Dr. H. K. Wolfe, through 1920-21 under the direction of Dr. Winifred Hyde Dodd, and during the last 3 years they were supervised by the senior author. While the actual experiments were conducted by laboratory assistants, and we are not prepared to insist that the conditions described above were always scrupulously carried out, we do know that for the last 6 years for which we have results, the assistants were carefully trained and instructed in the procedure of this experiment. There is no reason to believe that the earlier experiments were any less carefully supervised. All record sheets were discarded if the *S*s omitted any judgments or showed in any other way that they did not coöperate in the experiment. A number of such records had to be discarded every year. We have tested the reliabilities of the measurements for each year, and with few exceptions find them very acceptable. The coefficients of reliability are shown in Table II.

TABLE I

Showing the Affective Values of the Different Colors for the Men and Women Separately and for the Group Combined

Color	Affective values		
	464 men	815 women	1279 combined
VR	.128	.172	.150
R	.495	.602	.549
OR	−.040	−.457	−.249
RO	−.360	−.431	−.396
O	−.064	−.506	−.285
YO	−.545	−.460	−.503
OY	−.563	−.238	−.401
Y	−.207	−.192	−.200
GY	−.609	−.478	−.544
YG	−.307	−.221	−.264
G	.418	.151	.285
BG	−.089	.207	.059
GB	.194	.567	.381
B	.763	.304	.534
VB	.497	.405	.451
BV	−.142	.200	.029
V	.308	.437	.373
RV	−.190	.050	−.070

The method of computing scale values for the colors has been described elsewhere by the senior author.[2] The probable error of the normal cumulative frequency curve has been used as the unit of measurement instead of the standard error. Since the number of stimuli remained the same throughout the experiments, the scale values are comparable from one year to another.[3] The data from the men and women were treated separately and were also combined. The 756 scale values which were thus computed are not published, since the space required would not be justified by the importance of the single values. The affective values for a few of the colors showed distinct trends or changes during the course of years, and these trends are given in the form of curves (see Figs. 1 and 2). The norms that we undertook to find are listed in Table I.

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[2] J. P. Guilford, The method of paired comparisons as a psychometric method, *Psychol. Rev.*, 35, 1928, 499 f.

[3] Guilford, Some empirical tests of the method of paired comparisons, *J. Gen. Psychol.*, 5, 1931, 64-77.

The scale values for either sex during any one year usually ranged from about +1 P.E. to —1 P.E. When the various years are combined, these ranges decrease considerably, due principally to the fluctuation of the values from year to year. In Table I, the total range for men is 1.372, and for women it is 1.108. Elsewhere, the senior author has used the range, or better, the standard deviation of the whole group of scale values, as a measure of the homogeneity of the group of *S*s from which the judgments were obtained.[4] These results entirely justify that procedure. When the results of the two sexes are combined, as in the third column of Table I, a further reduction in range occurs, but not very much beyond the already narrow range for the women alone. The range for column three is 1.093 as compared with 1.108 for the women alone. Even when sex differences are eliminated, the differences in scale values do not vanish, which indicates that there is a common basis of feeling for different colors in this group of 1279 students. This group is surely large enough to be called a random sampling of the students in one part of the country. Some of the results, to be discussed in more detail later, indicate that the so-called culture patterns, fashions, fads, or other social factors may be the cause of wide differences in color preferences from one year to another and between the two sexes. Even when these factors are cancelled out, more or less, by the pooling of the results from nearly 1300 *S*s, there still remains sufficient agreement upon color preferences to indicate a basic, biological cause of likes and dislikes for colors.[5]

TABLE II

RELIABILITY COEFFICIENTS OF THE SCALE VALUES FOR THE DIFFERENT YEARS AND FOR THE TWO SEXES

Year	(N varies)				(N equals 100)	
	Men	(N)	Women	(N)	Men	Women
1910–11	.988	27	.578	18	.998	.862
1911–12	.876	30	.975	90	.980	.976
1912–13	.902	49	.978	85	.950	.982
1913–14	.850	36	.979	78	.951	.984
1914–15	.726	46	.900	90	.850	.909
1915–16	.929	60	.930	69	.946	.950
1916–17	.111	44	.845	115	.221	.825
1917–18	.360	13	.965	50	.834	.977
1918–19	.683	14	.954	50	.939	.976
1919–20	.958	68	.900	78	.970	.918
1920–21	.761	28	.618	27	.917	.857
1928–29	.380	6	.608	10	.910	.939
1929–30	.952	26	.888	32	.971	.959
1930–31	.854	17	.185	23	.943	.489
Means	.738	33.1	.807	58.2	.884	.900

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[4] Guilford, Racial preferences of a thousand American university students, *J. Soc. Psychol.*, 2, 1931, 179-204.

[5] Incidentally, it seems to be quite clear from Table I that the more familiar colors, like red, orange, yellow, green, blue, and violet, have slightly higher affective values than their less familiar neighbors in the spectral series. This is much more true of the men's values than of the women's.

We turn next to the question of the reliability of the measurements. This is important, in view of the somewhat uncertain conditions under which some of the results were obtained. It is of interest, also, to compare the self-consistency of groups of men and women *S*s, and the consistency of the results from one year with those from other years. To this end we have found the reliability coefficients for each sex for each year. This was done by splitting the data into two halves, the *S*s being divided at random, computing two sets of scale values, correlating the two sets of values, and applying the Spearman-Brown formula. This application of the Spearman-Brown formula has been tested empirically for similar types of data and it has been found to work with perfect satisfaction.[6] Since the reliability of any such set of measurements increases as a rule with the number of judges, and since the number of *S*s, both men and women, varied considerably from year to year, the reliability coefficients alone do not give us a fair comparison between the different years and the two sexes. We have taken the liberty to extend the use of the Spearman-Brown formula still further, and to predict for each set of values the reliability coefficient which would have obtained if there had been exactly 100 *S*s in each group of judges. The results, both the reliability coefficients for varying numbers of *S*s and those estimated on the basis of 100 *S*s, year by year, are presented in Table II.

The measurements can be considered very reliable since the coefficients are above 0.90 in 5 of the years for the men, and in 8 of the years for the women. They are totally unreliable in at least 3 years for the men, and in 1 year for the women. This unreliability is largely attributable to the small numbers of *S*s for most of those years, for when the Spearman-Brown formula is applied to estimate the reliability for 100 similar judges, the coefficients are nearly all in the 90's. Only one year has wholly unreliable measurements for the men and for the women. For the former, this is the year 1916-17; for the latter it is 1930-31. Since the original coefficients upon which these estimates are made are low and therefore have large probable errors, this may be due to chance. Therefore no interpretations will be attempted. When one compares the means of the coefficients of reliability, taken without regard to the number of *S*s, it would seem that the women tend to give more reliable measurements, that is to say, they agree with one another better. But when the number of *S*s is held constant, as in the last two columns of Table II, the averages are approximately equal. One might expect that women would show less agreement among themselves and therefore have less reliable measurements, as is indicated by their narrower range of scale values. The narrower range may after all be a better indicator of individual differences in preferences, whereas the reliability coefficient may indicate the adequacy of the sampling of the whole group. This would mean that although there are more personal, individual tastes among the women, it is just as easy to get a representative group of them as it is of the men by taking a random sampling of the same number. The indications in Table II lead to the conclusion that in order to secure reliable affective values for any relatively homogeneous group of *S*s, one should use not less than 50 *S*s chosen at random.

Will men, or women, disagree more among themselves from one year to another than they do within the same year? In order to obtain a rough answer to this question, we found the means of the inter-yearly correlations of scale values. By means of Kelly's formula[7] it is possible to find the mean intercorrelation between sets of observations without finding all the separate coefficients. We have made use of this procedure, first finding *rho* and then translating this into the Pearson r. For the men, the mean of the inter-yearly correlations was 0.802, and for the women it was 0.681. If these are compared with the means of the reliability coefficients in Table II, an interesting conclusion is suggested. Those two means are 0.738 and 0.807, respectively. The scale values for the men correlate just as well from one year to another, on the average, as they correlate with themselves in the same year. For the women, however, there is a drop of about 0.13 between the mean reliability and the mean inter-yearly correlation. If these estimates are correct, it means that women are more inclined to shift in color preferences from year to year, and that they are inclined to 'follow the herd' in doing so. This fact was also verified by Walton and Morrison in a study with colored lights.[8]

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[6] The empirical proof for this has not yet been published.

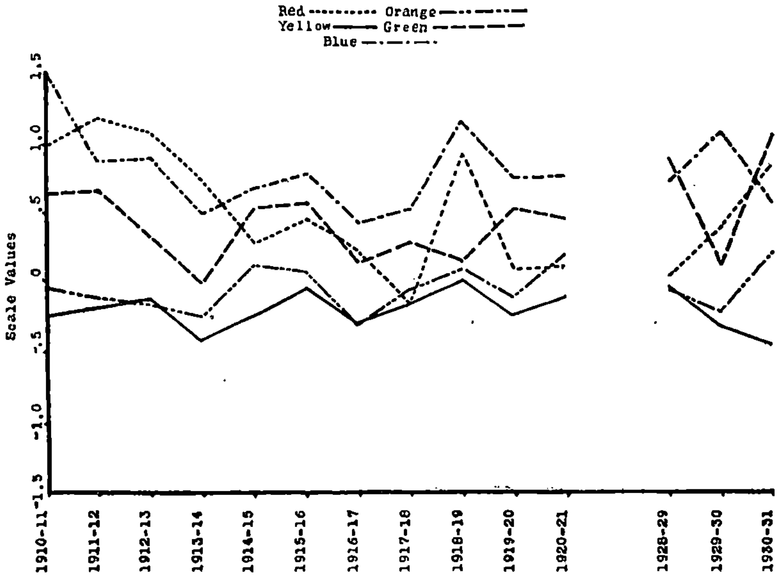

FIG. 1. SCALE VALUES OF FIVE COLORS FOR 464 MEN FROM 1910 TO 1931

What can be said as to the reliability of the norms that have been set up in Table I? Applying the Spearman-Brown formula again, we have predicted the reliability coefficients for all the men (464) and for all the women (815). The coefficients are 0.972 and 0.987 respectively. The second value might need to be slightly reduced, due to the yearly fluctuations in color preference of the women. The index of reliability, or the correlation between the actual measurements and the true thing that we are trying to measure, in this case affective values for colors, under the conditions which have prevailed during these fourteen years of experimentation, is equivalent to the square root of the coefficient of reliability. In these two cases the indices of reliability would be 0.986 and 0.993 respectively.

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[7] T. L. Kelley, *Statistical Methods*, 1924, 218.

[8] W. E. Walton and B. Morrison, A preliminary study of the affective values of colored lights, *J. Appl. Psychol.*, 15, 1931, 298.

Earlier in this report we mentioned distinct trends or alterations in affective values for single colors. The most decided of these, perhaps, was in the case of red. Beginning near the top of the list for agreeableness for both men and women in 1910-11, it declined in relative value almost continuously, until a minimum value was reached in the year 1917-18. A sharp rebound from this minimum occurred in the year immediately following and lasting for the one year only. When next we find the value of red in 1928-29 and the two years following, it is climbing back to a higher value. The agreement is so close between the two sexes in this respect, that one is tempted to look for some underlying reason. The regular decline and recovery of red could hardly be due to chance. Such irregularities in these results indicate the importance of continuing a study of color preferences and the like over a period of years and under varying conditions. Only in that way can the factors controlling color preferences be isolated and discovered.

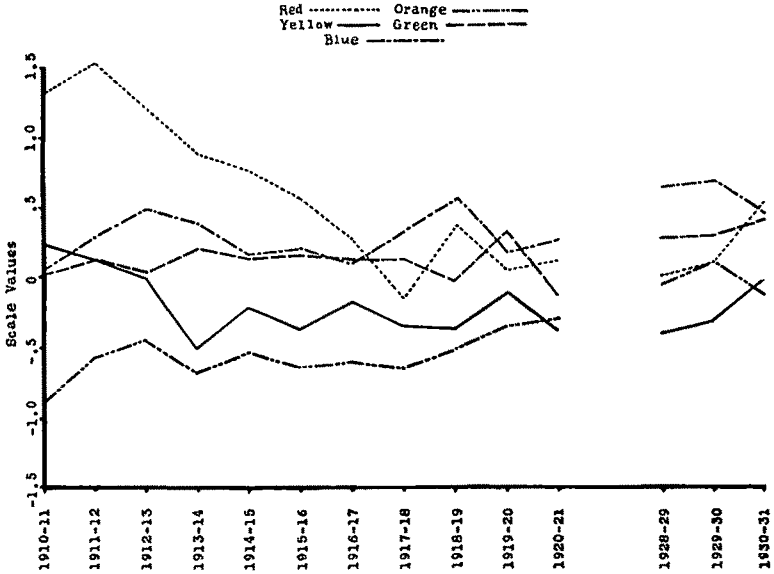

FIG. 2. SCALE VALUES OF FIVE COLORS FOR 815 WOMEN FROM 1910 TO 1931

Figs. 1 and 2 show the yearly fluctuations in several of the principal colors: red, blue, green, orange, and yellow. One of the persistent sex differences is the greater value of orange as compared with yellow for the men and the reverse relative evaluation of those two colors by the women. The two sexes, on the other hand, agree persistently in the relative values of blue and green. Although not all the 18 colors are represented in the curves, some idea can be gained concerning the shift in total range of values from year to year. When all 18 colors are considered, the following rather striking shifts in total range appear. For the men, the maximum range occurs in the first two years. The range then progressively narrows down to a minimum in the year 1916-17. From that year it again widens to a sub-maximal degree and remains fairly constant until the end. For the women, also, there is a maximal range at the start, and a progressive narrowing that does not reach a minimal point until the year 1920-21. The ranges for the last three years are relatively wide again. These shifts in range are seemingly unrelated to numbers of *S*s in the groups, and only slightly related to variations in coefficients of reliability. If the assumption is correct, that the total range measures the homogeneity of opinion in the group, one has some indication of trends in 'individualism' as expressed in color preferences in Figs. 1 and 2. Fluctuations from year to year such as we have just been discussing could hardly be due to gross alterations in experimental conditions. Chance alterations in experimental conditions could hardly produce continuous trends that persist over a period of years. We would again insist upon the importance of follow-up studies with accurately kept records from year to year. Only in this way can some of psychology's most persistent problems be solved.

Summary and Conclusions

Results from experiments upon color preferences had been accumulated over a period of 14 years. The 18 Milton Bradley colored papers were used as stimuli with a total of 464 men and 825 women *S*s. The following conclusions are indicated in the results.

(1) Very reliable norms, as tested by self-correlations, were established for the affective values of these 18 colors under varying conditions.

(2) Certain differences between the two sexes in color preference persist rather consistently from one year to another. For example, men usually prefer orange to yellow, but for women the preference is reversed.

(3) Certain colors showed a continuous shift in affective value during the course of years for both sexes. For example, red decreased continuously in value from 1910 to 1918, its value was uncertain for a time, and it appears now to be rising to its former high position on the scale.

(4) Women's color preferences are somewhat more inclined to fluctuate from one year to another than are the men's, but individual differences in color preferences within any one year are about equal for the two sexes.

(5) Persistent differences in affective values for colors, in spite of fluctuations from year to year and in spite of sex differences indicate that, in addition to social conditioning and other environmental factors, there are undoubtedly deep underlying biological factors that help to determine color preferences.

APPARATUS

A DEVICE FOR VARYING AND CONTROLLING THE ENTRANCE PUPIL

By C. E. Ferree and Gertrude Rand, Johns Hopkins University

The description of the device to be presented will be prefaced by a brief discussion of the factors which influence the size of the natural pupil and of the effects of size of pupil on the formation of the image and on the visual response.

Among the factors which are known to affect the size of the pupil the following may be mentioned: age, errors of refraction, distance of object, intensity of light, area and location of luminous surface in the field of view, drugs, other sensory stimuli, mental and emotional states, and pathologic disturbances. All of these factors are interacting in their influence. The effect of any one is modified by the presence of the others. Any statements that are made with reference to factors have, therefore, to be made in a very general way.

(1) Age. At birth the pupils are very small. They become larger during childhood and still larger during the period of adolescence. From adolescence to middle age they are of medium size and in old age they again become smaller. The causes which are assigned for these changes are largely a matter of conjecture. For example, the dilator muscles of the iris are under the influence of the sympathetic nervous system. In early childhood these muscles are inactive because of the generally undeveloped state of the nervous system. During the period of adolescence, however, their activity is greatly increased, due probably to overstimulation by an unstable and highly sensitive sympathetic system. In middle age a better balance is established between the action of the sphincter and dilator muscles and the pupil becomes of medium size. But in old age the dilator muscles again become inactive and the pupil returns to the smaller sizes found in early childhood. In addition to changes of innervation there are also present in old age, according to various writers, a loss of pliability of the iris due to vascular sclerosis, a thickening of connective tissue behind the sphincter muscle and the presence of hyalin connective tissue in the border of the pupil. All of these would tend to prevent dilatation. However, whatever the cause, as a partial compensation for the loss of accommodation, the hyperopia and other defects in focussing which come with advancing age, the small pupils of old age serve a very important purpose in the economy of vision.

(2) Errors of refraction. Myopes, particularly young myopes, have in general larger and hyperopes smaller pupils than emmetropes. This may be due in part to an associative relation with the function of accommodation, the myopes requiring less and the hyperopes more accommodation than emmetropes.

(3) The adjustment of the eye for distance. As the object approaches the eye,

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the pupil contracts. This is a valuable feature in the accommodative process for change of distance, and adds greatly to the clearness and speed of seeing when the eye is compelled to change its adjustment from far to near. In making this adjustment the lens may perhaps change too much or too little to give the best focus, but the change in the pupil is always in the direction of clearer imagery. To clean up after the lens in the process of accommodating for distance is a very important function of the pupil. The pupil as an aid to clear seeing is especially needed as the near point of vision is approached. The distance of the near point is strongly affected by size of pupil. This is true more particularly in middle age and in early presbyopia.

(4) Intensity of light. The reaction to intensity of light is strongly influenced by the presence of other factors. The most important of these from the standpoint of experimental control are perhaps size and distance of illuminated area or surface and its location in the field of vision. With a field of view bounded by surfaces of nearly uniform coefficient of reflection of the order of 75-78 percent we found in a study of 6 observers ranging between 20 and 30 years of age, that the knee of the curve for the reaction to intensity fell between 10 and 20 foot-candles. Below these values the curve was steep; above them the change in the slope of the curve was comparatively slight.[1]

(5) Area and location of luminous surface in the field of view. Area of field as well as the intensity of its illumination exerts an important effect on the size of the pupil.[2] The effect, however, is not so great for change of size as for change of intensity. Also for the range of size and intensities which we used there is no pronounced knee of the curve for size. The location of the illuminated surface in the field is a further important factor. Hess, for example, has said that the pupillomotor fibers are all confined to the macular region of the retina and that the stimulation of peripherally located fibers has no effect on the size of the pupil.[3] Our results on the influence of size of illuminated field show, however, that there is a marked effect of increase in size far beyond the limits of projection of the macular area into the field and that the effect is greater through that part of the retina which has increased sensitivity to light, at least in dark adaptation, than it is through the more centrally located portions of the retina.

(6) Drugs. With reference to effect on size of pupil, drugs may be divided into two classes: miotics, which produce a contraction of the pupil and mydriatics which cause the pupil to expand. Among the most commonly used drugs of the former class are eserin, pilocarpin and muscarin; among the latter, atropin, homatropin, belladonna, cocaine, adrenalin, scopolamin, hyoscomin, duboisin, etc. Unfortunately for experimental purposes, also unfortunately in some cases for use

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[1] C. E. Ferree and G. Rand. Relation of size of pupil to intensity of light and speed of vision, and other studies, *J. Exper. Psychol.,* 15, 1932, 37-55.

[2] Ferree, Rand, and E. A. Harris, Intensity of light and area of illuminated field as interacting factors in size of field, *ibid.,* (in press).

[3] C. Hess, Untersuchungen über die Ausdehnung des pupillomotorisch wirksamen Bezirkes des Netzhaut und die pupillomotorischen Aufnahmeorgane, *Arch. f. Augenhk.,* 58, 1907, 182-205; Investigations concerning the extent of the active pupillomotor area of the retina and concerning the pupillo-motor receiving organs, *Arch. Ophth.,* 38, 1909, 131-146; Das Differential-Pupilloskop: Eine Methode zur messenden Bestimmung von Störungen des Pupillenspieles. *Arch. f. Augenhk.,* 80, 1916, 213-228.

in treatment, these drugs do not affect the pupil alone. Pilocarpin, for example, in non-presbyopic eyes produces for a considerable time a well pronounced myopia and atropin and homatropin produce cycloplegia as well as mydriasis. The action of some of the mydriatics on accommodation, however, is slight.

(7) Other sensory stimuli and emotional states. Size of pupil is affected by sensory stimuli other than light. The tendency of these stimuli is in general to dilate the pupil. As an effect of the various mental and emotional states both contraction and dilatation may occur. In sleep in spite of the closed lids and dark adaptation the pupil is contracted. In light and medium general anaesthesia the pupil is contracted and immobile. In dangerous anaesthesia it is dilated and fixed. It is also dilated in epilepsy and convulsions and in coma and in death.

(8) Pathologic disturbances. Among the classes of effects in the reaction of the pupil that are produced by pathologic conditions may be mentioned spastic and paralytic miosis and mydriasis, anisocoria (unequal pupils), hippus (rapid fluctuation in breadth of pupil), the Argyll-Robertson pupil (absence of light reflex with retention of reaction to distance), and cyclic contractions and dilatations. While the reactions of the pupil in general constitute a most important and voluminous chapter in the diagnosis of nervous diseases, there seems to be no disease which can be diagnosed from a pupil sign alone. There is, however, some controversy on this point.

The influence of size of pupil on the visual response may be briefly summarized as follows: (1) There is an effect on the intensity of the image formed. The amount of light condensed into the image should be in direct proportion to the square of the breadth of the pupil. This applies not only to the light from the object or stimulus but also from the surrounding field, and thus affects the state of adaptation and sensitivity of the entire retina and both directly and indirectly the response of any given part of the retina.

(2) There is a very great effect also on the clearness of the image. This effect is produced in the following ways: (a) The breadth of the cross section of the cone of light from each point in the object is changed. As it is decreased, for example, the blurring of the image caused by the presence of diffusion circles is decreased. Always an important factor in the sharpness of the image formed, this effect becomes especially pronounced when there is a fault in the focussing action of the refracting system. As will be shown in a forthcoming paper, pupils of the order of 1-1.25 mm. in breadth render the eye practically independent of errors of refraction, also practically independent of changes in accommodation. These are very important incentives for the use of an artificial pupil in many types of experimental work. The facts also have many other practical applications.

(b) The proportion of light diffracted near the edges of the pupil to the total light and the breadth of the diffraction patterns in the image, central disk, and concentric rings, are also changed. This effect tends to counteract the beneficial action of the contraction of the pupil noted above. It does not, however, become of considerable consequence until unusually small sizes of pupil are reached. In recent determinations we have found, for example, that the values which have been assigned to the optimum breadth of pupil are much too high. The error in former determinations seems to have been due in large measure to the use of an artificial pupil too far in front of the eye.

(c) The aberration effects and other irregularities of focussing resulting from the use of the periphery of lens and cornea are excluded by pupils of small and medium size. The importance of these effects, while always great, varies considerably even in normal eyes. The spherical aberration ordinarily found in a refractive system as strong as is present in the eye is enormous. While the flattening of the lens and the decrease in the refractive index towards the periphery act as a corrective, there still remains a considerable amount of this defect in all eyes. Also with advancing age changes in density occur which are unequally distributed throughout the lens. Thus by decreasing the size of the diffusion circles in the image and by limiting the aperture to the most favorable portions of the refractive system the pupil acts as a final correction on all defects in focussing.

(3) Reduction in the size of the pupil may further exert a secondary effect on the clearness of the image. That is, in passing to the retina a certain percentage of light, variable from eye to eye, is scattered by internal reflections within the media. This scattered or unfocussed light tends to blur or confuse the image and when present in sufficient amount causes the phenomenon known as veiling glare. By reducing the area of the surface of the cornea acted upon, the cross section of the beam throughout and the variety of angles at which the rays of light may strike the bounding surfaces of the media and the various reflecting particles and surfaces which are to be found in the media, a small pupil over and above the effect on the amount or intensity of light reaching the retina may serve to reduce the overlay of scattered light in the image. In any event the use of such an aperture before an eye that would be classed as emmetropic noticeably clears up vision under light of moderate and high intensities in spite of the reduction in the intensity of the useful light in the image. This may not be due entirely to an improvement in the focussing.

An interesting objective demonstration of the effect of size of pupil on clearness of image for lecture and class room use can be made with a projectoscope, a lantern slide of a test letter such as the broken circle, and a plate or stop with an aperture of suitable size. An iris diaphragm is convenient for this purpose inasmuch as with it the effect of different amounts of reduction in size can be shown in series. Two cases may be made of the demonstration: when the focus is correct, or approximately so and when the image is so badly blurred as to be entirely formless and unrecognizable. In the former case some improvement in the clearness of the image may be noticed and in the latter the image can be cleared up with even a moderate reduction in size of the lenticular aperture. A very good image can be obtained with a small aperture even when the focus is so poor that the effect is merely a fairly uniform patch of projected light.

When an artificial pupil is employed, it and not the natural pupil should, of course, be treated as the entrance pupil. Unfortunately it has to be located in front of the refracting system and is usually placed much too far in front to serve well the purpose for which it is intended.

Two important uses for artificial pupils may be noted: (1) in a general examination and rating of eyes; and (2) in experimental work for various purposes of variation and control.

(1) The examination of the eye is usually begun with a test for acuity. Low acuity may be due to several causes such as refractive defects, low coefficient of

transmission of the media of the eye and defects in the sensorium. It is of very great advantage to be able to know at the beginning of the examination of an eye with low acuity whether or not the first of these factors plays an important rôle in the reduced vision. It has long been known that an artificial pupil renders important service in this connection. However an artificial pupil which fits into a spectacle or trial frame is too far removed from the natural pupil to give maximum service. Also we believe the apertures ordinarily used are too large to be of maximum benefit in forming the image. It is truly amazing what an aperture of optimum size and distance from the cornea will do in the way of rendering vision independent of defects in the refractive system.

(2) The uses for an artificial pupil in the laboratory are too well known and too varied to merit noting here. It may be worth while to conjecture, however, that much of the work that has been done with an artificial pupil would have given quite different results had an artificial pupil which more closely approximated the position of the natural pupil been used. Some of it, we have found, can well afford to be repeated under better conditions of use of pupil.

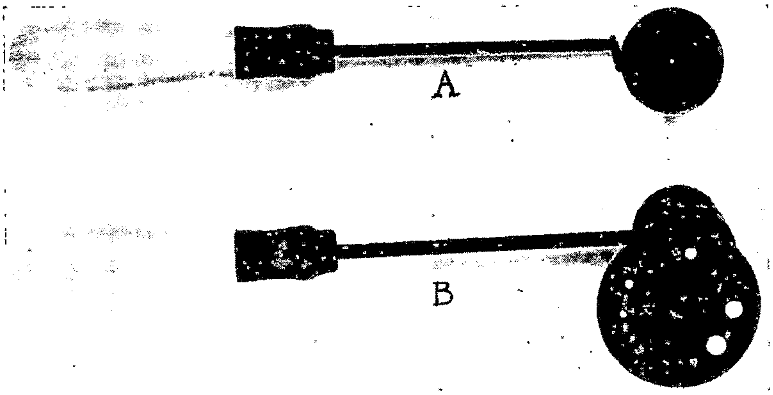

FIG. 1. MODELS OF THE DEVICE FOR CONTROLLING THE ENTRANCE PUPIL
(A) Model with single artificial pupil.
(B) Model with 10 variable pupils which range in breadth from 0.5–6 mm.

The device to be described has the following features and advantages. (1) When in position the pupil is in almost direct contact with the cornea. (2) It is mounted in an eyepiece which can quickly and easily be fitted to the eye. (3) The eyepiece is mounted on a handle for convenience of placing and holding in front of the eye. (4) Ten sizes of pupil are provided ranging in breadth from 0.5 mm. to 6.0 mm. (5) A change in size can be quickly and conveniently made by rotating a dial, without removing the device from in front of the eye.

A picture of the device is given in Fig. 1. The eyepiece is in the form of a tube 26 mm. in diam., 28 mm. long. The diaphragm forming the front of this eyepiece is slightly concaved so as closely to fit the anterior surface of the eye. At the center of this diaphragm is a circular aperture 12 mm. in diam. Mounted in a vertical plane directly behind this aperture is the disk containing 10 artificial pupils. It turns about an axle which is parallel to the axis of the eyepiece. This axle is carried by a broad radial arm which is fixedly attached to the tube of the eyepiece by a narrow rim or collar. The tube of the eyepiece telescopes into a second section of tubing or broad collar 20 mm. long, to which the handle of the instrument is fastened. The disk can thus be rotated at will about the axis of the eyepiece. When in position for use, it is turned to the temporal side of the eye examined. In this position, there is no interference with the nose, brow, or other parts of the face.

The 10 pupils are arranged in the form of a circle at a suitable distance from the margin of the disk. They are equally spaced and their centers are at a distance of 16.5 mm. from the center of the axle. Their diameters are respectively 0.5, 1, 1.5, 2, 2.5, 3, 3.5, 4, 5, and 6 mm. When in position in front of the eye, the center of each pupil coincides exactly with the center of the aperture in the diaphragm of the eyepiece. In order that each pupil shall automatically take this position in turn when the disk is rotated, 10 stops are provided. These stops are constructed as follows. The back wall of the disk contains 10 very small, shallow cups, suitably spaced on a circle with a radius of 20 mm., 3.5 mm. greater than the radius of the circle containing the centers of the 10 pupils. In a mount at the center of the radial arm which carries the disk and in exact radial relation with the axis of rotation of the disk and the axis of the tube of the eyepiece, a small ball is sunk against a spring to give tension, at the proper level to engage the cups as they pass in rotation. The positioning of the ball and the spacing of the cups has, of course, to be made with great accuracy and precision, if the center of each pupil is to come to rest at the exact center of the aperture in the diaphragm of the eyepiece. For convenience of rotation the disk is provided with a milled edge, and to permit of smoothness and freedom of movement in turning, the circle containing the cups is grooved. With the provisions made for convenience and accuracy of adjustment, the pupils can be quickly and easily changed either by the patient or the examiner without removing the instrument from in front of the eye.

In order to eliminate reflections and scattered light, the inside of the tube is painted a flat black. All the metal parts of the device are also suitably blackened. Near the farther end of the broad collar around the tube of the eyepiece at a convenient distance from the eye, the handle of the instrument is inserted. This distance is made sufficiently great so that the handle, when held vertically, does not interfere with the cheek or other parts of the face. When wanted for use in a fixed position in a laboratory set-up, the handle can be detached and the tube be held in a suitable mounting as desired.

There is also shown in Fig. 1 a more simple device for the use of an artificial pupil of a selected fixed size. In this case the aperture of the size desired is cut at the center of the concave diaphragm of the eyepiece. In the device shown the aperture is 1.16 mm. in diameter. In a later paper results will be given pertaining to the optimum size of aperture both for emmetropic and ametropic eyes for different intensities of illumination.

A NEW METHOD FOR REGISTERING TIME-INTERVALS

By A. Fleisch, University of Lausanne, Switzerland

Although the necessity for measuring time-intervals and frequencies arises often in experimental psychology, the methods so far available for this purpose appear primitive and hardly reliable for experiments of a prolonged duration. For many investigations in biology and psychology it would be highly valuable to have an apparatus which should record each time-interval between any two of a long series of phenomena immediately by means of a vertical line. This desired result may be obtained from an apparatus recently described by the writer[1] as a *pulse time recorder*. By its aid it is possible to record the pulse intervals (the time-intervals between two pulse beats) immediately as two successive lines. However, this apparatus may also be used for recording the frequencies of other phenomena and of any other time-intervals.

This apparatus[2] can be used for (a) the continuous registration of pulse-times, *i.e.* the reciprocal value of the pulse frequency, (b) the registering of the duration of respiration and of the duration of inspiration and expiration, (c) experiments connected with the physiology of work, and (d) various uses of the time-recorder.

This pulse time-recorder[3] works as follows. (See Fig. 1.) The rotation of an invariable motor, EM, is transmitted to an axle, W_1, and from this by means of a system of cog-wheels, Z, producing a reduction of speed, to an axle, W_4, to which is attached a rotating magnet, RM. When the magnet is magnetized, the iron disk, S, rotates and winds up the string, Sa, and the slider, Sch, which marks a vertical line on a blackened drum. As soon as the time-interval to be registered is ended by the occurrence of the next phenomenon, *e.g.* the next pulse-beat, the circuit in the rotating magnet, RM, is opened for a short but constant period of time, the slider, Sch, is quickly drawn down by a spring, F_1 and the recording of the next interval sets in immediately. This interruption of the current by the rotating magnet, RM, is effected by means of the pulse magnet, PM, and the steering disk, St.

Each time-interval is recorded with a deviation of only ± 0.004 sec. By a simple shifting of the buttons K_1, K_2, K_3, seven different speeds may be employed with 1 mm. of the recorded line corresponding to 0.01, 0.02, 0.05, 0.1, 0.2, 0.5, or 1.0 sec., respectively. Altogether, this time-recorder may thus register intervals from 0.2 to 138 sec. by means of the corresponding length of the recorded line.

(a) The registration of pulse-intervals. In order to record the intervals between two pulse-beats by means of the apparatus described above, a pulse-pelotte[3] is required which closes the circut for each pulse-beat to cause the pulse magnet, PM

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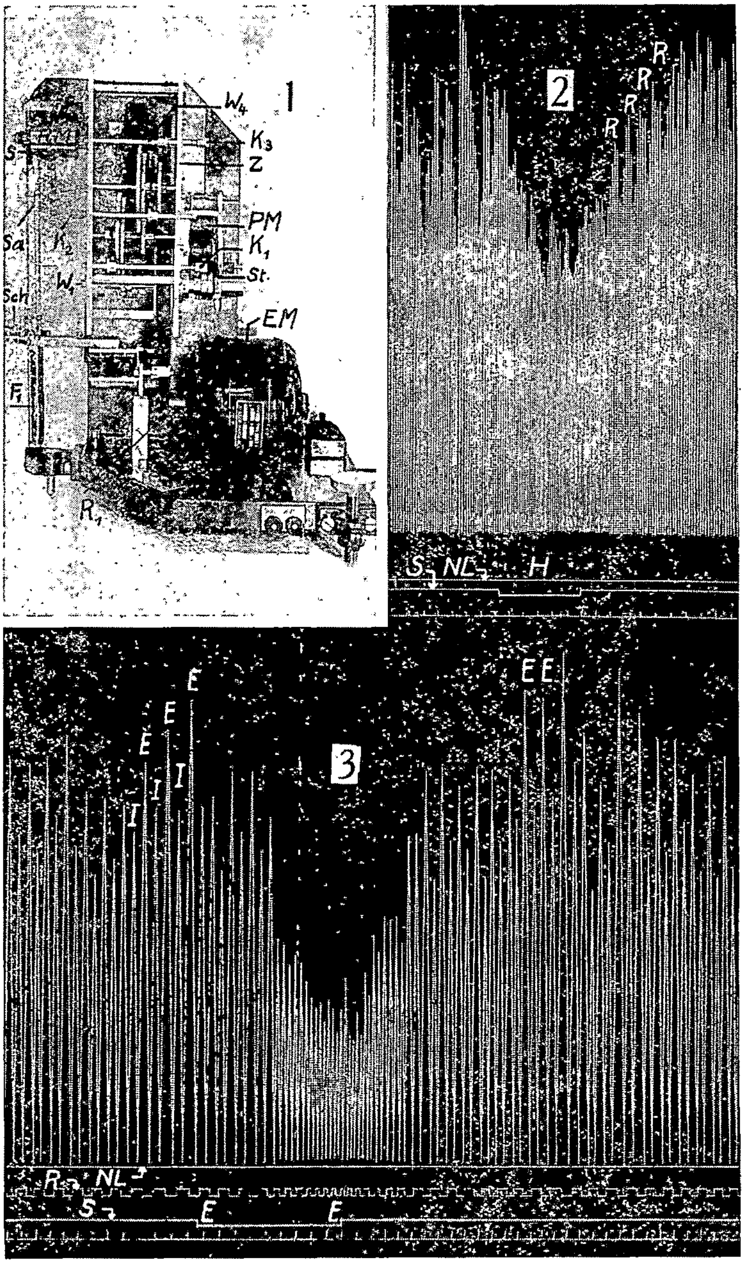
[1] A. Fleisch, Der Pulszeitschreiber: Ein Apparat zur Aufzeichnung der zeitlichen Pulsintervalle als Ordinate, *Zsch. f. exper. Med.*, 72, 1930, 384 ff. See also *idem*, Die Registierung zeitlicher Intervalle direkt als Ordinate mit dem Pulszeitschreiber, Abderhalden's *Handbuch der biologischen Arbeitsmethoden*, Abt. V, Teil 8, 905.

[2] A more detailed description will be found in Abderhalden's *Handbuch, loc. cit.*, which will be published shortly.

[3] The pulse-time recorder and the pulse-pelotte may be obtained from the mechanician of the writer's Physiological Institute.



FIGS. 1-3. See opposite page for legends.

in Fig. 1, to operate. By means of the pulse-pelotte[4] it is easy to record thousands of consecutive pulse-intervals without disturbance, and, given a good pulse, it hardly ever occurs that beats are registered twice or left unrecorded. As the transmission from the pelotte to the pulse-time recorder is effected by means of an electric current, the time-recorder may be placed in a room other than that of the *S*.

The way in which the results of the pulse-time recorder are presented is illustrated by the pulse-interval curve in Fig. 2, which represents the pulse-intervals of a healthy person free from mental or physical strain. Under such conditions the frequency of the pulse is never the same but is subject to constant rhythmic variations. There is always present a respiratory arhythmia of the pulse; synchronously with the respiration the pulse-intervals become either longer or shorter. In addition, certain more extensive variations become visible. At point H, the *S* was put in a pleasant state which at once caused the intervals to become shorter, and increased the frequency. When the excitement was over, the pulse-intervals again assumed their former lengths.

This peculiar arrangement of the pulse-time recorder further registers all extra-systoles of the heart; the shape of the irregularity renders possible conclusions as to the place of origin of the extra-systoles.[5]

(b) The registration of the duration of respiration. By the aid of this pulse-time recorder the length of the individual respirations may be registered directly in the form of an ordinate. For this purpose, one requires an additional contrivance which closes an electric circuit at the very moment of the breathing phase, *i.e.* at the beginning of inspiration. The contrivance used is simple, easy to work and reliable. A belt of an ordinary pneumograph is put around the chest of the *S*, and air is sucked into the pneumograph at the inspiration and expelled at the expiration. These movements of the air in the pneumograph are transferred to a Marey tambour, whose metal recording lever is lowered at the inspiration and by dipping into a vessel of mercury closes the electric circuit. It is necessary that the tambour should be in communication with the atmosphere by means of a T-pipe, so that the air pressure in the tambour and in the pneumograph may always

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[4] For a full description, see the references of note 1.

[5] Fleisch, Über das Verhalten des Pulsfrequenz bei seelischer Erregung, registriert mit einen neuen Zeitordinatenschreiber, *Arch. f. Physiol.*, to appear shortly; Fleisch, E. Masing, and R. Beckmann, Die Erkennung von Pulsarrhythmien mittels des Pulszeitschreibers, *Zsch. f. klin. Med.*, 119, 1932, 618 ff.

FIG. 1. PHOTOGRAPH OF THE PULSE-TIME RECORDER. (See opposite page.)

FIG. 2. The time intervals between two pulse beats each have been recorded as vertical lines. The rises of the line are to be measured as from the base line, NL. 1 mm. of the ordinate = 0.01 sec. Time-marks, t = 6 sec., while the signal line, S, curves downwards during H, the *S* is put in a state of happiness. RR represents the rhythmic variations of the pulse times due to respiratory arrhythmia.

FIG. 3. Direct registration of the duration of inspiration, I, and of Expiration, E, as an ordinate by means of the time-recorder. 1 mm. of the ordinate = 0.05 sec. The ordinates to be measured as from the base line, NL. While the signal line, S, curves downwards at E-E, the *S* is in expectation of a disagreeable event. Time-marks, t = 6 sec. The line, R, curves upwards while the duration of expiration is being recorded.

adjust itself to the atmospheric pressure, and that the lever may always return into its normal position.

It is also possible separately to register the duration of inspiration and of expiration. For this purpose a two-armed aluminum lever moving up and down with the breathing like the beam of a balance is attached to the Marey tambour. With the down-movement the ends dip into separate vessels filled with mercury, and thus close the electric circuits required to operate the time recorder. Here, too, a free communication of the Marey tambour with the atmosphere is necessary, so that the beam may not move proportionately to the volume of the air used in breathing, but rather proportionately to the 1st differential quotient of the volume of air, *i.e.* to the speed of the respirated air. With such an arrangement electric contacts are obtained at the beginning of both inspiration and of expiration.

Fig. 3 has been obtained by the method just described. It shows the duration of inspiration and the duration of expiration separately. The longer vertical lines, E, represent the duration of expiration, the lines I the inspiration. In order to avoid any confusion of inspiration with expiration, the signal line, R, provides an additional record of the phases of breathing. The upward movement of R marks expiration, its downward movement denotes inspiration. During E-E the *S* is warned of a disagreeable event; in consequence, the duration of respiration becomes shorter. Furthermore, the relation of the duration of the inspiration to that of the expiration is changed; the relation of these two phases becomes equal instead of 1:1.2 as before.

(c) Application of the pulse-time recorder in the physiology of work. If it is desired to determine a single interval of time, like the time of reaction, a chronoscope may be employed. The latter becomes useless, however, as soon as it is necessary to record, without interruption, a series of time-intervals following immediately one upon the other. For such a purpose, the pulse-time recorder is an ideal apparatus. Thus it may be employed in the physiology of work to record, uninterruptedly, the duration of rhythmic processes of work. When the worker has finished a piece, he puts it down in a special place, thereby closing the electric contact required for the operation of the pulse-time recorder. By this method a complete record of each separate work-interval and of all influences and irregularities is obtained.

(d) Other uses of the pulse-time recorder. The pulse-time recorder makes it possible, among other uses, to conduct serial experiments concerning the precision of reaction, as in the following example. The *S* regards a stop-watch whose fast moving hand completes a turn in 3 sec. On its face the full seconds are clearly marked in red. His task is to close a circuit at the passage of the hand through the second-marks. If it were desired to determine the deviations in the "personal equation" *S* should try to mark the intervals as seen by the eye correctly. If there were no error, all the recorded lines should have a length of exactly 100 mm., as 1 mm. corresponds to 0.01 sec. As, however, the *S* will press the button now too soon and now too late, the recorded lines will be either too long or too short.

It is also possible to conduct by means of the pulse-time recorder serial experiments concerning the time of reaction in a very efficient manner, with each separate time of reaction directly recorded quantitatively.

The pulse-time recorder further may supply, in the simplest manner imaginable, graphic representations of the highest exactness, when it is desirable to investigate a person's ability to repeat time-intervals, or to divide given time-intervals according to a certain rhythm, as in playing the piano.

Summary. A new apparatus, the pulse-time recorder, is briefly described as recording consecutively, on a blackened drum, the duration of any kind of time interval directly as a vertical line. Several uses of this pulse-time recorder are discussed, as, *e.g.* the registration of the frequency of the pulse, and of the respiration, separate registration of the duration of inspiration and expiration, experiments concerning the physiology of work, determination of the time of reaction, and of the precision of reactions involving time.

A MODEL OF THE SYNTHESIS OF CONDITIONED REFLEXES

By George K. Bennett and Lewis B. Ward, Yale University

In recent months several articles have appeared describing mechanisms which duplicate various adaptive phenomena formerly thought to be peculiar to 'psychical' behavior. Notable among them have been the articles of Stephens,[1] Baernstein and Hull,[2] Walton,[3] and Krueger and Hull;[4] and, from the point of view of mathematical physics, the study of Rashevsky.[5] The appearance of these articles is an indication of the interest in mechanical illustrations of behavior. The present article describes a mechanism employing well-known physico-chemical principles which illustrates several novel and slightly more complex types of adaptive behavior.[6]

Baernstein and Hull used toluene-mercury thermal relays as the basis of their adaptive mechanism. Krueger and Hull produced the mnemonic phenomena by storage of power in polarizable cells. The present authors have attempted a closer approach to the traditional synaptic theory of neural conduction in learning by employing an electro-chemical cell of variable resistance. This type of cell seems to permit of greater flexibility in circuit design, and thus to allow a simulation of higher forms of 'psychical' behavior.

Several types of cell were tried with varying degrees of success, but the one described here was the most satisfactory. It is in essence a lead-acid cell, the polarity of which is dependent on the direction of the previous charge. As used in this mechanism, polarity may be established in either direction, and the cell potential is added to or subtracted from a fixed potential in the circuit to produce or prevent the closure of a relay. This cell differs from the ordinary storage cell, (1) in its use in the circuit as a variable resistance, and (2) in that it provides an indiscrete polarization series running from a maximum of one polarity through a zero point to a maximum of the opposite polarity.

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[1] J. M. Stephens, A mechanical explanation of the law of effect, this Journal, 41, 1929, 422-431; esp. 424.

[2] H. D. Baernstein and C. L. Hull, A mechanical parallel to the conditioned reflex, *Science,* 70, 1929, 14 f.

[3] Albert Walton, Conditioning illustrated by an automatic mechanical device, this Journal, 42, 1930, 110 f.

[4] R. G. Krueger and C. L. Hull, An electro-chemical parallel to the conditioned reflex, *J. Gen. Psychol.,* 5, 1931, 262-269.

[5] N. Rashevsky, Learning as a property of physical systems, *ibid.,* 207-229.

[6] We wish to acknowledge our indebtedness to Professor Clark L. Hull.

In the accompanying diagram, Fig. 1, pushbuttons Su_1, and Su_2 represent the normal, or unconditioned stimuli, for reactions *Ra* and *Rb*, which are lights. Sc_1 and Sc_2 are also pushbuttons and represent stimuli to which the responses *Ra* and *Rb* can be conditioned. The variable resistance cells are represented by the symbols C_1, C_2, C_3, and C_4. As may be seen from the accompanying circuit, *Ra* and *Rb* represent antagonistic reactions in the sense that they cannot occur simultaneously. This is due to the action of the differential relay *Ryd*. Relays Ry_3 and Ry_4 are arranged so as to prevent the passage of current through the coil of relay *Ryd* which is not in use. The relays Ry_1 and Ry_2 are connected to Sc_1 and Sc_2 respectively. When Sc_1 is pressed, relay Ry_1 connects cells C_1 and C_2 to the intermediate potential source. Relay Ry_2 has a similar function in respect to cells C_3 and C_4, with Sc_2. Cells C_1 and C_3 connect through Relay Ry_4 to one coil of differential relay *Ryd* and through Su_1 (if closed) to the highest source of potential. Likewise cells C_2 and C_4 connect through relay Ry_3 and the other coil of differential relay *Ryd* and Su_2 to the highest potential.

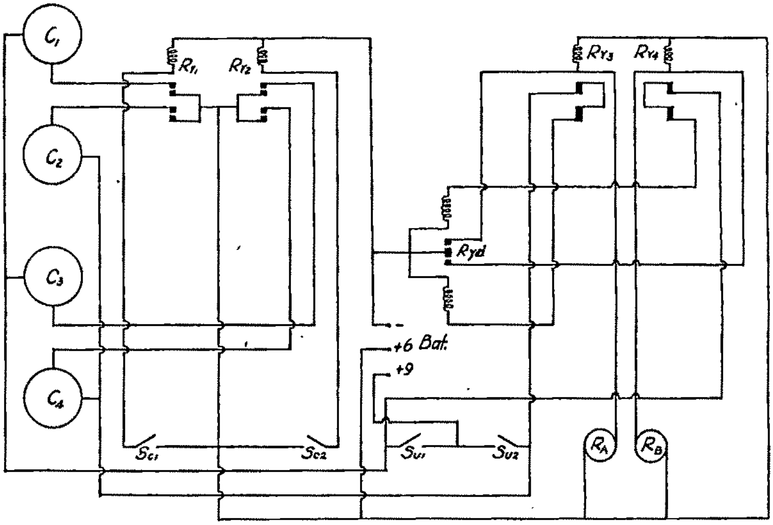

FIG. 1. WIRING DIAGRAM

It may be seen that when Sc_1 and Su_1 are simultaneously closed, a potential equal to the difference between the high and intermediate potential values is imposed upon cell C_1 so as to produce a polarity of charge in the same direction as that of the potential supply through relay Ry_1. If, now, Sc_1 is subsequently pressed, the total potential applied to the upper coil of *Ryd* will be sufficient to draw the armature in that direction and produce reaction *Ra*. See Fig. 2 below for a schematic representation.

The differential relay *Ryd* is so built as to be adjustable both as to gap on the armature contact system and also as to spring tension holding the armature in neutral position. As used in the mechanism described, the differential relay is so adjusted that the flow of current through each of the cells when in a relatively neutral or unconditioned state is insufficient to pull the armature over and cause either response. Thus, when the mechanism is in a neutral or unconditioned state, closure of Sc_1 and Sc_2 produces no reaction.

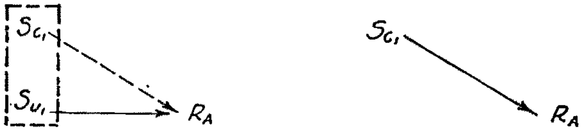

FIG. 2. SIMPLE CONDITIONING

In like manner (as to produce reaction *Ra*) Sc_1 can be caused to produce reaction *Rb,* and Sc_2 to produce either reaction *Ra* or *Rb.* Should Sc_1 and Sc_2 be closed simultaneously, together with Su_1 or Su_2, either *Sc* will produce the reaction corresponding to the *Su* which had been simultaneously closed, illustrating the phenomenon of the conditioning of a single response to multiple stimuli. See Fig. 3 below.

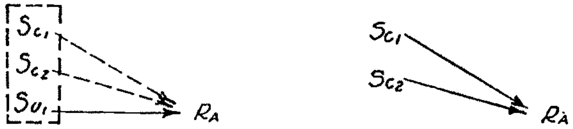

FIG. 3. CONVERGENT SERIES

Should Sc_1 or Sc_2 be closed with Su_1 and Su_2 together, either reaction (but not both) may occur; but on subsequent pressings of the *Sc,* both reactions will occur, one at a time, in a partly random order, depending on the relative strength of the current in the two coils of the differential relay *Ryd,* which in turn depends on the condition of the resistance cells, their previous and present degrees of polarization. Extinction of the so-called conditioned responses to Sc_1 and Sc_2 takes place through the development of a potential in the resistance cells opposite the potential through Sc_1 and Sc_2.

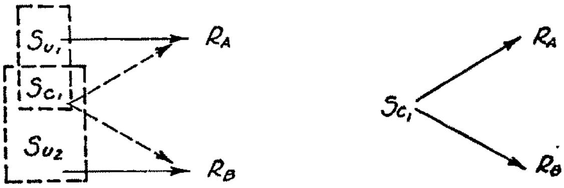

FIG. 4. DIVERGENT RESPONSE

The process described in the paragraph above is that of divergent response, as described by Hull wherein a neutral stimulus is presented successively in conjunction with each of two antagonistic responses (see Fig. 4). When the previously neutral stimulus is now presented the mechanism has two reaction tendencies which are incompatible. The response which has a stronger tendency will occur; but if it is not reënforced by the appropriate unconditioned stimulus, a successive presentation of the same previously neutral stimulus *may* produce the second response, since the excitatory tendency of the first response has been diminished by its unreënforced occurrence to below the strength of the second response.

It will be noted that the behavior of the mechanism in this case is closely analagous to certain forms of animal behavior usually described as 'trial-and-error learning.' In the case of an animal confronted by a puzzle box situation, if the first reaction is unsuccessful, another is likely to be made, and so on until success occurs or fatigue intervenes. If the animal succeeds in solving the problem box, the previously successful response will, on subsequent attempts, occur earlier in the series. Since the mechanism described here has only two responses as compared to a very considerable number for even the lowest forms of mammalian life, the trial and error process in the machine is a very much abbreviated and simplified version of its animal prototype.

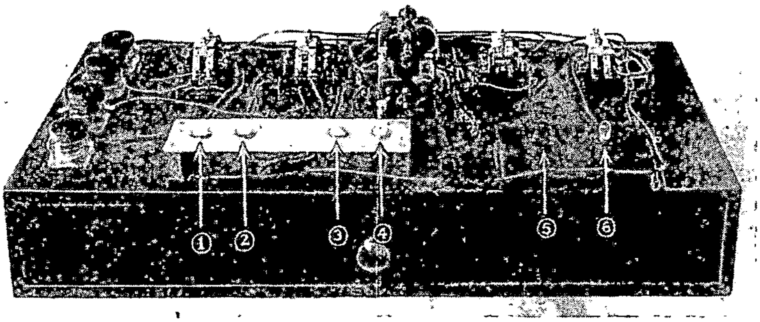

FIG. 5. PHOTOGRAPH OF MODEL

Nos. (1) and (2) indicate Sc_1 and Sc_2 respectively; (3) and (4) Su_1 and Su_2; and (5) and (6) *Ra* and *Rb*. The five relays are seen to the rear, with *Ryd* occupying the center position. The cells are at the left. The box contains six dry cells which are used as a source of potential.

The apparatus (see Fig. 5) described here illustrates also several of the quantitative aspects of the conditioned reflex. The number of unreënforced occurrences of the conditioned response is dependent upon the frequency of the previous simultaneous presentation, the lapse of time intervening, and the earlier excitatory or inhibitory relationships.

Following these principles there is at present in the process of construction a more elaborate mechanism designed to illustrate phenomena of divergent response on a higher level. It is also hoped to construct, and it does not seem infeasible, mechanisms demonstrating conditioning to patterns, serial conditioning, and possible maze learning.

APPARATUS NOTES

A Device for Automatically Varying a Series

It is sometimes desirable to be able to present a single long stimulus series, or to present half a dozen stimuli in varying orders. On the other hand it may be convenient to register multiple responses on a single recording instrument in some such fashion as Darrow's recording of the galvanic reflex. For such purposes, and in fact wherever rapid and complex switching is desired, the rotary automatic switch will be found useful. The 4 level automatic rotary switch has been used by Burtt for serial reaction time stimuli. However, the combination of a commutator and Strowger 6 level rotary switch,[1] which we have used, has somewhat wider application.

The 4 level switch has been described in detail by Burtt[2] (1931). The 6 level switch is of similar construction. Each level consists of 25 contacts mounted in a semi-circle, while a switch wiper whose axis is at the center of the semi-circle connects each contact in turn. The 6 levels are mounted side by side in a compact unit and all 6 wipers are operated together by an electromagnet. Thus the wipers make 6 connections in each of 25 positions as they rotate through the semi-circle.

We used the switch for automatically varying a series in the following manner. Electrically operated stimulating devices were connected so that one pole of each was attached to a common battery lead and the other to one of the 6 switch levels. The circuits through the 6 wipers and therefore through the stimulators were then closed one after the other by means of a timing device which consisted of a commutator arm running over a series of contacts. Thus the first series was presented. The same 6 stimulators were connected to the 6 levels of the switch in a different order on the second contact. Now when the wipers moved to the second position and were again energized one after the other as before, a second order of stimuli resulted. Similarly a third order was obtained from the third wiper position, and so on around the semi-circle.

An extra contact on the commutator produced the motion of the wipers from one position to another. That is, after the commutator had closed the circuits through the 6 wipers, one by one, it then, by means of the seventh contact, energized the magnet of the switch rotor, and thus caused the bank of wipers to step to the next position on the arc.

We found it convenient to mount the switch and to permanently connect the 6 sets of 25 contacts to Fahnestock clips on a terminal panel.[3] The order of stimuli for each position of the switch wipers, as described above, was then readily procured by connecting the leads from the stimulators in the desired order to the proper terminal board clips. Re-arrangement of the order of presentation merely required altering the connections from the stimulators to the terminal board.

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[1] Switches with as high as 50 points and 3 levels or 25 points and 6 levels are obtainable from the Automatic Electric Co. Chicago, Ill.

[2] H. E. Burtt, Inexpensive apparatus for continuous choice reaction, *J. Gen. Psychol.*, 5, 1931, 286-288.

[3] We are indebted to Mr. Eugene Sayler for advice and aid in mounting the switch.

The switch can be made to step fom one series of 6 to the next automatically, or under key control. It will also return automatically to the beginning of the cycle, even though some of the 25 series of contacts are unused in a given set up. This return is brought about by wiring the stepping magnet to the remaining unused contacts of one of the levels so that as the wipers move to each unused position, they activate their own stepping magnet and move on around the arc to the first position. A single series as long as the total number of contacts (150) may be obtained. Or if desired, two orders of presentation of a series of half this length, three of one third and so on, are obtainable. Two or more such switches can be used in the same hook-up to give a limitless number of combinations.

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A Simplified Heat Grill

Available heat grills vary in complexity from dual circulating water systems (Dallenbach and Burnett)[1] to interlocking sets of previously chilled and warmed nails (Schlosberg and Carmichael).[2] At the Northwestern Psychological Laboratory we have recently found that variable condensors can be easily adapted for a similar purpose.

These condensors are, in essential, two sets of thin brass plates, one set movable and the other stationary, intermeshed in such a way that the fingers can be placed upon the upper edges of both sets. They can be purchased very reasonably at any radio salvage company and adapted with very little effort for use in laboratory demonstrations of heat experience. The lower portion of the revolving plates is cut away; and the metal rod which connects them and serves as their axis is heated in some fashion. The condensor is set in a pan of ice water so that only the stationary plates become chilled. The movable plates can be warmed either by passing an electric current through them or placing a small candle beneath their axis rod, which projects some distance beyond the frame of the condensor. Since the movable and stationary plates are perfectly insulated, temperature changes in one set can effect those in the other only through air convection (a common feature in all heat grids). The fingers can be placed on the edges of the stationary plates and the movable plates brought in between them so that the skin is stimulated by alternate warm and cold edges. Under these conditions the experience of heat is usually reported.

This apparatus seems to have certain advantages over both types of heat grid mentioned at the beginning of this note. It is less expensive and cumbersome than the dual circulating water system. The space between areas of warm and cold stimulation is much finer than that obtained by use of interlocking nails.

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[1] N. C. Burnett and K. M. Dallenbach. The experience of heat, this Journal, 38, 1927, 418-431.

[2] H. Schlosberg and L. Carmichael. Three new pieces of apparatus from the psychological laboratory of Brown University, *ibid.*, 43, 1931, 119.

NOTES AND DISCUSSIONS

THE MEANING OF PLEASANTNESS AND UNPLEASANTNESS

Nafe published in 1924 an experimental report in which he identified pleasantness with a bright pressure and unpleasantness with a dull pressure.[1] Nafe concluded that these pressures *are* pleasantness and unpleasantness. The failure of Young to duplicate Nafe's results, with the exception of one of his subjects who had formerly observed for Nafe,[2] led Nafe to reply that these pressures were not "attention demanding" and would not be reported unless the observer were set for them.[3] This dependence on attitude has recently been confirmed by Wells.[4] A series of investigations conducted by the present writer verified the existence of these pressures and their intimate relation to hedonic tone.[5] The use of the method of correlation, in which a series of pressure judgments were correlated with a series of affective judgments obtained for the same stimuli but at different times, led the writer to conclude, however, that bright and dull pressures *accompany,* rather than *are,* pleasantness and unpleasantness. The fact that correlation establishes a relationship rather than an identity, together with the known dependence of these pressures upon attitude, makes this conservative conclusion quite proper; but one is still faced with the further problem of defining and explaining the relationship in question. We have discovered palpable sensory contents correlating with pleasantness and unpleasantness. These contents are bright and dull pressures; but pleasantness and unpleasantness seem to be something more than these pressures. What they are is the question that this paper attempts to answer.

The situation becomes somewhat clearer if we take a behavioristic approach and regard the words *pleasantness* and *unpleasantness* as conditioned verbal responses to a stimulus-situation, one of whose elements may be the pressures we have been observing. Thus the individual reacts to certain elements of a stimulus situation by giving either of the verbal responses "pleasantness" or "unpleasantness." These verbal responses may be overt, as are usually called for in the experimental setting; or they may be implicit, a mere appearance of the words in the conscious thought processes, as is characteristic in daily life. They may be conditioned to bright and dull pressures or they may be conditioned to other elements in the stimulus-situation. According to this view the terms *pleasantness* and *unpleasantness* have a reference which is not limited to the sensory contents mentioned, but extends to further, non-sensory material as well.

The inclusion of non-sensory material under the affective experience has given rise to the controversy of "meaning vs. process," which can be traced in the hedonic experimentation of the first part of this century. A typical discussion can be found in the work of Koch.[6] Young recently recognized this ambiguous reference of our hedonic concepts when he said: "The affective judgment is equivocal regarding the existence or non-existence of felt experience. Objects may be judged with little regard to the felt pleasantness and unpleasantness of experience at the time the judgment is made. The evaluation of an object as 'pleasant' or 'unpleasant' tells nothing directly about the felt experience of the subject."[7] In this matter we should not take sides and label affection as sensory or as non-sensory. Both references of the term have been established experimentally time and again. Rather we should understand how the hedonic terms can logically be accepted as referring to both. By thus hypothecating a possible situation wherein pleasantness and unpleasantness, as verbal responses, can become conditioned to more than one source of stimulation, we not only provide a compromise solution of the sensory, non-sensory difficulty, but we bring the findings of Nafe and the present writer on bright and dull pressures into logical relationship with the rest of the field.

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[1] J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544.

[2] P. T. Young, Studies in affective psychology, *ibid.,* 38, 1927, 175-185.

[3] Nafe, The psychology of felt experience, *ibid.,* 39, 1927, 367-389.

[4] E. F. Wells, The effect of attitude upon feeling, *ibid.,* 42, 1930, 573-580.

[5] W. A. Hunt, The relation of bright and dull pressure to affectivity, *ibid.,* 43, 1931, 87-92.

Let us start with the assumption that the affective response involves a general reaction of acceptance or rejection of a stimulus situation. These two general types of response are found throughout the animal world. As Piéron phrases it, "Chez tous les animaux, même inférieurs, on voit, à la suite de certaines excitations, des réactions positives marquant l'appetition, l'expansion, la recherche, la poursuite de l'excitant at des réactions négatives indiquant l'aversion, le retrait, la fuite. Nous voyons là l'opposition des réactions affective fondamentales, agréable et désagréable."[8] Troland uses this conception as the basis of his theory, saying that certain harmful stimuli arouse an innate rejection-response and certain beneficial stimuli arouse an innate acceptance-response. This is his principle of nociception and beneception.[9] By conditioning, these responses may spread beyond their original innate bases and become attached to other stimuli. Thus a stimulus may arouse one of two general responses, acceptance or rejection. These responses are of a lower order and are probably set off or mediated somewhere in the lower brain centers, such as the thalamus. It seems reasonable that these responses should involve a general bodily orientation, whose effects in the body-cavity result in the experiencing of bright or dull pressures. So-called pleasant stimuli, for instance, may set off a reflex at the thalamic level, and this reflex involves a bodily adjustment or reaction whose sensory resultants are experienced in consciousness as a *bright pressure*. The conscious reverberations of the bodily orientation to an unpleasant stimulus make up the experience known as a *dull pressure*.

This hypothesis properly implies that the pressure experience is a complex experience of the nature of a perception or blend, rather than a new sensory element depending on specific sense-organs as Beebe-Center would seem to imply.[10] While Nafe's terminology and the attempt to fit these pressures into the Titchenerian touch pyramid has favored this latter view, actual experimentation by the writer would tend to refute it. He found his observers reporting that bright and dull pressures resemble general complexes of sensation, organic perceptions as it were, and was able in a simple analytical situation to get reports of ordinary sensations of pressure in the trunk which correlated extremely highly with 'bright pressure' and 'dull pressure.'[11] This result supports the view that these pressures are the complex sensory resultant of the general bodily adjustment involved in the affective response. In terms of the context theory of meaning, these pressures are the *sensory* core of *affectivity.* Under a sensory attitude of report the presence of these pressures sets off the verbal response of 'pleasantness' or 'unpleasantness.' In such a case the terms *pleasantness* and *unpleasantness* refer to these pressures. They *mean* these pressures. But the meaning of these terms is not limited to bright and dull pressure alone, as sensory experience may or may not be present when a report of "pleasantness" or "unpleasantness" is given.

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[6] B. Koch, *Experimentelle Untersuchungen über die elementaren Gefühlsqualitäten*, 1913.

[7] Young, Studies in affective psychology, this JOURNAL, 42, 1930, 20.

[8] H. Piéron, *Psychologie expérimentale*, 1927.

[9] L. T. Troland, *The Fundamentals of Human Motivation*, 1928.

[10] J. G. Beebe-Center, The relation between affectivity and specific processes in sense-organs, *Psychol. Rev.*, 37, 1930, 327-333.

This difficulty will be cleared up if we examine this 'response' point of view more closely. Originally the verbal response of 'pleasantness' or 'unpleasantness' may have been attached to the sensory experience that we call a bright or a dull pressure; but by conditioning it then may have become directly attached to the stimulus arousing this pressure. This 'short-circuiting' might even result in the stimulus-object's calling forth the verbal response before the appearance of the sensory content which originally gave rise to it. Thus the taste of sugar might call forth the response 'pleasantness' without any necessary intervention of a bright pressure experience. Such a process of conditioning may well explain the tendency of certain observers to 'objectify' hedonic tone or to place it as an attribute of the stimulus-object rather than to look upon it as a subjective experience peculiar to the responding organism. Once we form such a verbal response, it may be used independently on a higher level, *i.e.* in the logical thought processes, without the accompaniment of any sensory determinant. Thus we may speak of pleasantness and unpleasantness without experiencing any sensory content. As Romanes pointed out, "the names of recepts now serve as symbols of the recepts themselves, and so admit of being grouped without reference to the sensuous perceptions out of which they originally sprang."[12]

Unfortunately, the intricate conditionings to which the verbal responses we are discussing may be subjected by the processes of thought do not lend themselves to exact treatment. Such treatment must await the development of our knowledge of the higher mental processes. It is important to note here, however, that in the usual experimental approach to affectivity the observer is called upon to make a report, not of 'pleasant*ness*' or 'unpleasant*ness*,' but of 'pleasant' or 'unpleasant.' The reference of these latter terms is even more equivocal than that of the terms *pleasantness* and *unpleasantness.* Their adjectival nature stresses the objective characteristics of the stimulus rather than the felt experience of the observer. It is only natural that these terms should wander far from any immediate and direct reference to a sensory content of bright or dull pressures.

In closing, it is only fair to mention that the verbal response might become attached to some bodily cue other than these pressures. Such a cue might well arise, as Cannon would have us believe, at the thalamic level, due to mediation of the thalamus in the affective response.[13] These possibilities exist and no doubt may add their perplexing bit to the welter of hedonic experience.

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[11] Hunt, Localization of bright and dull pressure, this JOURNAL, 44, 1932, 308 ff.

[12] G. F. Romanes, *Mental Evolution in Man,* 1889, 75.

[13] W. B. Cannon, Neural organization for emotional expression, *Wittenberg Symposium,* 1928, 257-269.

The interpretation of pleasantness and unpleasantness as conditioned verbal responses, back of which may lie the experience of bright or dull pressures, throws light upon many of the traditional problems in affectivity. Mixed feelings may well occur as verbal responses at a level of logical thought or meaning, or we might have a feeling process of one kind existing at a sensory level and accompanied in consciousness by the verbal response (aroused through another source) associated with its opposite. The argument over the sensory or non-sensory nature of feeling also becomes clear, since bright and dull pressure may explain the sensory side, and the non-sensory side may be explained by the processes of verbalized thought. Finally, the old question of why feeling fades under attention is answered by this view. Attention to the sensory content of pleasantness and unpleasantness, in other words attention to bright and dull pressures, means concentration upon a secondary process, with the attendant removal of attention from the stimulus responsible for the reaction and the consequent fading out of the very content under inspection. Attention to pleasantness and unpleasantness as verbal responses is also fruitless. There is nothing palpable there. Such is the experiential nature of words, and such must be the introspective nature of pleasantness and unpleasantness when these refer to verbal responses. The perfect completion of the hedonic picture must of course await the extension of our knowledge in many fields. In the meantime the view presented here seems to bring some temporary order and unity to the subject.

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WHAT IS THE CURVE OF FORGETTING?

For some reason or other, errors have frequently been made in textbook accounts of Ebbinghaus' classical experiments on memory.[1] Van Omer and Dallenbach have recently pointed out one of these errors,[2] and we wish here to call attention to another—to the misrepresentation of the curve of forgetting.

Ebbinghaus summarized the results of his experiments on "retention and obliviscence as a function of time" in a table of five columns,[3] which is fully and clearly labelled with headings and sub-headings as follows:

	I	II	III	IV
No.	After X hours	So much of the series learned was retained that in relearning a saving of Q% of the time of original learning was made	P.E.m	The amount forgotten was thus equivalent to v% of the original in terms of time of learning
	X=	Q=		v=
1	0.33	58.2	1	41.8
2	1.	44.2	1	55.8
3	8.8	35.8	1	64.2
4	24.	33.7	1.2	66.3
5	48.	27.8	1.4	72.2
6	6×24	25.4	1.3	74.6
7	31×24	21.1	0.8	78.9

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[1] Hermann Ebbinghaus, *Über das Gedächtnis,* 1885; Eng. trans. by H. A. Ruger and C. E. Bussenius, 1913.

[2] E. B. van Ormer and K. M. Dallenbach. A frequent error concerning Ebbinghaus' experiments on obliviscence, this JOURNAL, 43, 1931, 706 f.

[3] *Op. cit.,* Eng. trans., 76.

The figures under column II, *i.e.* the Q%, are calculated from direct experimental. results. They express the percentage of saving of time in relearning, *i.e.* the ratio of the difference of time between learning and relearning to the time of learning; hence a measure of retention. The figures under column IV, *i.e.* the v%, are deduced figures by subtracting the corresponding Q% from 100% in order to express the amount forgotten. Ebbinghaus pointed out the significance of these data in the following words:

> One hour after the end of the learning, the forgetting had already progressed so far that one half the amount of the original work had to be expended before the series could be reproduced again; after 8 hours the work to be made up amounted to two thirds of the first effort. Gradually, however, the process became slower so that even for rather long periods the additional loss could be ascertained only with difficulty. After 24 hours about one third was always remembered; after 6 days about one fourth, and after a whole month fully one fifth of the first work persisted in effect. The decrease of this after-effect in the later intervals of time is evidently so slow that it is easy to predict that a complete vanishing of the effect of the first memorization of these series would, if they had been left to themselves, have occurred only after an indefinitely long period of time.[4]

Ebbinghaus discusses here both forgetting and retention, referring sometimes to the figures in column II and sometimes to those in column IV. He represents, however, neither array of figures by graphs.

Ladd and Woodworth were the first to present the data of Ebbinghaus' column II in graphic form,[5] and they, unfortunately for many who followed them, mislabelled this falling curve as the "curve of forgetting."[6] Referring to the figures of column II, those that show the amount retained, Ladd and Woodworth wrote:

> These results may be plotted into a "curve of forgetting" (or of retention), by making the distances along the horizontal axis proportional to the percentages of material retained.[7]

Though it is clear enough from the context that Ladd and Woodworth are discussing retention, they write of the "curve of forgetting," and label the graph representing Ebbinghaus' data on retention as "The Curve of Forgetting."

Following Ladd and Woodworth in time if not in act, Thorndike also labelled the falling curve of retention as the "curve of forgetting."[8] And after Thorndike the error is rife. Pillsbury made it in his *Fundamentals* in 1916,[9] and repeated it in his revised edition in 1922,[10] though he did not make it in his *Essentials* published in 1911.[11] The error is also made by Starch,[12] Watson,[13] Smith and Guthrie,[14]

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[4] *Ibid.*, 76.

[5] Ogden ascribes primary to Ebbinghaus. He says, "Ebbinghaus was able to plot a curve of forgetting which indicates that the impression fades very rapidly during the first hour following the initial memorization" (R. M. Ogden, *Psychology and Education*, 1926, 208). I have been unable to verify Ogden.

[6] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911. 575.

[7] *Ibid.*, 575 f.

[8] E. L. Thorndike, *Educational Psychology*, II, 1913, 306.

[9] W. B. Pillsbury, *The Fundamentals of Psychology*, 1916, 361.

[10] *Op. cit.*, 2nd ed., 381.

[11] Pillsbury, *The Essentials of Psychology*, 1911, 194 f.

[12] Daniel Starch, *Educational Psychology*, 1919, 156 f.

[13] J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, 1919, 344.

[14] S. Smith and E. R. Guthrie, *General Psychology in Terms of Behavior*, 1921, 111.

Woodworth,[15] Warren,[16] Jones,[17] Gates,[18] Dashiell,[19] Pintner,[20] Colvin, Bagley, and Macdonald,[21] Wheeler,[22] Lucas and Benson,[23] and Monroe, DeVoss, and Regan.[24]

It is a curious and interesting fact that both the origin and reproduction of this error follow closely the course of the error pointed out by Van Ormer and Dallenbach.[25] Trust in secondary sources accounts for the reproduction, but it is more difficult, since Ebbinghaus was explicit in his exposition, to account for the origin. Our purpose, however, is not to explain the error, but merely to point it out so that it will no longer be perpetuated in the psychological literature.

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Paradoxical Warmth

Magnus Blix,[1] while investigating the apparent specificity of sensory nerves, happened upon a demonstration of the punctiform nature of cutaneous sensitivity and, in so doing, wrote the opening paragraphs of one of the strangest chapters in modern experimental psychology. Independently and almost simultaneously with the appearance of his article, Goldscheider,[2] in Germany, and Donaldson,[3] in America, published accounts of their discoveries of the same phenomena. Goldscheider, however, carried his studies further than either of the other two in that he made attempts to determine the effect of stimulating these newly-discovered warm and cold spots with various 'inadequate' stimuli. When he found that the spots reacted to thermal stimuli of opposite sign, he probably regarded this simply as a demonstration that the spots reacted to pressure, since it would have appeared ridiculous to propose at that time that a 'warm' stimulus-object could elicit an experience of cold.

A quotation to be given presently will show that this must have been his attitude on this matter; and the same quotation will make it apparent that Goldscheider actually made the first experimental demonstration of the existence of paradoxical warmth and paradoxical cold, although the discovery is commonly credited to Lehmann and the christening of the phenomenon to Von Frey. Be that as it may, paradoxical cold was early recognized as an affair of importance for psychology. It was soon mentioned in various secondary sources and was taken into consideration whenever theories of thermal response were being formulated. As time passed, its acceptance was so ready that it was quickly reduced to the level of an experimental commonplace to be verified anew by successive generations of undergraduates enrolled in the elementary laboratory course.

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[15] R. S. Woodworth, *Psychology: A Study of Mental Life,* 1921, 360; *Psychology,* 1929, 95.

[16] H. C. Warren, *Elements of Human Psychology,* 1922, 190.

[17] H. E. Jones, Experimental studies of college teaching, *Arch. Psychol.,* 10, 1923, (no. 68), 1-71.

[18] A. I. Gates, *Psychology for Students of Education,* 1923, 259 f.; *Elementary Psychology,* 1925, 331 f.

[19] J. F. Dashiell, *Fundamentals of Objective Psychology,* 1928, 363.

[20] Rudolf Pintner, *Educational Psychology,* 1929, 254.

[21] S. S. Colvin, W. C. Bagley, and M. E. Macdonald, *Human Behavior,* 1929, 233 f.

[22] R. H. Wheeler, *The Science of Psychology,* 1929, 288.

[23] D. B. Lucas and C. E. Benson, *Psychology for Advertisers,* 1930, 261.

[24] W. S. Monroe, J. C. DeVoss, and G. W. Reagan, *Educational Psychology,* 1930, 180.

[25] *Op. cit.,* 706 f.

[1] Magnus Blix, Experimentela bidrag till lösning af fragan om hudnervnas specifika energi, *Upsala Läkäreforenings Förhandlingar,* 1882-1883, 87-102; 427-441. A German translation of this article was published in *Zsch. f. Biol.,* 20, 1884, 141-156.

[2] A Goldscheider, Die specifische Energie der Temperaturnerven, *Monat. f. Pract. Dermatol.,* 1884, 3, (cf. *Gesammelte Abhandlungen,* 1898, 53).

[3] H. H. Donaldson, On the temperature sense, *Mind,* 10, 1885, 399.

Paradoxical warmth, on the other hand, never gained this ready recognition; it was completely ignored in the literature and eventually suffered the supreme ignominy of complete obliviscence by its own discoverer. For it is a fact that although Goldscheider plainly described paradoxical warmth in 1884 he proceeded to forget this altogether and to discover it afresh in 1912! Although Rubin made further experimental verification of its existence in the same year, paradoxical warmth again sank to obscurity, failing to merit even passing mention in secondary sources. In view of this state of affairs the writers have deemed it worth while to recover these 'lost' references, to offer further experimental evidence of the existence of the phenomenon, and to insist that any adequate theory of thermal sensitivity must account for its occurrence.

That Goldscheider did indeed make the first experimental demonstration of the existence of paradoxical warmth will be apparent upon careful perusal of the following citation from his paper of 1884:

> "Dass es in der Tat lediglich der Druck ist, welcher die specifische Empfindung auslöst geht unzweifelhaft aus der Beobachtung hervor, dass es gelingt, nicht blos mit erwärmter, sondern auch mit stärker erhitzter Nadel mechanisch an manchen Kältepunkten Kälteempfindung hervorzubringen. . . . *Ebenso kann man mit der abgekühlten Nadel, ja mit dem kühlen Messingcylinder an manchen Wärmepunkten Wärmegefühl erzeugen.*"[4]

Even with the quotation out of context it is patent that Goldscheider failed to grasp the systematic significance of the observations he had reported, seeing in them only a demonstration of specificity since the thermal sign of the stimulus was opposed to that of the experience.

In view of this cavalier treatment of his novel findings it is no wonder that Goldscheider failed to remember his own discovery or that twenty years later Thunberg should write:

> "Ob es auch eine paradoxe, also eine durch Kältereizung der Wärmenerven entstehende Wärmesensation gibt, ist noch nicht sicher entschieden . . . Gegen die Möglichkeit solch paradoxer Reizbarkeit spricht zwar, dass weder Lehmann noch Alrutz noch Kiesow paradoxe Warmeempfindungen erzielen konnten, obgleich sehr intensive Kältereizmittel (bis —70°) zur Anwendung kamen."[5]

This lack of evidence led Titchener to accept Thunberg's doubt as to the existence of paradoxical warmth and to write in 1910 that:

> "Paradoxical sensations of warmth . . . have never been observed in the normal subject. It may be that they do not occur; or it may be that the end-organs are too deeply set in the skin to be reached by a punctiform cold stimulus."[6]

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[4] A. Goldscheider, *op. cit.*, 63. (Italics ours.)

[5] T. Thunberg in Nagel's *Handbuch der Physiologie des Menschen*, 1904, 679.

[6] E. B. Titchener, *A Text-Book of Psychology*, 1909, 151.

He notes with Thunberg the existence of a 'perverse warmth' reported by Strümpell in pathological cases but regards it as "doubtful whether such statements do not rest upon a confusion."

This is typical of the situation until 1912 when Goldscheider, with complete disregard for his discovery of three decades previous, proceeds to re-discover paradoxical warmth and to report it as a novel finding. He writes:

> "Ich konnte an einzelnen besonders empfindlichen Wärmepunkten mittels umschriebener Kältereize deutliche Wärmeempfindungen erzeugen. Doch gelingt dies nur bei relativ schwachen, nicht bei starken Kältereizen. Am günstigsten schienen mir Reiztemperaturen zwischen 22° and 26° C zu sein. Verschiedene Authoren haben sich mit der Frage der theoretisch zu postulierenden paradoxen Wärmeempfindung beschäftigt ohne das Phanomen auffinden zu können. *Auch mir ist es früher nicht gelungen.* Wahrscheinlich haben wir stets zu grosse Kältereize angewendet."[7]

In the same year Rubin publishes his independent discovery of paradoxical warmth in the following words:

> "Ich glaube, dass es mir gelungen ist, entsprechende paradoxe Wärmeempfindungen unter ziemlich normalen Verhältnissen zur konstatieren. . . . Wenn das Wasser im Behälter genügend warm war, bekam die Vp., natürlich . . . eine normale Wärmeempfindung. Wurde das Wasser nun in kleinen Stufen kälter gemacht, dann wurden . . . die Wärmeempfindungen zuerst schwächer, um dann ganz zu verschwinden. Aber wenn das Wasser noch kälter gemacht wurde, kamen mit der jetzt eintretenden Abkühlung der Wärmepunkte die Wärmeempfindungen wieder. Sie wurden zuerst mit wachsenden 'Kältereizen' stärker, um dann wieder abzunehmen und traten bei ganz niedrigen Kältereizen nur sporadisch auf."[8]

Although these two references contain definite evidence for the existence of paradoxical warmth, they have been neglected entirely in the American literature and have received but little better treatment elsewhere. Troland made no mention of them in his comprehensive *Psychophysiology,*[9] although he allowed for paradoxical cold in formulating his theory of thermal receptor action. Fröbes,[10] it is true, does mention the existence of paradoxical warmth but contents himself by citing Rubin and ignoring Goldscheider. The writers have been unable to find any account of the phenomenon in other continental or American sources.

While it is hoped that a more complete and comprehensive program of research on this topic may be presented at a later date, the writers wish to take this opportunity to report certain preliminary observations made with the simplest of laboratory apparatus. The procedure employed was much like that of the common drill experiment on the location of temperature spots. Small grids were stamped on the skin of the ventral side of the forearm and the corners of each square of these grids were then stimulated by means of standard temperature cylinders maintained at fairly constant temperature by frequent immersion in water baths. The routine observed for each subject was as follows:

(1) A map of the warm spots was made with a stimulus-temperature of 39° C.

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[7] A Goldscheider, Revision der Lehre vom Temperatursinn, *Ber.u.d.V.Kong.f.exp. Psychol.,* Berlin, 1912, 227. (Italics ours.)

[8] E. Rubin, Beobachtungen über Temperaturempfindungen, *Zsch. f. Sinnesphysiol.,* 1912, 46, 392.

[9] L. T. Troland, *op. cit.,* 1930, II, 299-312.

[10] J. Fröbes, *Lehrbuch der experimentellen Psychologie,* 1923, 1, 140.

After a few minutes' rest, this map was checked by re-mapping the same area with the same stimulus-temperature.

(2) To prevent *O* from developing a 'set' for reports of warmth the remaining spots were stimulated with a cylinder maintained at 10° C. This led to uniform reports of cold.

(3) The stage was now set for the crucial series. To permit direct comparison between warmth as evoked by a 'normal' stimulus and that elicited by an 'inadequate' stimulus, a cylinder at 39° C. was alternated with one at 26° C. (a temperature reported by Rubin and by Goldscheider as favorable for paradoxical warmth).

The outcome of this procedure may be briefly stated. Paradoxical warmth was reported by 29 out of 33 *O*s, and in no case did the *O* describe this warmth as differing from that obtained by 'adequate' stimulus-temperatures. None of the *O*s suspected that there was anything out of the ordinary since they were blindfolded and the cold cylinder was applied only to the warm spots in the crucial series. *O* thus had either an experience of warmth or no experience of a thermal sort at all. Reports described a variety of intensities for warmths of both sorts, varying from 'weak,' and 'indistinct,' to 'clear cut,' and 'strong.' Frequency of reports of paradoxical warmth for a single grid (2 cm. x 2 cm.) varied from 0 to 8, with a median value of 2. In view of the difficulty untrained *O*s always experience in any attempt to report upon punctiform stimulation of warm spots, these results bulk too large to be dismissed without serious consideration.

The work must be repeated, of course, under more adequately controlled conditions and with trained *O*s before any significant quantitative results can be obtained. In the meantime, however, these results, together with those reported by Goldscheider and by Rubin, should contain an adequate warning to systematists that paradoxical warmth is no longer to be ignored by those who wish to write adequate theories of thermal sensitivity.

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GEORGE PAVLICEK
JOHN G. JENKINS

THE VISUAL IMAGERY OF A LIGHTNING CALCULATOR

The performances of Mr. Salo Finkelstein, Polish calculating genius, have recently received favorable attention in the universities of Europe and in the United States. Sándor has made certain quantitative studies of the range of this calculator's mental manipulations, and has furnished some evidence for a comparison with other mathematical prodigies who have been discussed from time to time in the literature.[1] Sándor's observations, in spite of their relative inadequacy, should entitle Mr. Finkelstein to a place among the world's foremost calculators. In addition to the quantitative aspects of the lightning calculator's performances, however, we may study with profit certain significant subjective features. Mitchell, among other things, was concerned with the memory types of mathematical geniuses in so far as they were auditory or visual.[2] Meumann used lightning calculators as examples of his ideational types.[3] This paper involves an extension of these principles of Mitchell and Meumann in that the data are related to the phenomenon of eidetic imagery, and the classification of Mr. Finkelstein as an *Eidetiker*. The results reported are based on about six hours of informal experimentation, and observations made in the course of attendance at three public performances. A number of the times for the various feats have been checked by us with a stop-watch.

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[1] B. Sándor, Die Gedächtnistätigkeit und Arbeitsweise von Rechenkunstlern, *Charakter,* 1, 1932, 47-50.

[2] F. D. Mitchell, Mathematical prodigies, this JOURNAL, 18, 1907, 61-143.

[3] E. Meumann, *The Psychology of Learning,* 1913, 214-222.

The ability involved in Mr. Finkelstein's performance is essentially two-fold. In the first place, he shows an unusual capacity for memorization with respect both to learning and retention. Among his other accomplishments is he able to memorize a visually perceived square of digits, composed of five rows of five digits each, so that the whole series can be repeated in any sequence desired. We have checked this feat on several occasions, and have found him successful at a rate of about 2 secs. per digit. He claims to give offhand the figures of π to 300 decimal places and the logarithm of any number from 1 to 100, to 7 places. Sándor is authority for Mr. Finkelstein's knowledge of the digits of π to 200 places, and there is indirect evidence that the further expansion of this decimal is used in connection with his calculations. His knowledge of the logarithms has been checked in public performances.

In the second place, it is interesting to note Mr. Finkelstein's extraordinary speed in the mental manipulation of figures. He can give the sum of 15 digits at a glance (Sándor), and the speed of addition of 3- and 4-place numbers at least exceeds that of an adding machine. He claims that he could analyze any 4-place number into the sum of four squares, $a^2 + b^2 + c^2 + d^2$. Our observations show that he succeeds in this task usually within one minute and is never over two minutes. All these calculations he performed without extraneous aid; they were entirely subjective.

However, the purpose of this note is not to review what is known about Mr. Finkelstein's speed of calculation, but to discuss the problem of his type of imagery.

The most striking feature of the actual process of calculation and memorization is the normality of the operations. Except for the products, sums, roots, and powers already definitely known, every calculation requires deliberate and fully conscious handling of the figures. He uses short-cut methods whenever possible, but the procedure is always orderly and systematic. The superiority of the processes lies in the fact that he is capable of dealing with large groups of figures. In the task of addition, for example, the average individual is limited to a succession of two and sometimes three-digit manipulations. Mr. Finkelstein, on the other hand, instantaneously perceives the sum of four, five, and six digits, with the result that the sum of a column of figures is given before most people can even read the figures. The superior adult has a memory span of about eight digits dictated in succession. With the same rate of dictation, Mr. Finkelstein is usually able to repeat twenty.

Mr. Finkelstein's process of memorization of numbers is aided by associations. In the course of the experiments, the following varieties were repeatedly reported: (a) dates of historical events; (b) mathematical associations such as powers, roots, logarithms, and prime numbers; (c) permutations of significant numbers; (d) ascending and descending series; (e) telephone numbers; (f) numerical characteristics of literary works, such as number of paragraphs contained. Because of the readiness of the occurrence of associations, if some type of association is not apparent, the numbers appear to be unusual and acquire memory value on account of this lack.

Mr. Finkelstein frequently speaks of "nice" numbers, and the "niceness" is a function of the number of associations aroused. Numbers designated as "nice," however, appear to have an emotional appeal; they are appreciated for their individuality, and automatically arouse aesthetic feelings. The numbers are not endowed with sex, but they possess, for him, uniqueness and personal value. When numbers were written in yellow, a color which he finds unpleasant, he could not avoid a reaction of distinct displeasure. It was, he reported, as if an artistic creation had been hideously caricatured.

Concomitant with the more extensive processes of memorization and calculation, one notes in Mr. Finkelstein a large amount of motor activity such as pacing to and fro, gesticulation, and facial contortion. Before writing the sum of a column of figures or the product of two large numbers written on the blackboard, he invariably draws a line through them. A definite kinesthetic component is here involved. There is a feeling of inhibition when these movements are suppressed.

In respect to the question of visual imagery, it is evident that the imaginal process is virtually integrated into the processes of memorization and calculation, and without it the involved manipulations of figures would undoubtedly be impossible. The imagery may be said to serve a reference function, since numbers resulting from various calculations and numbers which have acquired significance through associations are chalked down, so to speak, and held in readiness for subsequent reference. The imagery, accordingly, leaves the attention free for subsequent calculations, and there is no necessity for continuous review of the figures in order that retention may take place. Certain of the more prominent features of this imagery for numbers are as follows: (a) The numbers appear as if written with chalk on a freshly washed blackboard. (b) The numbers are in Mr. Finkelstein's own handwriting regardless of the form of presentation. (c) Ordinarily the numbers appear to be from 5 to 7 cm. in height. (d) The images normally appear to be at a distance of 35 to 40 cm. from the eyes. (e) The span of imagery includes about six figures with a definite preference for their horizontal arrangement. If, for example, a list of 200 numbers has been memorized, at any one moment any group of about six figures may be made to stand out clearly. (f) When the figures are visualized on a ground at a distance of about 1½ m., they are about 30% smaller and less distinct. Emmert's law of the proportionate variation of the size of the after-image with the distance of the projection ground from the eye seems to be reversed.

The general question of visual imagery necessarily includes a consideration of the so-called after-image. Several tests were undertaken with the purpose of investigating the behavior of this phenomenon. Inasmuch as this calculator manifested a definite color weakness, chromatic attributes could not be reported upon with certainty. The Ishihara test for color blindness revealed a decided deficiency for the four psychological primaries. The general weakness for color may account, in part, for the achromatic nature of the number of images even when the numbers are written with colored crayons.

Successive contrast was tested by having Mr. Finkelstein fixate squares of well saturated color on a medium gray ground for a period of one minute. The image was then projected on a large gray ground which could be moved toward and away from him. Mistakes were made most frequently in the naming of the hue of the after image, and less frequently in naming the presented color. In every case, however, the size of the image varied proportionately with the distance of the projection ground, thus fulfilling Emmert's law. Intermittent appearance of the after-images was, at times, spontaneously reported.

A final test of the effects of color consisted of the presentation of colored figures. Numbers of three digits, each digit about 6 cm. in height, were drawn on white paper. Mr. Finkelstein glanced at the figures, and then observed his imagery of them. Only the four psychologically primary colors were employed. The imagery of the numbers drawn in red, green, and blue behaved in the usual way, that is to say, they were visualized in his own handwriting and were achromatic. In the case of the yellow, however, as has been noted, the situation was quite different. The visualized figures were blurred with yellow, and their form resembled the original figures. Mr. Finkelstein described the experience as both unsatisfactory and decidedly unpleasant. In spite of the color weakness regularly shown, he has a decided preference for certain colors and a decided dislike for others. Yellow is in the group of unpleasant colors.

The high degree of definiteness and stability of the imagery of numbers regularly reported by this calculator necessarily raises the issue of its proper classification. The essential question is whether or not the phenomenon is properly designated as eidetic.

E. R. Jaensch has asserted that the eidetic phenomenon is intermediate between the after-image, AI, and the memory image, MI.[4] Being intermediate, the eidetic image, EI, displays characteristics of both AI and MI. In certain cases there is a preponderance of AI features, and such cases are correlated with the clinical condition designated as "T-type." The preponderance of MI features, however, is correlated with the "B-type."

G. W. Allport contends that the EI is, properly considered, a member of the general class of MI, and differs from the ordinary MI only in degree.[5] The evidence for either one of these views is best tested by examining generally accepted descriptive and functional characteristics of the EI, and relating these to those of the AI on the one hand, and those of the MI on the other. This is essentially what Allport has done. The more certain and prominent of the characteristics listed by Allport[6] may be applied directly to the data which have been collected from Mr. Finkelstein.

(1) Localization. The content of the EI is externally projected and seen. The fulfilling of this criterion is evident in the ready determination of the optimum distance of the projection plane from the calculator's eyes, and in the perceptual character of the imagery which was regularly the writing of chalk on a freshly washed black-board.

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[4] E. R. Jaensch, *Eidetic Imagery,* 1930, pp. 1-73; Über Eidetik and typologische Forschungsmethode, *Zsch. f. Psychol.,* 102, 1927, 35-56.

[5] G. W. Allport, Eidetic imagery, *Brit. J. Psychol.,* 15, 1924, 99-120; The eidetic image and the after-image, this JOURNAL, 40, 1928, 418-425.

[6] Allport, Eidetic imagery, *op. cit.,* 101-113.

(2) Richness in detail (Clearness). In the great majority of cases the EI is superior in this respect to both AI and MI. It has been noted that Mr. Finkelstein's imagery is sufficient to serve a definite reference function. He habitually reads numbers from his imagery.

(3) Persistence. The EI not only persists through long intervals, but may be revived at will by voluntary effort. It was found that Mr. Finkelstein could revive his imagery after long intervals of time. A memorized square of numbers, for example, was repeated by columns two hours after the original learning. This was accomplished by reference to imagery. It may be noted, however, that there exists for him a definite retroactive inhibition, so that the recall of a list of numbers may be inhibited by the memorization of a subsequent list.

(4) Selective tendencies. The capacity for arousing EI is correlated with the interest in the material concerned. The emotional and aesthetic appeal of numbers and their capacity for arousing a wealth of associations could be conditioned by nothing else than a heightened interest. When objects which are relatively uninteresting, such as letters of the alphabet, are visualized, the images are both smaller in size and less distinct.

(5) Flexibility. Details of the EI may be manipulated. Imagined figures are added, subtracted, multiplied, and handled in any way the calculator desires in the course of long and complicated calculations. Furthermore, he is able to call on large groups of imagined figures whenever they are needed.

(6) Invariability. The EI does not follow Emmert's law as does the AI. It has been noted that the imagined figures decreased in size as the projection ground was moved away from the calculator, hence their behavior was the reverse of Emmert's law.

(7) Displacement in space. The EI is subject to spatial displacements such as being turned upside down, and reversed from right to left. This phenomenon was occasionally manifested in the reversal of the order of two memorized numbers. It never occurred, however, when the imagery was reënforced by definite associations.

(8) Conditions of its arousal and disappearance. Allport considers the following points: (a) Time of presentation necessary for arousal, AI>EI>MI. (b) Rigid fixation necessary for arousal, AI>EI>MI. (c) Possibility of voluntary recall, MI>EI>AI. (d) The best projection ground for the EI is in most cases a homogeneous gray corresponding in brightness to retinal gray. (e) The EI may disappear suddenly, gradually, or by piecemeal. Mr. Finkelstein's images of numbers are sharply distinguished from the AI in accordance with these criteria. There is an inevitable difficulty, however, in distinguishing the usual images of numbers from the MI. This calculator cannot satisfactorily make the separation since the accustomed imagery has become spontaneous, and it habitually appears. He was able to obtain the images with or without his eyes closed and upon a gray projection ground. Ordinarily the imagery occurred freely without extraneous aid. No definite manner of disappearance of the imagery was reported.

The application of the eight criteria definitely indicates that Mr. Finkelstein's images of numbers may be classified with certainty as EI. But the data go further in that they show in a striking manner that the essential technical requirements of the EI may be amply fulfilled, and yet the imagery may fail to show any of the salient features of the AI. The analysis lends support to Allport's conception of the EI as being a special variety of the MI. The difficulty in relating the conditions of arousal and disappearance of the number images to those of the MI must be interpreted as giving validity to this characterization of the EI. Application of the essential criteria indicates a type of imagery that differs from the usual MI only in degree, that is, in vividness, stability, and flexibility. Only one definite feature of the AI has been noted, namely, the external localization. Even here, as Allport has pointed out, the usual *dinghaft* attitude of the *Eidetiker* may account for the external quality of the imagery so that the evidence of the localization of the EI in perceptual space is relatively uncertain.[7]

In summary we may state that the examination of Mr. Finkelstein's images of numbers indicates: (1) the imagery serves a reference function in the mental manipulation of figures; (2) the images are eidetic; and (3) the EI are phenomenally and probably genetically related to the MI, but essentially unrelated to the AI.

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H. Barry, Jr.

Auditory Action Currents

In a recent review of the subject of electrical phenomena of the ear, Kreezer summarized the various papers which had appeared on the subject up to July, 1932, and, after pointing out the various pitfalls and physical artifacts encountered in the experiments which were reported, came to the conclusion that on the basis of the available evidence no one had conclusively proved the existence of auditory action currents.[1] We believe, however, that in view of the evidence summarized below, much of which was not then available, that there can no longer be any doubt that such action currents have actually been obtained by most of the workers in this field. The action currents in question have the same characteristics as those recorded from peripheral nerves and from other tracts of the central nervous system, such as the optic or proprioceptive.[2] They meet all the criteria used as tests of true action currents.[3] The doubts and difficulties in positively identifying action currents in the auditory tract, as opposed to other tracts, were largely due to the fact that when a sound is applied to the ear two entirely distinct electrical effects are produced: (1) a disturbance originating in the cochlea and spreading thence through all of the tissues of the head; and (2) true action currents associated with nerve impulses in the acoustic pathways.[4] Of course the action currents can also be detected at a little distance from the nerve itself, with adequate electrodes and amplification, just as the electrocardiogram is recorded from the limbs. The fundamental distinction between the cochlear phenomena and action currents lies in their respective sources.

The cochlear "spread" is most intense as recorded from the round window,[5] although it is also relatively strong in the subarcuate fossa and in the internal auditory meatus. At other points the intensity seems to depend primarily on the distance from the inner ear and the electrical resistance of the intervening tissues. The latency between the arrival of a sound wave in the cochlea and the appearance of spread anywhere in the head has not yet proved measurable, but is less than 0.1 σ.[2] The phenomenon is very resistant to narcosis, persists (although with reduced intensity) many minutes after death, and reproduces the frequency of the stimulating sound up to at least 5000 per sec. The cathode ray oscillograph shows that the spread also reproduces the wave-form of the stimulus above 800 per sec., although at lower pitches the response may be more complex.[6]

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[7] According to Jaensch's classification Mr. Finkelstein might be regarded as a 'pure' B-type.

[1] G. Kreezer, this Journal, 44, 1932, 638.

[2] L. J. Saul and H. Davis, *Trans. Neurol. Soc.*, in press.

[3] Saul and Davis, *Trans. Am. Otol. Soc.*, 22, 1932, 137.

[4] Saul and Davis, *Arch. Neurol. and Psychiat.*, 28, 1932, 1104.

[5] E. D. Adrian, *J. Physiol.*, 71, 1931, xxviii.

The action currents, on the other hand, are localized in the auditory tracts, are more intense in the larger tracts and centers than in the 8th nerve (despite the proximity of the latter to the cochlea), show the usual sensitivity to local or general anesthetics, and disappear just before or instantly at death. They follow the frequency of the stimulating waves up to but not over 900 or 1000 per sec. and with higher frequencies of stimulation appear as scattered asynchronous impulses. They travel with the usual speeds of nerve impulses, and appear on the cathode ray oscillograph as waves of shapes and time relations characteristic of action currents in peripheral nerves.

These two effects can be separated and studied independently, the spread by leading off from the round window, where it is very intense and where little or no action-current component is encountered, and the action currents by using highly localizing concentric electrodes,[7] choosing tracts at some distance from the cochlea, and suitably adjusting the intensity of the stimulus and the degree of amplification.

In regard to the possibility that the electrical disturbances might arise from vibration of the electrodes relative to the tissue with resulting changes in the electrical characteristics of the circuit, we may mention one of the numerous controls which we have employed. With electrodes in place in an auditory pathway we touched the handle of a vibrating tuning fork to the clamp supporting the electrodes. This must have caused the electrode to vibrate strongly with respect to the tissue, but only a barely perceptible response was obtained, less than when the fork touched the table or the stethoscope leading to the cat's ear, and obviously referable to stimulation by the clearly audible sound in the room. A movement of the electrode of 2 mm. to a point giving very feeble responses to auditory stimulation abolished even this response.

It is not the intention of this note to go into further details of technique and of controls, and many of the statements in the foregoing outline are to be regarded as first approximations. We wish primarily to point out the chief cause of previous difficulties; namely, the presence of two distinct physiological effects. We are not acquainted with any work on this problem which is in conflict with this interpretation except for the totally negative results of Kreezer and Darge,[8] which we are quite at a loss to explain. The results of the other workers are readily reconciled on the basis of differences of technique, chiefly in degree of amplification used and the location of their electrodes. In literature, as in our personal discussions with Adrian, Wever, and Crowe and his associates, we have found all of the results to be mutually confirmatory and attesting the reality of auditory action currents.

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HALLOWELL DAVIS
LEON J. SAUL

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[6] Unpublished observations.
[7] E. D. Adrian and D. W. Bronk, *J. Physiol.*, 67, 1929, 119.
[8] G. Kreezer and H. Darge, *Science*, 75, 1932, 105.

A CHILD'S SPONTANEOUS REPORTS OF IMAGERY

At various times my older child, Henry, has made a number of 'observations' spontaneously, indicating that a child five years of age and even earlier not only has, but may even report on, phenomena which are commonly regarded as belonging to the realm of the trained introspectionist. How generally children have reported on these various types of images and sensations, with the exception of so-called eidetic images which have been extensively investigated, I do not know. The fact of interest in the present case is that the reports were given without any previous training, without the child's attention having been directed to them, and without questions or the formality of an investigation. The nature of the reports and the circumstances attending them are as follows.

Negative after-image. While riding in the car with me on a sunny, cloudless day, Henry, then aged 3 yr., 2 mo., remarked as we entered a wooded stretch of road: "I still see the sun when I look at the trees, only now it is dark, much darker than it was." Obviously he had been looking at the sun and the trees furnished a favorable background for the after-image to appear since the rest of the landscape and sky were rather brilliant.

Eidetic image. While passing through one of the rooms of the house when he was about 4 yr. of age, Henry stopped and broke out with the following: "Do you know, daddy, I can see pictures on the wall in here if I look at it and think." I asked him if he could see the pictures outside and he answered in the negative, and upon being asked if he could see them at night (which for him meant after dark, even in summer) he replied: "No, not outside and not when it's dark. Only when it's not too light or too dark; only when it is just right." I have not verified his observation that the eidetic image appears best under medium to shady illumination since the report was made without solicitation and so the fact must be accepted as valid, at least for this child at that particular time.

Dream images. From the time he was a little over three years of age Henry has reported dreams. Since no record has been kept of these reports I shall not try to summarize the nature of the dreams. This must be a common and easy type of report to get with children since my other child, now 3 yr., 5 mo. old, also reports her dreams, but has never duplicated the observations of Henry with respect to any other type of imagery.

Synesthesis. The observations showing synesthetic experiences have been made within the last two weeks, and like the others, have been given without the least suggestion. While engaged in play on the lawn with a number of other children (I was reading on the porch), the name of one of the children "Elsa" was called. Henry spoke up and said: *"That sounds like dark blue."* Upon my trying to question him further concerning other possible color-sound combinations the other children, much amused at his remark, immediately parodied him in mentioning names and colors so I gave up the attempt and said no more. About two weeks later after having been vaccinated (the vaccinating needle having been inserted well under the skin) he remarked as he left the doctor's office: "It hurts still; *a sort of pinkish hurt.*" This was at the age of 5 yr.

After-images of movement. Several months ago Henry made the statement that if things go too fast you don't see them moving at all. In attempting to get him to explain what he meant his exposition made use of the following illustration: "If you move your hand fast, just like this (suiting the action to the word), you see a whole lot of fingers but some of them don't move." This observation was probably first made as a result of a device used by Henry and his playmates to illustrate speed of movement. A slow motion of a train or animal is shown by moving the hand and with index finger extended, and faster motions with the whole hand thrown across the field of vision. Probably during one of these fast motions he had noticed the multiplication of images which he used to illustrate his assertion concerning fast movement.

Paradoxical heat sensation. At the age of 4 yr., 6 mo., in attempting to reach the faucet, Henry suddenly jerked his hand from under the spigot and wiped it. Thinking he had received a drop of hot water on his hand, I chuckled at his discomfiture. His come-back was: "It felt hot, but I *know* it was cold." All of this happened within a second or two after he reached for the faucet. I believe he knew the stimulus was cold after touching it with the other hand, just as a second, more deliberate touch assures us of the 'real' temperature of a stimulus under similar conditions.

It would be gratuitous to attempt to answer an hypothetical critic's objections that these responses merely represent 'verbal reactions' or special conditioning of some sort. It has not been our custom to discuss synesthesis, negative after-images, and the like at home. Moreover, if we did, I fail to see how a child of his age could grasp what these terms could mean unless he had the experiences corresponding to them. I have purposely refrained from asking questions or making suggestions. As a rule the facts are seldom mentioned again by Henry. They have the appearance, judging from the condition under which they have been reported and the manner of report, of being clear, novel experiences which are mentioned for these reasons. The reports have been in the nature of exclamations similar to those one hears from children the first time they visit the zoo or the circus or get a new toy. The nearest thing to psychological experience of any extended kind which he has had was the Stanford Revision, given after most of these reports were made. That it is not a matter of conditioning appears certain in view of the fact that his sister, as noted above, has not reported experiences which he did at the same age. Nor do I believe it is a matter of intelligence for the other child actually received a rating 11 points higher on the Stanford Revision, given by a member of the Education Department (not the father!).

There is no reason why children should not have these various types of experience at a very early age, possibly soon after birth. It seems to be largely a matter of getting the report under conditions excluding suggestion or any other contaminating influence. I have thought it worth while to record this case because I am certain of the spontaneity of the reports and their freedom from any sort of outside influence.

Bryn Mawr College HARRY HELSON

A NOVEL EXPERIMENT WITH THE NEGATIVE AFTER-IMAGE

Children in India find pleasure in a little pastime they call "Seeing the big white man." The child stands in the sunshine and fixates a small object in his shadow for several minutes. Then he looks at the sky. A very large white image of the child's shadow will be seen against the sky. In the blazing sunlight of India this after-image is quite clear and distinct and persists for some time. Even small children obtain it readily.

This experiment illustrates in a novel way how the distance of the projection background affects the size of the negative after-image. First year psychology students performing this experiment should never forget the principle of distant projection.

NORMA V. SCHEIDEMANN

June Etta Downey: 1875-1932

With the death of June Etta Downey on October 11, 1932, psychology lost a distinguished contributor and the University of Wyoming one of its outstanding scientists.

Dr. Downey was born on July 13, 1875, in Laramie, Wyoming, the daughter of the founder of the University of Wyoming, one of the state's most distinguished pioneers. She received her B.A. from the University of Wyoming in 1895, her M.A. from the University of Chicago in 1898, and her Ph.D. from Chicago in 1907. With the exception of two trips abroad, and summers at Cornell, Chicago, Iowa, and the Boston Psychopathic Hospital, her entire career was spent at the University of Wyoming. From 1898 to 1902 she was an instructor in English, from 1902 to 1905 an assistant professor, from 1905 to 1915 professor of English and philosophy, and from 1915 to her death professor of philosophy and psychology and head of the combined department. From 1924 to 1926 she was coöperating investigator in the National Research Council migrations research. She was coöperating editor of the *Journal of Applied Psychology,* a fellow of the American Association for the Advancement of Science, in the starred list of *American Men of Science,* a member of the Society for Experimental Psychologists and of the American Psychological Association. From 1923 to 1925 she was on the Council of Directors of the American Psychological Association. She was also a member of Sigma Xi, Phi Kappa Phi, the Colorado-Wyoming Academy of Science and the American College Quill Club.

Her earliest publication, *A musical experiment,* in this JOURNAL in 1897, reflects an interest in aesthetics that was to continue throughout her life. Subsequent publications in aesthetics (except for the study of the effect on a musical program of the familiarity and sequence of selections, published in 1927) were centered in the literary field and were concerned with such topics as: the imaginal reaction to poetry, literary self-projection, emotional poetry and the preference judgment, a program for the psychology of literature, and the psychology of figures of speech. Two books: one, *Plots and Personalities,* written jointly with E. E. Slosson (1922) and another, *Creative Imagination* (1929), also show this interest.

Her interest in the field of sensation and perception is represented by several papers on imagery and synesthesis. An article on three generations of color-blindness was published in 1931.

Dr. Downey's interest, however, lay in motor rather than sensory phenomena. Her Ph.D. thesis, *Control processes in modified handwriting: an experimental study,* done under the direction of James R. Angell and published in the *Psychological Monographs* in 1908, was the first of numerous papers dealing with handwriting, automatic phenomena, muscle reading, the reading and writing of mirror script, writing under distraction, the retention of writing skill after lapse of practice, handwriting disguise, and pen lapses. Later out of these studies were to come the will-temperament tests with which her name is most prominently identified. From 1910 to 1924 she summarizes the literature on handwriting for the *Psychological Bulletin* under the title *Graphic functions.* From the studies of writing under many varying conditions, she moved on to analyses of family resemblance in handwriting, judgments on the sex of handwriting and judgments of similarity and difference in handwriting. In a book, *Graphology and Psychology of Handwriting* (1919), much of this work is summarized and evaluated in relation to the literature. Her researches on handwriting led to her studies of handedness and dextrality types, a recurrent interest represented by a number of papers. In the period from 1917 to 1920 she published several papers on intelligence tests. This was never a major interest with her, however, except insofar as intelligence is related to personality.

In 1919 her first publication on the will profile appeared, a term she soon changed to *will-temperament.* There followed a number of studies exploring the possibilities of will-temperament testing, studies which culminated in a book, *The Will Temperament and Its Testing* (1924). Here she distinguished three personality types: a fluidic, speedy, hair-trigger type, a dynamic, aggressive, forceful type, and a slow, deliberate, inhibited type. Dr. Downey viewed personality as an integrated whole and objected to abstracting a trait from its setting. Hence she placed emphasis on a plotted profile rather than a single measure. The will-temperament tests attracted much attention and resulted in much research by both Dr. Downey and other workers. Although their subsequent use and evaluation has revealed decided limitations, they may be regarded as pioneer efforts to measure personality and as contributions that have stimulated and carried forward scientific thinking.

Throughout her entire career Dr. Downey was a tireless worker despite limited facilities and a heavy teaching load. Her scientific publications as listed in the *Psychological Register* number 76, of which 2 appeared in her third decade, 18 in the fourth, 31 in the fifth, and 25 in the sixth. Besides she was the author of 29 popular writings including poems, plays, short stories and articles. A volume of her poems was published in 1904. She was also the author of *Alma Mater,* a University of Wyoming song. She also wrote many book reviews both for scientific and popular journals. In 1927 she published *The Kingdom of the Mind,* a book for children and younger readers which presents the results of experimental psychology in popular form.

Her teaching was marked by brilliancy, breadth of interest, fertility of ideas and an unbounding enthusiasm for psychology and its significance for human affairs. Much of this enthusiasm was contributed to her students. Despite her literary, musical, and artistic interests, she consistently emphasized the importance of the laboratory and of a scientific approach to the complexities of human behavior. Always there were interesting things going on in her laboratory, many of which did not find their way into print. Her success in securing the interest and coöperation of undergraduates in psychological research was striking. Unfortunately at Wyoming she had access to but few graduate students.

Personally Dr. Downey was quiet and retiring, somewhat diffident on first acquaintance, but possessing rare charm, an alert, original and stimulating mind, and a kindly and deep human feeling.

University of Minnesota JOHN E. ANDERSON

Edgar James Swift: 1860-1932

Edgar James Swift was born July 24, 1860, at Ravenna, Ohio, received his bachelor's degree from Amherst College in 1886 and for three years, 1889 to 1892, traveled and studied in Europe, spending some time with Wundt at Leipzig and more with Ebbinghaus and Paulsen at Berlin. From 1895 until 1900 he was at Stevens Point Normal School and it was here that he began his psychological and educational publications, eleven original articles appearing during this time.

In 1900 he entered the graduate school at Clark University where, under the direction of G. Stanley Hall, his interest in these subjects was intensified and all the more, perhaps, because he and Professor Hall did not wholly agree upon important conclusions. During his three years at Clark University he published ten more studies upon psychological and educational subjects. It was in Worcester that he met and, in 1906, married Claire Martha Coburn.

In 1908, five years after taking his doctorate, his first complete volume, *Mind in the Making,* appeared. His next two books, *Youth and the Race,* published in 1912 and *Learning by Doing,* 1914, went further into detail in regard to pedagogical method and in his *Psychology and the Days Work,* published in 1918, many of the principles are restated and re-related to each other, other subjects are added and rearrangement makes it obvious that the book was intended primarily for the general public. These books made a deep impression upon those interested in education as Dr. Swift's letters plainly show. Miss Helen Parkhurst, who organized the Dalton School, freely acknowledges her debt to him and the ideas behind the Dalton School have influenced American education very greatly and are still influencing it. The specific modifications introduced with that system have, in other schools, been extended and changed, but Dr. Swift's great contribution to the movement must be acknowledged.

His next book appeared seven years later in 1925. In the interim he had published fifteen original articles, many addressed to the public through the medium of *Scribner's Magazine* and some through trade journals, but there were also such experimental studies as that on the "Perception of Filled and Empty Time." The 1925 book appeared with the title, *Business Power through Psychology,* but it reappeared shortly afterward under the name, *How to Influence. Men.* This book frankly deals with the application of psychological principles to modern business and constitutes a very real effort to meet the public upon their own ground, to humanize psychology and to disseminate psychological knowledge in a form available for practical application.

Dr. Swift returned to his original interest long enough to republish his *Youth and the Race* under the title *Psychology of Youth,* 1927, and to complete the subject and bring it up to date with the *Psychology of Childhood,* published in 1930. During this same time he was also preparing a work which was to carry a fight offensively against what he considered ignorance and superstition. Out of this book he got the most fun and perhaps in it he put the most work. *The Jungle of the Mind* was published in 1931. It is as much a philosophy as a psychology; in it he scorns the fine-spun arguments of scientists and philosophers about beginnings and finalities and spends his effort against what he regards as the most common fallacies, the beliefs which most retard the advance of civilization and which may ultimately defeat it: common credulity, the tendency to build logical systems upon emotional bases, the common belief in magic and the common denial of the universality of natural law. His thesis is that a superficial stratum of elegance cannot successfully be laid over an essentially vulgar attitude toward life.

All of these books were published during Dr. Swift's connection with Washington University for, upon receiving his doctorate from Clark University in 1903, he had accepted an appointment there, and until 1924 he was head of the Department of Psychology and Education. After the creation of separate Departments of Education and Psychology in 1924, he continued as head of the Department of Psychology until his retirement in 1931. He taught summer-sessions at Peabody College, 1914; the University of Wyoming, 1915; the University of Chicago, 1916; Kansas Agricultural College, 1917; the University of California, 1921; and he was Special Lecturer of Applied Psychology in the Post Graduate School of the United States Naval Academy at Annapolis from 1921 through 1927. He was a lecturer at the Naval War College at Newport, R.I., from 1921 through 1924; a fellow of the American Association for the Advancement of Science, a member of the American Psychological Association, Western Philosophical Association, Society of College Teachers of Education and Phi Beta Kappa. He died August 30, 1932, at Hollis Center, Maine.

Besides the seven volumes mentioned Professor Swift published, during his life, eighty-four articles and reviews. He wrote and worked earnestly and happily, to carry a message: the necessity of using what knowledge we have. He contributed much to the sanity and wisdom of his generation and presented his subject with strength and dignity but entertainingly, and his homely council was sane, simple, penetrating and practical.

Washington University JOHN PAUL NAFE

Ludwig Reinhold Geissler: 1879-1932

Ludwig Reinhold Geissler, a member of the faculty of Randolph-Macon Women's College since 1920, died on December 15, 1932, after a prolonged illness. Born in Leipzig, Germany, on September 22, 1879, he came to this country in his youth with his parents, who settled in Texas. It was there that he received his early training and education.

After his graduation from the University of Texas in 1905, he transferred to Cornell University for graduate study with Titchener. He received his doctorate from Cornell in 1909, and remained there one year as instructor in psychology. He then served one year as research psychologist at the Nela Park Industrial Laboratory, Cleveland, Ohio; four years as associate professor of psychology and education at the University of Georgia; and four years as assistant professor of psychology and education at Clark College. While at Clark he was also non-resident lecturer at Wellesley College. In 1920, he went to Randolph-Macon Woman's College as Professor of Philosophy and Education and Director of the Psychological Laboratory—the position that he held at the time of his death.

During the early part of his professional life Geissler was concerned with scientific and systematic problems; and his contributions during these years numbered fourteen papers, all of which were published in this JOURNAL. His studies upon attention[1] were systematically important, as they shaped the Titchenerian doctrine which was formulated during his stay at Cornell.

After his brief sojourn at the Nela Park Laboratories, his dominant interest became technological, and his studies and papers were directed upon the practical aspects of psychology. He was co-founder and co-editor with G. Stanley Hall and John Wallace Baird of the *Journal of Applied Psychology,* and during his editorship, which was discontinued when he left Clark for Randolph-Macon, he was a frequent contributor to its pages. During the latter years of his life, in spite of ill health and remoteness from the industrial centers, he contributed numerous articles upon the application of psychology to industry.

His technological articles were at first theoretical. His systematic training showed itself in his inquiries concerning the nature of applied psychology. As time progressed, however, his work became more practical and specific. From 1923 to the time of his death he was State Mental Examiner of Virginia, and from 1923-1929 he served as Psychological Examiner of the State Colony for Epileptic and Feebleminded.

Geissler was a member of the American Psychological Association and the Southern Society for Philosophy and Psychology, serving as president of the latter in 1929.

K. M. D.

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[1] Fluctuations of attention to cutaneous stimuli, this JOURNAL, 18, 1907, 309-321; A critique of Professor Wirth's methods of measurement of attention, *ibid.,* 20, 1909, 120-130; The measurement of attention, *ibid.,* 20, 1909, 473-529; The measurability of attention by Professor Wirth's methods, *ibid.,* 21, 1910, 151-156; Professor Wirth on the experimental analysis of consciousness, *ibid.,* 21, 1910, 489-499; Analysis of consciousness under negative instruction, *ibid.,* 23, 1912, 183-213.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Readings in Experimental Psychology. Edited by WILLARD LEE VALENTINE. Special readings by H. M. Johnson, Willis Carter Beasley, Dael Wolfle, and Helen Morrill Wolfle, and a foreword by Albert Paul Weiss. New York, Harper & Bros., 1931. Pp. xv, 606.

This book is written for the introductory course in psychology. It can be used as a supplementary text in courses consisting mainly of lectures and demonstrations but is better adapted to the needs of laboratory courses in which experimental methods are principally emphasized. It is a compilation of papers representative of recent experimental investigations in psychology. In selecting the papers no thought was given to the historical aspects of the science. They are intended to be a sampling of the work of modern American laboratories rather than a collection of classic experiments.

The compilation has been made to fulfill two needs. In the first place it is a source-book which introduces the elementary student to the original accounts of experiments which are treated "sketchily and at second hand in the formal texts." In the second place the book is intended to teach the student to analyze and present data of an experimental nature. In nearly all of the papers the treatment includes a statement of the problem, a presentation of the data, and a discussion of the results with conclusions. In preparing an original paper for inclusion, the editor's general procedure was to shorten it considerably by reducing detailed descriptions of apparatus and methods, excluding "remote generalizations," and "less direct allusions," and simplifying the language wherever necessary. In practically every paper tables and graphs are used for presenting the data.

Descriptions of apparatus, with supplementary drawings and photographs, are frequent throughout the book. Some of these need amplifying, being too brief for the limited understanding of the elementary student. Descriptions of experimental methods and techniques are usually restricted to the specific topic of the investigation in which they are found. An application of these methods to other possible situations would be of material assistance to the student (in his understanding) as to their significance. The explanation of the use of certain statistical constants in analyzing data is inadequate. The student is not given sufficient information to enable him properly to interpret data presented in the form of standard deviations, probable errors, and the like. Questions and references are appended to most of the papers.

In selecting the particular papers for the compilation the editor has not been prejudiced by any particular kind of analysis. He has very carefully excluded allusions to different schools of psychology and represents psychology as an experimental science rather than a number of "isms." The papers are presented under ten main headings. Experiments on reaction-time are placed first. They include studies of reactions to sound, light and touch with two papers on the personal equation. The conditioned response is then considered with papers on the pupillary and eyelid reflexes and a discussion of the time interval on conditioning, using the finger withdrawal response. Infant behavior is discussed under the titles: "Sensori-Motor Responses in Infants," and "Activities of Newborn Infants." Under the heading of internal stimuli and their responses are included two studies of hunger (one using human subjects and the other white rats), a study of the sex and hunger drives of the white rate and a paper on the obstruction method of measuring animal drives. Experiments on the differentiation of emotional responses in infants, and the differentiation of emotions by systolic blood pressure, together with a study of changes in pulse, breathing and steadiness under conditions of startled and excited expectancy are classed together under the topic of emotion. The topic of postural responses is divided into four subjects. There are three papers dealing with the subject of attention, two with delayed reactions in animals, one with sleep and two with hypnosis. Five papers are placed under the topic of discrimination. Experiments in audition, vision, and tactual localization are reported together with a discussion on "local signs as orientation tendencies." The topic of learning has been divided into two parts. In the first are found papers on the following subjects: animal drives in relation to learning, the mirror drawing experiment, a comparison of human adults and white rats in maze learning, the nature and elimination of unsuccessful acts, brain injury in relation to learning in the white rat, the learning curve for the complex process of generalizing abstractions, the relation between learning and the amount to be learned, and the relation between teaching and learning. The second set of papers deals with complex types of learning. There is one experiment on the multiple choice method as used with primates and three experiments on the evolution of concepts and the relative efficiency of different types of concepts. A paper on the experimental approach to the study of language is the only one to be included under this topic. The last four papers are classified under the heading "Social Studies." The first is a study of the relation between intelligence test scores and scholastic success; the next is an experiment on the use of photographs in predicting success and vocational aptitude; the third is a study of common annoyances and the fourth is a description of a method for measuring public opinion.

Although the writings of a great many individuals are represented in the book, the editor has been very successful in keeping the difficulty of the reading within the range of the elementary student. In only one instance has he failed to achieve this and that is in the experimental articles on vision and audition. A large amount of the content of these papers is sufficiently difficult for advanced courses in experimental psychology.

University of California C. W. BROWN

Handbook of Child Psychology. By JOHN E. ANDERSON and others. Ed. by Carl Murchison. Worcester, Clark Univ. Press, 1931. Pp. xii, 711.

The *Handbook of Child Psychology* represents a series of papers by the twenty-two authors, most of whom are well known for their research work in the fields which they discuss. The editor has apparently made no attempt to restrict the freedom of the authors in any way except by assignment of topics. Probably the most successful papers from the point of view of usefulness to those for whom the book will be of most service (advanced students, teachers of child psychology, and research workers), are those papers which critically analyze most of the important work in the particular field discussed. Such critical analysis involves the presentation of investigations and experiments in such a way that the reader has available information regarding (1) the problem which each investigator or experimenter was attacking, (2) the techniques or methods used in the experiment or investigation, (3) the results, and (4) an evaluation of the extent to which the reviewer finds the techniques and results adequate for the problem attacked. This method of review gives the reader not only a body of integrated information, but also suggests a variety of new problems, and methods to be used or avoided. Authors who have effectively used this method in the *Handbook* and the titles of their papers are: Bühler, "The Social Behavior of the Child;" Gesell, "The Developmental Psychology of Twins;" Goodenough, "Children's Drawings;" H. E. Jones, "Birth Order in Relation to the Development of the Child;" M. C. Jones, "The Conditioning of Children's Emotions;" Vernon Jones, "Children's Morals;" Klüver, "The Eidetic Child;" McCarthy, "Language Development;" and Peterson, "Learning in Children."

Two valuable chapters, the contents of which preclude the type of treatment mentioned above, are those by Anderson and by Lewin. Anderson discusses the history of child psychology briefly and describes methods or techniques which have been used, illustrating with typical experiments. Lewin's paper, "Environmental Forces in Child Behavior and Development," presents a well worked out "system" of child psychology and is a challenge to current theory and method.

Woolley discusses problems of eating, sleeping and elimination, at some length, drawing largely from empirical evidence. Hers is a discussion which should be of considerable value to individuals concerned with the training of infants in these biological functions. Another topic, the inclusion of which in the *Handbook* may be pushing the boundaries of subject matter too far afield, but which makes interesting reading, is Isaacs' description of a super-nursery school or kindergarten. However, there would probably be no exact agreement among child psychologists about what topics should be allotted to a handbook on child psychology and about the relative amount of space that should be devoted to them.

Several omissions seem apparent to the reviewer. The extremes of the distribution of intelligence are discussed in papers by Terman (The Gifted Child) and by Pintner (The Feebleminded), but there is no space allotted to intelligence testing *per se*. This would seem a real omission, especially in view of the comparatively recent but great amount of work which has been done in testing infants and preschool children. Another topic practically untouched in the *Handbook* is that of clinical psychology, *i.e.* with reference to the type of work being done in the many child guidance clinics throughout the country. Several papers skirt the edges of this subject, namely, Anna Freud's "Psychoanalysis of the Child," and Kimmins', "Children's Dreams," but nowhere is there a discussion of the problems attacked by clinical psychology, the methods and results obtained. Such a chapter would be of more practical value than a discussion of the hypotheses of one school of psychoanalysis or of a statistical analysis of the dreams reported by 5000 school children.

At the close of each chapter there is a list of the references used by the authors. These total to some 1300 to 1400 titles. In addition several of the authors have made available additional bibliography. The volume is very satisfactorily indexed with both name and subject indices.

University of California CAROLINE McCANN TRYON

Genius and Creative Intelligence. By NATHANIEL D. METRON HIRSCH. Cambridge, Mass., Sci-Art Publishers, 1931. Pp. 339.

This book is ambitiously labeled: "A Psychological Foundation for Twentieth Century Political and Sociological Theory and Action" (fly leaf). The main arguments are in favor of the theses: (1) that, in the study of man, the most important undertaking is the study of genius; and (2) that genius is scarcely human (preface).

The author contends that the genius is the greatest single factor of social destiny (preface), though adequate demonstration of such a causal relationship is difficult to see from the data presented. The genius is pictured as of delicate health in early life and often in maturity (p. 318)—(no data are given for the generality!); as precocious mentally and arising to full maturity much later than the average (p. 318); as neither psychotic nor neurotic, yet prone to minor mental disorders (p. 319); as a poor writer, because of a lack of nervous balance, and as lacking in motor coördination (p. 319)—(no comparison is made with non-genius and there is no evaluation of practice and training); as lacking in procreative power (p. 320)—(the insinuation is that he is undersexed—one is led to wonder whether the literature on the sex life of so many men of genius is all wrong); as bashful in childhood (p. 321); as over sincere (p. 322); and melancholic (p. 322). Genius seeks solitude as a defense against the mob (p. 325); has intense faith in himself and his mission (p. 328); and is a biological mutant (p. 320). Statistical analysis forms no part of the base for most of these conclusions. "The genius differs in *kind* from the species, man. Genius can be defined only in terms of its own unique mental and temperamental processes, traits, qualities and products. Genius is another psycho-biological species, differing as much from man, in his mental and temperamental processes, as man differs from the ape" (p. 298).

Unfortunately the term *genius* is never concisely defined by the author. "His origin is biological. Great men are not made by environment, potential great men are often destroyed by it" (p. 38). But just what constitutes genius, what the term should stand for, is not stated. That social appraisal, at some time or other, is essential to the detection of genius is scarcely recognized by the author and that the criteria of evaluation may not be constant for differing social structures seems not to have been considered.

Part I is an introduction to the nature of man—brief in its treatment, but positive in its conclusions. Frequent recourse is had to the teachings of William McDougall—*elan vital* and all! The importance of production and protection of genius is stressed. Since genius, in the opinion of the author, is a mutant, why concern ourselves with its production? So far as the reviewer is aware, all attempts to predict the nature of mutations have been unsuccessful. Dangers of over-populating are feared. Because those who live to be 65, 70, 75, or 80 today do not survive as long thereafter, on the average, as those who lived to those ages a century ago did (when only the very hardy attained those ages at all), it is concluded that a man who lives to those ages today may not expect to live as long as he would had he lived a century ago (p. 51). The differential birth rate is noted and decried (p. 70). Conclusions are drawn from data covering, at best, a 30-to-50-yr. period—certainly a short period of time in which to determine what birth-control information will do for a population. Contraceptive information may reasonably be expected, and the reviewer believes, be shown to become the property of the more fortunate classes socially, intellectually and financially first, and from them spread downward. If this is so, no one can predict the effects of that information as it becomes knowledge to those considered of lesser social value.

In Part II intelligence is said to have three dimensions: (1) the cognitive aspect of instinct; (2) a sensori-motor, perceptual and reproductive intelligence; and (3) a symbolic, analytical, verbal and conceptual type. Throughout this discussion there is much with which most psychologists can agree and a great deal with which many will find fault. Qualitative differences are made distinct from quantitative ones (a distinction undefined and difficult to follow), and are made an argument against the single-factor theory of general intelligence (p. 111). Inadequacies of present tests and testing methods are emphasized. The conclusion seems to be that since certain improvements have not been made they cannot be made. It is claimed that non-verbal tests do not differentiate above the 9- or 10-yr. level—a conclusion scarcely in keeping with the findings of Edward B. Greene at the University of Michigan (Non-Language Series, Ann Arbor, Edwards Bros.).

Too many things are taken for granted through the discussion of the book. The reviewer is somewhat taken aback by the unsupported statement that "beauty, with intelligence, character, and initiative is declining" (p. 80); that puritan New England had high morals and intellectual poverty (p. 127)—(what are morals anyway?); that ability to use one's native language is unaffected by environmental factors, but ability to use a foreign language is affected (p. 141); that the mouse possesses an instinct of fear (p. 117); that man created the intellectual gap between himself and the other animals (p. 171); that sex is man's greatest frailty (p. 79); and so on *ad infinitum*. This book is altogether too ambitious for the data presented, or, for that matter, available.

Western Kentucky State Teachers College MILTON B. JENSEN

A Personnel Study of 10,000 Iowa High School Seniors. By JOSEPH RAYMOND GERBERICH. University of Iowa Studies in Education, 5, 1930, (no. 3), 1-117.

This monograph is a report of the results of the Iowa High School Survey inaugurated in 1923 primarily for the purpose of securing data for use in educational guidance. Two tests, the Iowa High School Content Examination and the Iowa Comprehension Test, were given during all of the 6 years covered by the study. The Thorndike Intelligence examination given during the first 3 years was replaced thereafter by parts of the Iowa Placement Examination series. The reliabilities of the tests employed vary from 0.76 to 0.97. The results of 6 years of the survey are presented, with ample explanations and discussions, in 50 tables.

Large differences in the test performance of various schools are attributed to probable differences in educational efficiency and the eastern section of the state is compared with the western much to the advantage of the former. That the performance of the larger schools, more prevalent in the eastern section, markedly surpasses that of the smaller schools, more frequent in the western section, is held not to invalidate the sectional comparison inasmuch as the east has more fairly large cities than the west. The reviewer's conception of a fair regional comparison would be one based on schools of the same type in each region. Probably the same differences that were found between the performance of eastern and of western schools would have appeared in a similar comparison of large with small schools in either section. Again it is quite possible that small rural schools, even if more efficient educationally than large city schools, might still fall lower in test performance because of inherent differences in mentality.

Sex differences in performance on individual tests favor the girls in all the English tests and the boys in all others. These differences are statistically significant in 16 of the 21 comparisons, 12 of these 16 being in favor of the boys.

Correlations between age and the various tests and composites ranged from —0.13 to —0.49, the relationship being only slightly higher in the case of general ability tests than of achievement tests generally. From this fact the author concludes that the older less able students receive more than their rightful share of attention at the expense of the younger more capable ones. However, this fact may as well be explained on the supposition that selective elimination of the less studious and less highly motivated dull students leaves only the more scholarly members of the duller group in school at the senior level whereas no such selective factor is operative among those of high intelligence.

The amount of selection between high school graduation and college entrance is indicated by the following figures which show the number who entered college from each "decile" (author's terminology) of the survey test group beginning with the lowest tenth and advancing to the highest: 10, 17, 20, 31, 26, 20, 35, 34, 44, 79. The median percentile of those who entered the University of Iowa as freshmen was 69.2; for those who entered the second and the third year, respectively, it was 74.9 and 78.1. No sensible gains occurred beyond this point.

In view of the high correlations (0.65 to 0.75) between first semester grade averages and scores on the Iowa Placement Examinations reported from previous studies it is a little disappointing to find that the replacement of the Thorndike Intelligence Examination by the Iowa English Aptitude and English Training examinations, raised the multiple correlation coefficient only from 0.563 to 0.586.

A bibliography of 156 titles is one of the many useful features of this interesting monograph.

Kansas State College JOHN C. PETERSON

Growing Up in New Guinea: A Comparative Study of Primitive Education. By MARGARET MEAD. New York, William Morrow, 1930. Pp. 372.

A six month's trip to the Admiralty Islands was undertaken with the intention of collecting data for three contributions to Anthropology: first, the book under review; secondly, an article on the relationship between spontaneous animism and the thinking characteristics of mentally immature persons (refererred to on p. 289 of the "New Guinea" book); and thirdly, an essay by Dr. Fortune, the author's husband, on the ethnology of the Manus (p. 293). The last two of these seem to be the more serious studies, whereas the first was written in a more popular style, perhaps not without an eye on the financing of future field trips. Although the book possesses certain pretensions to detailed anthropology, its general argumentative and impressionistic manner keep it from demanding extensive attention in a psychological journal.

Mention, however, should be made of two of the author's general attitudes towards her material. In the present book, she has done for the Manus what she did in her previous book for the Samoans in order to carry further her comparative study of various cultures. This was done in the hope that such study would yield more exact information about what may be considered as inherent and what purely cultural in the nature of man. Properly used, the comparative method is unquestionably one of the most profitable for anthropology, but in practice it is also one of the most dangerous, since there is the omnipresent possibility of identifying aspects which are only superficially similar. In reading Miss Mead's long comparisons of the Manus culture with our own, one feels that she has not avoided this danger.

Secondly, her chief working hypothesis, implied and stated at various points throughout the book (cf. p. 279), is open to some question. This is that a primitive society presents such a unified and simple picture that even a total outsider may go into the community and within six months completely understand it. On this point, there is justification for the most profound disagreement. Even if it be granted that a primitive culture is more simple than ours—a point which men like Rivers, Radin, and Leroy might be expected to deny—the mere understanding and evaluating of a culture other than one's own presents such grave difficulties that it is surprising to find such a view definitely stated by an investigator with as much insight as Miss Mead (p. 5). If anthropological field-work could extract so readily the essence of a foreign culture, it is quite amazing to find trained investigators mistakenly maintaining for sixty years that there was a mysterious animistic tendency inherent in primitive thinking, instead of promptly realizing, as does Miss Mead, that these manifestations could be explained simply on the ground of external cultural factors. That a "trained" field-worker may glibly analyse social customs and fit them into such categories as his training has given him would be readily admitted, but this superficial anthropology is not the complete understanding which should be aimed at—and in fact achieved if anthropology is ever to solve its problems. The mere collection of data under preconceived headings may delay this understanding when those headings are ill-conceived, since it permits the formulation of theories, apparently valid because based on a tremendous number of "facts," but actually quite false, since the facts in question come rather from the reaction of the field-worker to a strange culture than from an adequate interpretation of the essence of that culture.

Cornell University R. S. HILL

The Laws of Feeling. By F. PAULHAN. Translated by C. K. Ogden. New York, Harcourt, Brace & Co., 1930. Pp. xiv, 213.

This is an essay which first appeared in the *Rev. Philos.* in 1884. After four revisions it now appears in translation together with a supplementary essay on Feeling, Intelligence, and Will (*Rev. Philos.*, 1920). Two arguments prepare the way for the general law of feeling, viz., (1) that man is an ensemble of organs united and imperfectly harmonized by one of the organs, the nervous system, and (2) that psychological study is a way of studying the functions of the brain. All affective phenomena, from the most ordinary emotions to the highest and most complex feelings, are the result of arrested tendencies to action, that is, of "more or less complicated reflex action which cannot terminate as it would if the organization of the phenomena were complete" (p. 17). The general law of feeling then is: "An affect is the expression of a more or less profound disturbance of the organism, due to the fact that a relatively considerable quantity of nervous energy is released without being able to be used in a systematic manner. An arrest of the tendencies aroused and a number of physical or psychical phenomena of various kinds, are then produced. At the same time, one or several or all of the following phenomena always appear: persistence of tendencies, relative lack of coördination and sudden appearance of the phenomena produced, and the tendency of the awakened impulse to monopolize the field of consciousness" (p. 57). This general law is then applied to the production of three classes of affective phenomena, viz., (1) passions, feelings, affective impulses and signs, (2) affective sensations, and (3) emotions. In a discussion of compound affective phenomena, Paulhan almost casts his vote in favor of mixed feelings. A short conclusion relates the study of emotion and the principle of arrested tendencies to the general problem of psychology.

The burden of the supplementary essay is to be found in the argument that feeling, intelligence, and will are not separate facts and, much less, products of three independent faculties. On the contrary, "there is no fragment of mental life, however insignificant, which does not exhibit all three of them at work," their separation being due only to the fact that we are impressed more strongly now by one and now by another of them (p. 204).

University of Illinois COLEMAN R. GRIFFITH

Mental Tests and Heredity, including a Survey of Non-Verbal Tests. By BARBARA SCHIEFFELIN and GLADYS SCHWESINGER. New York, Galton Publishing Co., Inc., 1930. Pp. ix, 298.

This book has two purposes. The first and more general one is to outline the problems involved when mental tests are used for the purposes of eugenics. The second is to survey as completely as possible the non-verbal tests which have been offered at various times for use in appraising "general intelligence." The first part of the book (18 pages) is devoted to the historical attempts at measuring individual differences and to definitions and theories. Part II outlines analytic studies of environmental factors and investigations of mental inheritance in their various settings and approaches. Part III, the largest division of the book (175 pages), is devoted to opinions and experimental data on different sorts of tests, especially verbal and non-verbal, whether for individual or group application; the interpretation of statistical data; and an annoted survey of tests available for the purpose of the authors, as stated. Verbal tests which may be used practically as non-verbal or may be given in different languages without the necessity of re-standardization are included. These tests are classified under the heads of learning; memory; sensory; sorting; special ability and mechanical puzzles; constructive ability; picture; construction; formboards; mazes; recognition of form, color or weight; spatial relations; and group tests. Under each specific test are data on origin, materials needed, directions, responses to score, standardization, reliability and validity, discussion and references. The information consists in quotations from articles and books, and also from personal letters received by the authors. Drawings of materials are occasionally given. The last part of the book summarizes, draws conclusions and makes recommendations; again these are given largely in the form of quotations or summarized statements from the testers themselves. The work seems to have been done with much care, although there are of course minor errors here and there, and mere quotations often do not represent an author correctly. In such cases the personal letters are helpful.

Useful material and suggestions are found in the appendices. In the entire book different views are assembled not to exhibit disagreements, but to present the picture of the developments and the status of tests as they actually are. The book will doubtless be found a useful tool in the hands of professional psychologists as well as a guide for special workers in the field of eugenics. This will be evident when one considers the origin of the work. It was projected by the Eugenics Research Association, a committee of which—including Clark Wissler, C. B. Davenport, H. H. Laughlin, Charles G. Campbell and Frederick Osborn—outlined the original project and turned over the details of the work to a committee of the Carnegie Institution of Washington under whose auspices the authors worked. This committee consisted of A. V. Kidder, C. C. Brigham, C. B. Davenport, Leslie C. Dunn, H. H. Laughlin, Rudolph Pintner, E. L. Thorndike, and Clark Wissler.

J. P.

The Mind in Action: A Study of Motives and Values. By A. CAMPBELL GARNETT. New York, D. Appleton & Co., 1932. Pp. xii, 226.

Despite the contradiction between the wrapper and the title page of this book, the psychologist soon discovers that the author is really a philosopher. He has taken viewpoints of Ward and Stout, James' self, McDougall's instincts, Spearman's cognition, a modicum of *Gestalt,* some interesting ideas of his own, and developed them into a verbally logical system of sources of motivation and value.

Innate motivation is in terms of need for activity, pleasure and seeking, pain and avoiding, appetites for food, air, sex, and so on—thirteen "instincts pure and simple" (p. 112), and three instincts which are also appetites. The latter half of the book is concerned with acquired sources of action including habits, the self, sentiments of love, loyalty, respect and hatred; and abstract ideas or higher values of beauty, morality, truth, and religion.

Many formal definitions are enunciated. Instinctive behavior is defined as "the pursuit, with more or less foresight, of an innately determined end in response to the cognition of a meaningful situation" (p. 22). Cognition is involved in all instinctive behavior; the latter is always more or less intelligent. Appetites are innate wants which recur periodically in connection with the normal functioning of the organism. Both instincts and appetites are dependent upon structures in the organism and they are essentially the same except that in the case of instinct the structure determines a type of response to a type of extraorganic stimulus, whereas in that of appetite the structure determines a type of intraorganic stimulus. This seems to the reviewer to be a valuable distinction.

"Motor habit is the acquired tendency to respond with certain movements to a certain type of conation" (p. 120). It is of secondary motivational value, being static relative to conative habit. "Conative habits are instinctive responses to the acquired meaning of objects and situations" (p. 120). Begging by a dog when hungry is given as an instance of the former, while attacking by a dog whenever he sees a cat illustrates the latter.

The self with its expansions and appropriations is made to play a very large part in the development of the sentiments and the so-called higher values. Sentiments are produced through the acquirement of meanings by the self or subject. Values are developed through efforts to develop and maintain systems of relations. The 'self' or finite center of experience plays its part by "continually enlarging its cognitive and conative grasp or penetration of the reality in which it is immersed" (p. 211).

The paragraphs above aim to suggest how the text, as it proceeds from beginning to end, gets increasingly philosophical. Thus an author might start out with the best of intentions but the weight of his major interest might become progressively effective and so produce a gradient in his writing. The actual treatment is 'armchair' almost throughout and to the experimentalist reader it is extremely uninteresting. There is a theoretical integration but it contains no new facts. Explanations are in such general terms as conation, cognition, self; adduced evidence is at the anecdotal level. Though a jig-saw puzzle has been completed, too many of the pieces are imaginary. One feels that for the writer the words are sometimes realities and that faculty psychology is breathing. The most serious difficulty of all is the fact that this work, like so many others, stands almost completely apart from specific present-day experimental results.

The book makes little significant contribution to scientific psychology, and definitely it seems not to belong in a contemporary library of psychology with a dual criterion of popularity and scientific accuracy. The systematist and the self-psychologist, however, may study its organization with interest.

Yale University S. M. NEWHALL

Religious Realism. A series of fifteen articles contributed by various contemporary theologians and philosophers. Edited by D. C. MACINTOSH. New York, The Macmillan Co., 1931. Pp. viii, 502.

This book is a decidedly important presentation of contemporary opinion in regard to the fundamentals of philosophy and religion. It is also a possible starting point of new and significant discussion, new methods of approach, and even of possible experimentation, in connection with some of its problems. The articles are perhaps the kind of thing one might hope to get should the American Association of the Advancement of Science add a section on theology to one of its divisions. The greatest variety and freedom are exercised by the various authors, both in their choice of topics and manner of treatment, yet the book succeeds in its endeavor to present a common, objective, point of view, and it is properly entitled "Religious Realism."

As stated in the preface, religious realism means the view, "that a religious object, such as may be appropriately called God, exists independently of our consciousness thereof," of which, "it is possible for us to gain either (as some would maintain) adequately verified knowledge or (as others would be content to affirm) a practically valuable and theoretically permissible faith." The articles cover a wide variety of topics. Some of the most interesting are, "The Implications of Human Consciousness," by J. B. Pratt of Williams College; "A Phenomenological Approach to Religious Realism," by J. S. Bixler of Smith College, in which some characteristic ideas of Husserl, Scheler, Plato, and the Gestalt School are used in support of the thesis that the world, "as revealed in phenomenological intuition is a series of data which, in the religious field, are *a priori* true and, for the emotions, are *a priori* valid." "Religious Realism and the Empirical Facts of Religion," by

A. G. Widgery of Duke University, presents an interesting, partly historical, sketch of religious developments in widely separated religious groups, in each of which, he considers, is to be found some measure of objective truth as to the nature of the divine. "Plato as a Religious Realist," by R. L. Calhoun of Yale, is a well documented, but not very convincing, attempt to show that Plato's philosophy comprised many things which it possibly did not contain.

While there is a good deal in *Religious Realism* that may have bearing and effect on psychology, especially if the realistic movement continues to develop, and while the problems and discussions which the book contains are of considerable general interest and value, there is but a limited amount of material in the book which is of direct and immediate interest to the professional psychologist.

Hobart College FOSTER P. BOSWELL

Psychoanalyse und Pädagogik. By ERNST SCHNEIDER, Pädagogisches Magazin, Heft 1303. Langensalza, Beyer & Sons, 1930. Pp. 72.

This monograph is divided into three parts which are preceded by a brief historical introduction. In the first part is presented a series of "difficult pedagogical situations" together with psychoanalytical comments on each. These cases include such pedagogical problems with reference to children and adolescents as petty stealing, specific speech difficulties, difficulties with the multiplication table, etc. The second section contains an exposition of the theory of psychoanalysis, and the third part presents a brief statement of the value of psychoanalysis for pedagogy. There is appended a brief bibliography (forty titles) of psychoanalytic works in German of interest to the teacher.

The place of psychoanalysis in the theory and practice of pedagogy is indicated by the author's assumptions regarding the functions of pedagogical technique. Such technique, he defines, as the method of leading the individual toward a systematization of personal and super-personal interrelationships. The educated man must have achieved order within his personality and have integrated himself with society. Re-education becomes necessary when any degree of disorganization appears. This disorganization is indicated by the "symptoms" or disturbances which occur as the individual passes through the various stages of psychical development. These "symptoms" represent the attempt of the unconscious to substitute an unsatisfactory type of organization when the normal course of development has been blocked.

The value of the psychoanalytic technique for the teacher is found in the insight which it affords into a definite group of disturbances which hinder and endanger education as defined above. The symptoms of a diseased personality may be remedied by psychological and pedagogical methods directed towards the study of the unconscious. In addition the insight furnished by psychoanalysis will modify the attitudes of the teacher and hence his methodology so that he may be able to identify symptoms and prevent conflicts. Finally, psychoanalysis offers itself as a method which will remove repressed conflicts in teacher and pupil. Analytical procedures may be undertaken by the teacher himself when minor difficulties only are involved. More complete analyses must be carried out by the professional, doctor or pedagogue, who has no other relationship to the patient than that of analyst.

Critical comment appears to be superfluous. The author presents little that is new. The exposition of psychoanalytic theory follows closely the Freudian pattern, and the applications to pedagogy are of the conventional type. The "difficult pedagogical situations" closely resemble the case studies with which the clinical psychologist is familiar under the term "behavior problems." For those persons to whom the basic assumptions of psychoanalytic theory are valid, the applications and point of view in the present monograph will be acceptable. For those to whom psychoanalytic theory is an interesting but experimentally unsupported group of concepts, the present monograph will offer evidence which is no more convincing than that presented in a score of publications during the past twenty years.

Northwestern University FRANKLIN FEARING

Speech Pathology. By LEE EDWARD TRAVIS. New York, D. Appleton & Co., 1931. Pp. xxxiv, 331.

This book presents a summary of much of the work which has been done by the group of workers who are interested in the influence of cerebral physiology upon the speech mechanism. It incorporates, with his own research findings, pertinent clinical and neurological observations, which are of much academic interest as well as of considerable clinical significance. The author holds that the neural mechanism of speech depends on a partial balance between the two sides of the cerebral cortex, in which one side must be dominant in order to have a proper coördination. When the individual is unable to allow his natural tendencies to express themselves in such forms as normal right or left handedness, there is interference with speech. The long exposé of the physiology of stuttering makes the book worthwhile in itself, presenting graphic evidence of defective synchronization of the thoracic and abdominal movements, tremors and tone-rigidity in the voice, abnormally long duration of tones, periodical fluctuation of breath-sounds and bizarre voice-wave forms to demonstrate cerebral influences. The author indicates briefly his findings regarding the abruptness of the emission of tones and repetition of sounds, words, phrases, as well as speech blocking. There is presented a summary of the influence of stuttering upon the patient with reference to secondary changes in his personality and social adjustment, and Travis' own opinion about the cause of stuttering. In the chapter on articulation and formation, as well as that on aphasia and kindred disorders, current thought on these subjects is briefly summarized. An appendix of 50 pages, consists of lists of stimulus words and sentences for speech training. Altogether, the book is an excellent and much needed contribution to the study of the mechanisms of speech. Considering the highly specialized nature of Professor Travis' research, the rather serious criticism of omitting any considerable discussion of the psychogenic factors in speech disorders can well be minimized.

Institute for Juvenile Research, Chicago LOWELL S. SELLING

An Experimental Study of Apparent Movement. By SUGI MIBAI. Psychol. Monog., Vol. 42, No. 190. Princeton, Psychological Review Co., 1931. Pp. 91.

This monograph contains the report of a series of experiments on apparent movement, as attacked by a new technique. The first part of the work is devoted to an historical summary of the findings of previous investigators, and the experiments are described and discussed in the later and major part of the study.

Mibai selected an Eastman kodascope as a suitable apparatus with which to demonstrate apparent movement. According to Eastman, this instrument has a speed of 16 sections of film per sec., but Mibai found that the instrument used in his work had a speed of about 19 sections of film per sec. The length of pause between the two still presentations, which when made in proper temporal succession provided the conditions for apparent movement, was varied by changing the number of blank sections of film which were interpolated between the initial and the terminal picture. The two pictures were presented in different parallel and angular patterns in order to determine the influence of arrangement upon apparent movement. As compared with pieces of apparatus used in previous investigations on apparent movement, this apparatus offered the advantage of allowing repeated exposures of the pair of still pictures to be made.

Three problems were the particular objects of study. First, back and forth movement or movement back and forth from first and second positions. Secondly, rotary motion, especially the illusion of seeing a moving wheel spinning in the direction opposite to that in which it is going. And thirdly, comparison of apparent speeds.

True continuous movement was apparent when the two members were separated by 70 to 130σ. With a separation of 310σ, there was always mere succession of the pictures without apparent movement, and with a pause of 250σ succession was usually reported. Apparent movement was more clearly seen when the two successive members were presented in an angular than in a parallel arrangement. Conical movement, involving perception of the third dimension, was the rule when the angle made by the two members was greater than 90°. With novel patterns, movement tended to lack definiteness of direction even within the optimal conditions for apparent movement. Self-rotation, or rotation of a bar on its own axis, was reported when the temporal separation was short and when the distance between the exposures was small. The gray patch or flash, suggested by Dimmick as the correlate of *Phi,* was found to be very uncertain; it was not observed by all subjects, and was not consistently observed by the same subjects.

With regard to the illusion of seeing a wheel spinning backward when the movement is actually forward, it was determined that there were indifferent areas in the middle of the spaces between the spokes. When the spokes fell on these in succeeding exposures, the movement of the wheel appeared to fluctuate; when they fell on the spaces between the original positions and these areas, the rotation appeared to be clockwise; but when they fell beyond these areas, counter-clockwise movement was perceived. By increasing the number of spokes, the indifferent areas became smaller, and the correct direction of rotation was more favorably seen.

The significant findings in regard to comparison of apparent speeds were these: (1) fast motions were more accurately judged than slow motions, (2) speed tended to be judged in terms of time rather than in terms of distance, and (3) the speed of the second member tended to be accelerated.

Wellesley College MICHAEL J. ZIGLER

L'âme des Bêtes: quelques pages d'histoire. By L. VERLAINE. Paris, F. Alcan, 1931. Pp. 202.

This is a very readable history of animal psychology from the time of the ancient Greeks up to the end of the 19th century. The general theme of the book is based on the ancient contention that the major difference between animals and man is that the former are governed by instincts, while the latter are controlled by intelligence; or, in other words, that man has a soul or mind, a faculty which animals lack. During many centuries this idea seems to have been the basis for most studies in animal psychology, the works of those days consisting mainly of accounts of the marvels of animals instincts, and frequent controversies on the problem of intelligence. The author shows how the different movements of thought, philosophical and at times even religious, have contributed to this early idea that animals differ materially from man, and how they have altered this concept until gradually, through the influence of scientific ideas in general and the development of scientific methods in physiology, psychological interest in animals changed to other problems. Some of these new problems were: Are they conscious? Do they think? In what physiological and psychological attributes do they differ from man and from each other as one goes down the scale of animal life? Along with this change of interest there appeared also a tendency to apply scientific and experimental methods to the study of these various problems.

This book is apparently an account of the development of comparative psychology from philosophical speculation. Verlaine's method, however, may be compared with that of an orator who carries his audience to the climax of his story only to inform it that this will be the end. He stops where the story really begins to get interesting. Little or nothing is said about the many experimental studies on animals which have been carried out by European and American scholars since animal psychology has become a branch of scientific psychology. The development of animal psychology in the United States is not even mentioned; nor are the many studies which deal with the learning ability of animals, the complexity and limitation of the learning capacity, and also those which correlate learning ability with structure in order to discover how the development of the sensory, motor, and nervous equipment influences the adaptive or learning capacity. As far as it goes, the little book offers interesting reading for the philosophical-minded reader. The material is interestingly arranged and presented for the avowed purpose of "restoring the philosophy of nature."

Chicago, Illinois C. Burri

Anger in Young Children. By Florence L. Goodenough. Minneapolis, University of Minnesota Press, 1931. Pp. xiii, 278.

This recent study of the child's emotions presents much interesting information relative to (1) the nature of angry behavior, (2) the frequency and duration of outbursts, (3) the conditions making for irascibility, (4) the immediate causes of anger, (5) the methods of control, and (6) parent-child relationships. The conclusions, carefully drawn and with an eye to their quite palpable limitations, are based upon reports involving some forty-five children ranging in age from seven months to seven years ten months. The records of behavior were made by parents on especially devised forms and cover periods of observation ranging from six to 133 days. These parents, because of their superior general academic training, were judged to be "at least equal in education and ability to the average of the technical assistants employed in most research laboratories" (p. 16). The results were pooled and analyzed in an attempt to bring out sex and age differences.

The most obvious criticism of the study is that the reliability of the basic data used was not determined. Some attention is devoted to this problem, but the fact that records were complete was usually used as a basis for inferring that they were accurate. The report would have been considerably enhanced had two individuals observed the same child independently for at least a day and then compared records for consistency of observation. Illustrations of the lack of reliability of the evidence are apparent in a number of instances. For example, Table 3 indicates that some mothers of babies seven months of age observed them constantly for almost exactly the same number of waking hours per day as did those whose children were more than seven years old. Furthermore, there was so little correspondence between the mothers' statements (from memory) as to how they handled anger outbursts, and their actual technics reported at the time (p. 211) that the reviewer wonders if even these latter assertions were not quite generally rationalized. While the author thinks this improbable (p. 212), it is by no means an unjustified inference in the absence of any objective evidence to the contrary. A second limitation from which the study suffers, and which is candidly admitted by the author, is the paucity of subjects considered. This is quite obvious in cases where an attempt is made to locate sex differences in behavior at different ages from the study of groups of not more than eight subjects.

Anger in Young Children is called an exploratory study—as such it has value. No new technics of observation were developed, but an attempt was made to standardize reports made by parents, with the apparent realization that we must turn to this source for much of our information about child life.

It is to be regretted that the study was not planned to include both a determination of the reliability of the data and enough subjects to permit of some substantial generalizations.

University of Nebraska STEPHEN M. COREY

Statistical Analysis of American Divorce. By ALFRED CAHEN. New York, Columbia University Press, 1932. Pp. 149.

This is an interesting analysis of the divorce records of the United States Census from the Civil War to the present time. In this period the approximate increase of population was 300%, of marriages 400%, and of divorces 2000%. The divorce rate for 1000 married population has increased about 500% with a comparatively uniform rate of increase over six decades. At present the probability of divorce is approximately 18% of all marriages, and in 1929 there were 201,468 divorces granted in the United States.

The author adopts the standpoint, so prevalent in social and economic science, that statistical data possess distinctive factual importance and quite rightly therefore confines himself to a statistical analysis. The almost inevitable result is that he can formulate few positive conclusions as to the basic explanations of, or possible controls over, the prevailing condition. His summing up is that; "the rapidity of increase in the American divorce rate is the product of changing social conditions. The equilibrium of the family institution has been shaken by the radical transformations in its economic and social environment." This is not very helpful.

The value of the study lies principally in the discussion of commonly accepted explanations for the prevalence of divorce. In general these are shown to be inadequate. Some of his findings in this regard are that legal restrictions have little effect upon divorce rate. Establishing residence in another state for purposes of divorce accounts for only 3% of total divorces. Only about one-third of the divorced people re-wed, which is only very slightly higher than for widowed persons. The urban divorce rate is about double the rural. Only 8% of marriages with children end in divorce as against 71% of childless marriages, and childless marriages account for 63% of all divorces. Economic production, urban growth, decreasing ratio of children and women workers "operate in the nature of concomitant or mutually interrelated forces as chief environmental causes in American family breakdown."

The study illustrates the limitations imposed upon an investigator who is dependent upon census data. These data are interesting but not very useful in bringing to light the basic influences that contribute to the prevalence of divorce in modern life. The necessary corrections and manipulations of the data are extensive but on the whole statistically justified. There is an extensive bibliography.

University of Toronto S. N. F. CHANT

Those in the Dark Silence: The Deaf-Blind in North America. By CORINNE ROCHELEAU and REBECCA MACK. Washington, Volta Bureau, 1930. Pp. 169.

In their study of "the most terribly handicapped of all human beings with intact minds, and the most neglected as a class," the authors have made the widest survey of this field that has so far been undertaken. They have recorded 665 living cases, with varying degrees of defect, from those totally deaf and totally blind, to those with minor degrees of either or both deficiencies. A practical classification in terms of causes is given: (1) the rare cases of congenital deaf-blindness; (2) the most numerous group—those who lost sight and hearing as a result of illness in infancy or early childhood; (3) the deaf who lose their vision and the blind who lose their hearing; (4) persons with normal senses who in adult life suddenly lose sight and hearing by illness or accident.

The first third of the book is devoted to an excellent picture of the status of the deaf-blind. Possibilities for pre-school treatment in the home are described, with suggestions for more adequate school training. In Europe there are already special institutions for the deaf-blind, but in America there is as yet no such provision for them. Until there is such special provision, the authors recommend education in schools for the deaf, in order to learn means of communication; 11 methods for conversation and 13 methods for reading and writing are described. Training can then be continued in schools for the blind.

The authors point out certain objections to "psychological diagnoses," until special intelligence tests for the deaf-blind have been developed. The results of tests not adapted to their special needs may destroy confidence in their potential abilities. There have been cases in which children have been consigned to institutions for the feeble-minded as hopeless custodial cases, although their later history proved them to be educable.

The problem of work for the deaf-blind is well presented, and suggestions made for the coöperation of state, church, and welfare societies. For example, various religious bodies might each assign one person to the special ministry of this neglected group. "One priest, one minister, and one rabbi could take care of them all," by correspondence with the individuals and with their local agencies when personal visits are impossible. Until a national institute devoted to their general welfare is established, the authors offer this practicable plan for bringing to the deaf-blind the pleasures of friendship, the consolations of religion, and contact with the outside world.

The latter and larger part of the book consists of a biographical section where notes on the cases are given, ranging from the mere listing of names or initials to accounts covering several pages. The remarkable attainments possible after proper home and school training make a forceful plea for the extension of adequate opportunities to all deaf-blind children.

The book is a valuable and timely contribution to the growing literature upon the handicapped.

Mount Holyoke College SAMUEL P. HAYES

Nightmare, Witches, and Devils. By ERNEST JONES. New York, W. W. Norton & Co., 1931. Pp. 374.

The main body of the book is divided into four parts. The major topics are respectively the Pathology of the Nightmare, Connections between the Nightmare and Mediaeval Superstitions, the Mare and Mara, and the Conclusion.

The book was written, mainly in 1909 and 1910, from the point of view of Freud's school of psychoanalysis, and the treatment follows the lines suggested by Freud in his early assumptions. For example, the statement is made that Nightmare is always an expression of intense mental conflict centering about some form of "repressed" sexual desire and that the repression concerns incest trends of sexual life. Similarly the claim is made that the Devil represents in the main an exteriorization of the repressed wishes which are ultimately derived from the infantile Oedipus situation. The Witch belief is also analyzed as an exteriorization in the main of the repressed sexual conflicts of women, especially those relating to the feminine counterpart of the infantile Oedipus situation. In short, the conclusion is that all mediaeval superstitions are of incestuous origin.

The point of view upon which the statements are based would neither be accepted by most American psychologists today nor by some of the "depth" psychologists. About 500 authors, not experimental psychologists, are cited to suit the purpose of the discussion and many of the fundamental assumptions are obviously hypotheses. But Jones passes serenly on without apparently realizing that his tenets are prejudiced or speculative in the least.

It will be difficult to determine the ultimate value of the publication before psychoanalysts develop their views further on some factual basis and before experimental psychologists do more in the way of examining the mechanisms involved.

Eastern Kentucky State Teachers College NOEL B. CUFF

Personality and Will. By FRANCIS AVELING. New York, D. Appleton & Co., 1931. Pp. x, 246.

To have personality or to be a person is, according to Aveling, to have a soul. This soul or profound self is given in immediate experience, especially during acts of will, and is characterized "as a substance, or entity subsisting in itself: as an activity immanently energizing, or originating and terminating its action within itself: as a being altogether incommunicable to any other being." The experience of this self is non-senorial or super-phenomenal, "a unique experience in which a noumenon may be said to be identical with its non-sensorial phenomenon," but withal an experience "which is as empirical as any experience can be." This soul is inseparable from the body and in fact necessary to the very existence of the body. The body is "an immense aggregate multitude of energies which are qualitatively, quantitatively, locally and temporally indetermined," while the soul is "a substantial activity, likewise energetic." The energy of the soul systematizes and integrates the indetermined energies of the body into a unitary living organism. "The soul is not the lute, nor the harmony of the lute; though lute and harmony are both involved in personality. It is that which constitutes the lute an instrument of harmony. Neither the body nor the soul is the person. The person or self is the organic unitary whole, the resultant of a multitude of energies, systematized by a constitutive principle which is conscious energy itself."

Such, in brief, is the thesis of Aveling's book. The problems dealt with—the nature of the soul, the manner in which it is revealed, and its relation to the body—are problems largely foreign to American psychology. The proposed solution to the mind-body problem is, as far as the reviewer knows, a new and original one, but one which is not convincing. In all biological structures there is a multiude of energies which are synthesized and integrated, but surely every such structure does not possess a soul or a conscious energy which integrates this multitude of part energies. The author's method is old; it names a faculty or soul, but fails to indicate how this entity does its work. Science is interested in the 'how.'

Stephens College CARL N. REXROAD

Clinical Psychology. Studies in Honor of Lightner Witmer. Edited by ROBERT A. BROTEMARKLE. Philadelphia, University of Pennsylvania Press, 1931. Pp. xxi, 409.

Of the two *motifs* recurring in and dominating this commemorative volume for Professor Witmer, one is, not unnaturally, Lightner Witmer. The other is that emphasis and stress on individual psychology as against statistics, on diagnosis contemplating the total personality, which is so much the clinical psychologist's way of thinking, but still relatively foreign to the interests of many followers of psychological science. The area of the clinical field covered by the 30 articles constituting the volume may be estimated from the list of topics considered: the preschool child, the superior child, mental deficiency, speech defects, psychobiochemistry, industry, diagnostic education, criminology, conduct problems, religious experience. Among the authors are Fernberger, Maxfield, Miles S. Murphy, Brotemarkle, Buzby, Viteles, Twitmyer, and Gault, while Professor Witmer himself is represented by three reprints, dated respectively, 1907, 1915, and 1925. The volume also brings into historical perspective a special sector of American clinical work, tracing its relationship to experimental psychology, through the Witmer paternity, to Wundt. Hardly so much stressed as present circumstances would warrant, is the effort to bring into similar perspective the position of clinical psychology in relation to the associated activities of psychiatry and social work. The coöperation with (and often lack of it from) school systems is more adequately treated. One should add that the trained clinical psychologist will find in the volume little that is new. Certainly little that to him ought to be new. The reader will find, too, much overlapping of thought and principles expressed, and of methods and objectives described. This is, however, almost inherent in a collection from so close-knit a group all writing around one central topic.

Not so inherent, in a group of scientific writers, should be the recurrence of that other mentioned *motif,* adulation not far from idolatry; father-introjection would be a psychoanalytic name. Professor Witmer offers in the following litany more concise expression of the theme his disciples are chorusing; (388 f): "Whenever I think of the dead weight of obscure words defining mystical metaphors, like the 'group mind,' . . . Whenever I consider the often equivocal sociological statistics which pass current as 'educational psychology,' or which impose I.Q.'s with a soothsayer's finality upon the ignorant and undiscerning, and . . . whenever I contemplate the mountainous labors which produce the usually ridiculous mouse called 'a coefficient of correlation,' . . . I pray for all the guidance necessary to avoid futility and dullness—enough analytic discrimination to make clear a few ideas, and the artists' choice of the best words in which to clothe them." Among these artists' words, be it noted, are "psychonomic personeering."

Boston Psychopathic Hospital SAMUEL J. BECK

A Mind that was Different. By DOW THOMPSON. Introduction by M. V. O'Shea. Oklahoma City, Harlow Publishing Co., 1931. Pp. xiii, 117.

Even though O'Shea says in the Introduction that this book "can be commended to the psychologist who is searching for reliable data bearing upon the traits of the introvert and also of the extrovert," and characterizes the author as an "enthusiastic *psychologist*" (italics mine), the book is really not scientific or psychological at all. It contains observations, interpretations, and opinions, and a number of clear rationalization processes to account for personality differences between the author and a brother. The former is characterized as introvert and the latter (who "made friends easily and naturally,") as extrovert. The account is traced in a general way through years of development and the author characterizes the different personalities of the brothers in terms of reactions to various situations confronted. The author came gradually to a realization, from readings and observations, that instead of being peculiar or 'different,' he was only like a certain class of individuals, whom—following C. G. Jung—he calls introverts. For him this is a permanent innate trait. There is little recognition that one may in different circumstances, despite any general disposition, show now introvert and now extrovert reactions. For the author, the distinction between these two types is so easily made that he proceeds—once he has characterized the types—to select a considerable number of historical examples. Some introverts selected are: Coolidge, Hoover, Theodore Roosevelt, Henry Ford, John D. Rockefeller, Sr., U. S. Grant, Ferdinand Foch, Edison, Charlie Chaplin, Benito Mussolini, Gustave Stresemann, etc. Indeed, it seems to him that most great leaders should be included. They are individualists, show a tenseness or need of relaxation, prefer to live by themselves, are not able to subordinate their opinions to those of others, manifest indifference to the views of others, etc. But often with great leaders (introverts) there are closely associated persons who get on well with them because they are their opposites, or extroverts. Thus with Pierre Curie was Madame Curie; with William James was his brother Henry, etc. Here the author made another 'discovery:' "the universality of nature's law that introverts marry extroverts" (p. 66). He even finds many analogies to this law in the inanimate world. Going into the matter of marriage between the two types, he found introvert-extrovert mates in his own immediate family, in his brother's family (his brother, although he had "kept company in his youth with extrovert girls, selected an introvert for his wife"), in his sister's family, in his father's family, and in many historical families. No; Nature's universal law is not *always* followed; a few persons make errors—due to other complicating factors—and consequently do not get along well with their mates!

Another discovery: Nature has provided that children in the same family alternate in type so that if the first is an extrovert the second must be introvert, etc. Altogether, though it is admitted that there are some weak introverts, the author seems to find comfort finally in the discovery of the type to which he belongs. A number of typographical errors occur in the book.

Unfortunately, this sort of enthusiastic 'psychology' is of no value even to the lay reader; and it may do him an injustice—particularly if he believes what he reads.

J. P.

Speech and Voice. By G. OSCAR RUSSELL. New York, The Macmillan Co., 1931, Pp. xvii, 250.

The bulk of this book is concerned with an interpretation of over 100 X-ray pictures of the vocal and articulatory mechanism. A phonographic record, synchronized so as to record the voice produced during exposure, was made simultaneously along with each X-ray picture. The outstanding single conclusion is that while the tone remains the same the cavities may be altered and that neither a single individual from time to time nor different individuals take the same position for a given cavity in order to produce a given tone. However the author does concede that some given characteristic seems typical of the position of the cavities when a given vowel is sung or spoken.

More than any other writer, Russell emphasizes the influence of surfaces (hard and soft), the false vocal cords, the epiglottis, the ventricles of Margagni and the glottal factor in tone formation. Narrowing of soft surfaces (lips, tongue, velum, faucal pillars, soft palate) mellows a tone and exposure of hard surfaces to tone waves makes the tone metallic. The former is accomplished by "deadening," and the latter by accentuating, the high partials. The false vocal cords and the ventricles of Morgagni operate in the creation of low-pitched guttural and hoarse tones. The epiglottis functions as a soft-surface filter to lower pitch and mellow tonal quality. Bony cavities (sphenoidal, frontal and maxillary sinuses) are of little or no importance.

There is a chapter on X-ray pictures as aids in training the deaf and several chapters on the physiology and anatomy of the peripheral speech mechanism.

Menville and Ane (*Proc. Soc. Exper. Biol. & Med.*, 29, 1932, 825-826) seem to have an improvement over Russell's mechanical means of outlining the forms and sizes of the speech and singing cavities. They used a preparation of glucose and bismuth. Parmenter and Trevino (*Quar. J. Speech,* 18, 1932, 351-369) have questioned all of Russell's X-ray findings. They suspect that Russell did not keep the subject in the same position for the different radiographs. Consequently comparisons from one picture to another for the same subject or for different subjects cannot be made. Further they found in contradistinction to Russell that each individual takes a fairly definite tongue position for each vowel. Regardless of how this controversy is settled Russell deserves much credit for his extensive investigations.

University of Iowa LEE EDWARD TRAVIS

Order of Birth, Parent-Age, and Intelligence. By L. L. THURSTONE and RICHARD L. JENKINS. Chicago, The University of Chicago Press, 1931, Pp. xiii, 135.

The first part of this book reports an investigation of the relation between the order of birth and the intelligence of children, and such other related problems as fecundity and intelligence, effect of sex on siblings, ages of parents and intelligence of children, and time interval between successive births and intelligence of children. These investigations were carried out on data collected at the Institute of Juvenile Research, Chicago. A sample of about 1500 cases in chronological order was drawn from some ten thousand case-records on file at the Institute. The mental ages and intelligence quotients used in the investigation were obtained by individual psychological examinations, generally with the Stanford-Binet examination.

The most important study is the first one named. In order to rule out the effect of social and economic status, it was limited to siblings. A comparison of the average I.Q. of first- and second-born in the same family, first- and third-born, etc., including eight birth-orders, shows a significantly higher average I.Q. and a greater variability of test intelligence for the later-born children. An examination of the internal consistency of the differences between the weighted means of various birth-orders involved in each comparison indicates a definite increase in intelligence with birth-order.

In the related problems studied, some of the more important conclusions are: (1) There is no definite relation between individual ages of parents at the time of birth of a child and the intelligence of that child, but there seems to be a tendency for children of parents widely separated in age to be mentally inferior to those of parents more nearly the same age. (2) Twins do not appear to be inferior in intelligence to their non-twin siblings, nor is there any difference in this regard between same-sexed and opposite-sexed twins. (3) There is seemingly a better prognosis for the behavior adjustment of a child that has an opposite-sexed next older or next younger sibling than of a child with a corresponding same-sexed sibling. The authors do not attempt to explain the results of these investigations, but certainly environmental factors need more careful examination.

Part II is devoted to a review of other studies bearing on the problems reported in Part I. There is a bibliography of 167 titles.

University of Kentucky E. J. ASHER

Delayed Reactions in Rats. By C. H. HONZIK. University of California Publications in Psychology, Vol. 4, No. 19. Berkeley, The University Press, 1931. Pp. 307-318, 1 fig.

Honzik tried out, in experiments on 21 rats, his idea that if rats, before the delay period, were allowed to run up to the stimulus this might not only increase the certainty that they actually received the stimulus but it would also serve to reënforce the stimulus and thereby possibly extend the delay period. A box with three curtained doors (two black and one white) was used. The white curtain, which was to be the cue to food via an elevated runway behind this curtain, was shifted in a random order according to a chance schedule. The animals were habituated to the situation before the experiment began. Motivation in the preliminary training to go to the white-curtained door was food for right and a mild electric shock for wrong choices. Then, after some overlapping of training and delay tests, the delay period was determined by letting the rats run up to the white-curtained door. Here their entrance to the food was prevented by a block attached to the lower end of the curtain. An opaque detention screen just before the curtain was then lowered, delaying the rat any predetermined period of time but leaving it free to run about in the box ahead of the screen. Meanwhile the white curtain and block were raised, bringing into position another black curtain (attached to the block) so that the rat, when released, had to choose among three similar black curtains of which only the one that replaced the white would yield admittance via the runway to the food. Various precautions were taken to prevent visual, auditory, or olfactory cues and to force the animals after the delay to use whatever other 'guiding cue' they might have. Delays of at least 45 sec. (possibly 3 min.) were found to be possible for most of the rats, with correct responses "somewhat higher than would be expected by chance." Two repeated studies by the author, involving slight modifications and new sets of animals, supported the results of the main experiment.

Differences between the behavior of successful and of unsuccessful rats are mentioned. "The better rats would often pause before a curtain or before two curtains in succession before finally choosing the correct one, the poor rats would undiscriminatingly poke their noses under the nearest curtain and very often return to a curtain that they had already tried" (p. 317). It is unfortunate that such observations are merely general, verbal statements more or less casually made, and that the reader does not have detailed data on other important matters so that he can analyze the results himself in an objective manner. Unless other experiments can present such details, obtained by objective and accurate technique, this interesting experiment will lose much of its scientific value.

J. P.

Policy and Ethics in Business. By CARL F. TAEUSCH. New York, McGraw-Hill Book Co., 1931. Pp. xi, 624.

This book is a plea for autonomy in business. The supporting argument is straightforward: (1) business, if controlled internally by proper ethical standards, should be exempt from interference from the outside; (2) to realize this ideal business must defend itself against forces within itself and also against other forces on the outside which, if not put under proper control, would cause it to operate at less than a reasonable profit—but the profit must be reasonable because it is bad taste to retain more than a reasonable profit; (3) in the achievement of the autonomy to which business is justly entitled, the constructive task will be the establishment of a standard of profit which will be defensible by the principles of ethics; (4) such a standard, safe for business and harmless to society, must be constructed through a complete knowledge not only of the intrinsic nature of business itself but also of all pertinent extrinsic factors such as the racial, geographical, religious, and social features of the people themselves. The book is not purely theoretical. At all points the argument is supported by a comprehensive structure of facts.

What the psychologist may glean from the argument and the facts will depend upon his own conception of what constitutes the psychological. There is no section of the book devoted exclusively to the psychological principles in business; but for those who find psychology in all human relations, abundant material is offered.

Wittenberg College H. G. BISHOP

The Experimental Study of Reading. By M. D. VERNON. Cambridge, Cambridge University Press, 1931. Pp. xv, 190.

The author attempts "to give a concise account of any experimental work . . . which throws some light upon the psychology of reading." This statement, however, indicates a more comprehensive treatise than is produced. In general, eye movements, perception, and legibility in reading are treated, with more space devoted to the first which is the author's own field of study. Methodology is emphasized. It may be characterized as a good book, but one which contains several inadequacies. Many of the analyses reveal alternations of fine critical insight and credulous acceptance of unwarranted statements or hypotheses. A lack of thoroughness is manifested by omission of both citation and discussion of many investigations whose consideration would have, in some instances, modified the author's conclusions. This is particularly true of the section on reading disabilities. The parts on visual perceptual processes in adults and in children, while adequately done, are somewhat superfluous and add little or nothing to the discussions which follow on visual perception in reading.

The critical reader will find much to commend in the book, especially in the discussions of various aspects of eye movements. One should mention in particular the important analyses of subjective accompaniments to variations in eye movements. The influence on ocular behavior of interest, relevant and irrelevant associations and imagery, and affective reactions, are cited from Vernon's own work. Among other things she rightly points out that a moderate kind of irregularity in reading movements need not be "inconsistent with the best kind of critical appreciation; and it may indicate the existence of illustrative thought and rational reflection."

University of Minnesota MILES A. TINKER

Englisch-Deutsch für Psychologen. By JAMES A. HAMILTON. Frankfurt a. M., Germany, Gaul & Bantelmann, 1931. Pp. 103.

This dictionary of English psychological terms, translated into the equivalent German terms, ought to prove particularly useful to foreign scholars who have a better command of German than of English. Some two thousand words are given and the book is printed in excellent Roman typography with the English words in black face capitals. The gender of the German nouns is indicated. The definitions though concise, restricted as they are for the most part to one or two equivalents, are, nevertheless, adequate. The reviewer, who is also the author of a German-English dictionary of psychological terms, realizes altogether too well the difficulty of having the first edition of this dictionary letter-perfect. But errors will creep in, as, for example, the misspelling of Ebbinghaus's name on page 24. There are quite a few terms in the list that are simple transliterations, like "shock" = "Schock." On page 76 there are at least six such simple transliterations. The compiler shows a considerable amount of psychological insight as well as a command of both languages. The booklet should be in the hands, not only of those psychologists who would become more familiar with the English language, but also those American and English students of psychology who wish to check back their knowledge of German terms. Few people who have not undertaken tasks of this sort can fully appreciate the tremendous amount of accurate and painstaking work which lies buried in the compilation of these hundred pages.

University of Iowa CHRISTIAN A. RUCKMICK

The Person of Evolution; The Outer Consciousness; The Outer Knowledge; The Directive Power. By W. D. Lighthall. Toronto, The Macmillan Co., 1930. Pp. 216.

This is a book of "studies" of consciousness in evolution privately published as *The Outer Consciousness* in 1926, but now including "several articles published in *The Philosophical Review* and other places." It is not a scientific book, the propositions being only the author's "own judgments" as stimulated by "the latest discussions of the best authorities on the several sciences" (p. 13).

The interests of the author are fundamentally theological, and the book is one of fantastic, untestable hypotheses. 'Instinct' is an independent drive in the individual from without. It operates toward 'paths of joy.' "throwing out as its organs, conscious individuals to pursue them" (*i.e.* these paths). This drive turns out to be "a Power conscious of what it 'drives' towards," God, "the ultimate Person of Evolution," etc. This conclusion is forced on the author by *"the ingenuity of the innumerable contrivances* which favor joy and avoid pain, through all the history of terrestrial life." Teleology, the transmission of acquired characters, etc., are assumed in the face of established scientific facts to the contrary, and consciousness is taken as a *vera causa.* There is an extensive literature today of this sort, distorting scientific facts to meet ulterior ends, or to follow personal feelings. Authors are not always so frank to tell the reader that they are giving merely their own opinions.

J. P.

Prediction Methods and Parole. By George B. Vold. Hanover, The Sociological Press, 1931. Pp. xvi, 138.

The author of this study has adapted the modern methods of rating and scaling to the problem of predicting the successful adjustment of paroled prisoners. As stated in the Preface this study "attempts to show how the information already in the parole records may be utilized for a more systematic and intelligent selection of the men to be placed on parole." The method was taken largely from the pioneer studies of Burgess and Glueck. The results, in agreement with the previous studies, indicate that no one factor is of outstanding importance in predicting violation or non-violation of parole. However, the author does find that a cumulation of factors, either weighted or unweighted, is quite significant. A scale is presented which has a practical value in that data on records of a parole department are used. This should be of special interest to those concerned with the general supervision of the parole of prisoners. The author suggests, as the logical next step in the development of a more scientific parole procedure, the application of the Experience-Expectancy parole violations at the time of granting the parole.

North Carolina State College K. C. Garrison

The Development of the Sexual Impulses. By R. E. Money-Kyrle. New York, Harcourt, Brace & Co., 1932. Pp. 219.

An excellent synthesis of sexual psychology from the viewpoint of psychoanalysis. In spite of its highly speculative nature and of the unverified assumptions, it should prove both interesting and valuable to those interested in the sexual factors in personality.

Trinity College R. B. W. Hutt

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WEIGHT-DISCRIMINATION WITH A VARYING STANDARD

By Herbert Woodrow, University of Illinois

Experiments in discrimination or comparison are usually conducted by repeatedly presenting to a subject pairs of stimuli, one of which, called the standard, remains the same in all pairs. It is possible, therefore, that the subject's judgment concerning the members of any pair may depend to a considerable extent upon the accuracy of his attunement or adjustment to the constantly repeated standard. The subject may become 'set' so as to be prepared for the presentation of the familiar standard, as either the first or second member of the pair. There are indications, such as the phenomenon of absolute judgments in experiments entailing series of weight comparisons, that these 'sets' play an important rôle in the mechanism of comparison. To obtain facts by which to estimate the importance and significance of this factor of set in the process of comparison was the purpose of the present investigation. It seemed that a fairly direct way of studying the matter would be to compare the results of series of comparisons in which the standard was kept constant with the results of other series in which from pair to pair the standard varied irregularly over a wide range of intensities. Obviously the subject could not become adapted or 'set' with respect to the standard if the latter varied greatly from one comparison pair to another; so that such an experiment should at least bring about variation in set—and to bring about variation in any factor under study is the first prerequisite to scientific determination of its function. Such an experiment was accordingly carried out, with lifted weights used as stimuli. With both the constant and varying standard the method of constant stimuli was employed, and in general the series of the two sorts were obtained under conditions so nearly equivalent that any difference in outcome could be attributed to the matter of constancy or variation in the standard.

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The data obtained with either the fixed or varying standard consist solely of the obtained percentages of reports of the three categories, heavier, lighter, or equal. These percentages yield two sorts of facts: one of these pertains to what may be termed the subjects' keenness or accuracy of discrimination; the other, and probably the more important, deals with the time-order error, that is, the difference between the percentages of correct reports concerning the same pair of stimuli depending upon the order in which the stimuli are given. It is believed that both sets of facts, particularly those concerning the time-order error, constitute important tests of the validity of any theory of the mechanism of successive comparison. The theoretical significance of these time-order errors has already been fully recognized by Köhler.[1]

METHOD AND PROCEDURE

As has been indicated, this study consists in a comparison of two procedures, the procedure with a fixed standard (the customary procedure) and that with a varying standard. A brief description will therefore be given of these two procedures and their differences.

With the first procedure two experiments were made, one with a standard of 110 grm. and the other with a standard of 200 grm. Each of 5 *Ss* was given 4 sittings of 100 trials each, in the case of both the standard of 110 grm. and that of 200 grm., a total of 800 trials with the fixed-standard procedure. Each was also given 800 trials, at 8 sittings of 100 trials each, with the varying standard. In the case of the varying standard, the standard varied irregularly from trial to trial by steps of 10 grm. from 110 to 200 grm. inclusive, and the trials at any one sitting included an equal number with each of the 10 standards (110, 120, 130, etc., to 200 grm.). Thus the varying standard took 10 different values, *i.e.* it consisted of 10 different standards, all used at any one sitting and mixed in an irregular order. Five variables were used in the case of both procedures, these variables differing from the standard with which they were compared by the following percentages: —3; 0; +3; +6; and +9. This asymmetrical and somewhat abbreviated series of variables sufficed for the present purposes and yet allowed a considerable saving of time as compared with a series containing as many variables lighter as heavier than the standard.

To equalize the effect of practice upon the two procedures, these were used in the order A A′ B B B B A′ A A A′ B B B B A′ A, in which A and A′ represent sittings with a fixed standard, A with the standard 110 grm. and A′ with the standard 200 grm., and B represents series with a standard varying from 110 to 200 grm. With both the fixed and varying standard, the order of standard and variable at any trial, from the first to the one hundredth or last, was the same at all sittings. The order in which the 5 variables succeeded each other was also the same for all sittings.

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[1] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forschung,* 4, 1923, 115-175.

In order to avoid any misunderstanding of the two procedures, the actual order in which the weights were presented will be indicated by listing the weights used during the last 10 trials of each day's sitting.

In the above table the weight of the variable is indicated as the weight of the standard plus or minus a certain percentage. If the absolute weights of the variables were given, there would appear only 5 weights in the case of either of the columns for the fixed standard; namely, the weights 106.7, 110.0, 113.3, 116.6, and 119.9 grm. in the case of sittings 1, 8, 9, and 16, and the weights 194, 200, 206, 212, and 218 grm. in the case of sittings 2, 7, 10, and 15. In the case of the varying standard, however, there would appear 50 different weights, since there were 5 variables for each of the 10 standards. Thus the variable appearing in the above table as 120+9% had a weight of 130.8 grm.; that of 180+9% a weight of 196.2 grm.; etc. The important point is that in all sittings corresponding trials always involved a comparison of 2 weights of the same relative or percentage difference and with the standard and variable given in the same order.

Standard....	Fixed				Varying	
Sittings.....	1, 8, 9, 16		2, 7, 10, 15		3, 4, 5, 6, 11, 12, 13, 14	
Order.......	1st	2nd	1st	2nd	1st	2nd
Trial 91.....	110	110+0%	200	200+0%	140	140+0%
Trial 92.....	110+3%	110	200+3%	200	170+3%	170
Trial 93.....	110+9%	110	200+9%	200	180+9%	180
Trial 94.....	110+3%	110	200+3%	200	200+3%	200
Trial 95.....	110	110+0%	200	200+0%	180	180+0%
Trial 96.....	110+9%	110	200+9%	200	120+9%	120
Trial 97.....	110	110+9%	200	200+9%	200	200+9%
Trial 98.....	110−3%	110	200−3%	200	150−3%	150
Trial 99.....	110	110+0%	200	200+0%	160	160+0%
Trial 100....	110+6%	110	200+6%	200	130+6%	130

The time elapsing from the moment when the *S*, after lifting the first weight of a pair, replaced it on the table to the moment he began lifting the second weight was approximately 5 sec.; and since the *S* used approximately 2 sec. to raise and 2 sec. to lower a weight, the time between corresponding phases in the two stimulation processes set up by any one pair of weights was approximately 9 sec. The time elapsing between the release of the second weight of one pair and the beginning of the lifting of the first weight of the next pair was approximately 10 sec.

The weights were made of shot and paper loaded in small, uniform bottles. In the case of the regular fixed-standard procedure, where the same standard was used repeatedly, 4 duplicate standards were employed in rotation so as to prevent identification of the standard by any possible change in its temperature or dryness as compared with the variables. The weights were lifted by being grasped by the thumb and the first two fingers in a manner carefully explained and taught to all the *S*s. The rate and height of lifting were not rigidly controlled by mechanical means, since such control is bound to introduce a distracting influence, but all the *S*s were given preliminary practice in lifting so that they took 2 sec. in raising the arm and 2 sec.

in lowering it. They also were instructed to aim at the greatest possible uniformity in the height of lifting, though this height was allowed to vary slightly in the case of the different *Ss*. Any deviations from regularity in an *S*'s lifting movements noted by *E* were immediately corrected by calling *S*'s attention to them. *S* sat in a comfortable position alongside a table, his elbow resting on heavy felt. Screens erected at the edge of the table prevented *S* from seeing his hand, the weights, or any of the manipulations of *E*.

The *Ss* were instructed to say "heavier," "lighter," or "equal," depending upon whether the second weight of a pair seemed heavier, lighter or equal when compared with the first. They were requested always to endeavor to make a judgment of heavier or lighter and to reserve the term equal for those cases in which they had a positive impression of equality, instead of employing it simply as a synonym for doubtful. In compiling the results half the number of equality judgments was added to the number of heavier and half to the number of lighter judgments. This procedure was followed merely in order to simplify the statement of the outcome of the experiments. The conclusions to be drawn would not be impaired had the equal judgments been considered separately or even entirely ignored. With the fixed-standard procedure there was a total of 492 or 12.3% of "equal" reports; with the varying-standard procedure, a total of 699 or 17.5%.

RESULTS

So far as the main trends are concerned the data obtained may be shown by Tables I–III, in which the results for all 5 *Ss* are pooled. No substantial exception to the general trends indicated by the pooled data is shown by the more detailed results by individuals given in Tables IV and V.

Table I is exceedingly simple and needs little explanation. It shows in what percentage of the trials the reports indicated that the variable seemed heavier, in each of two cases; namely, that in which the variable came first and that in which it came second. Since *S*'s report always referred to the second weight, there were no reports of "first heavier;" but it is possible to interpret the report "second lighter" to mean, or to be the equivalent of, "first heavier," and thus to determine in what percentage of trials the variable was indicated as the heavier when it was the first of the pair, and to compare this percentage with that obtained when it came second. It is necessary merely to remember that by the number of trials in which the variable was judged the heavier of the pair when it came first is meant the number of trials in which actually the standard, being second, was judged "lighter" (plus, as already stated, one half the number of "equal" judgments).

One other point in need of explanation is the manner of tabulating the results in the case of zero difference between the standard and variable. Since the distinction between variable and standard is entirely arbitrary in this case, there is no good reason for distinguishing between the case of variable first and that of variable second. For the sake of uniformity in tabulation, the tables show, in the case of zero difference, under the heading "V 1st" in what percentage of the trials the first was indicated to be the heavier, and under "V 2d," in what percentage the second was reported heavier. To illustrate: in one-half the trials a bottle labelled V was given first, and in the other half of the trials such bottle was given second. The results were as follows, standard 110 grm.: bottle labelled V given 1st, first indicated as heavier in 41% of the trials and 2d indicated as heavier in 59%; bottle labelled V given 2d, first indicated as heavier in 39% of the trials and 2d indicated as heavier in 61%. It follows that the 2d was heavier on the average in (59 + 61)/2, or in 60%, and the first heavier in (39 + 41)/2 or in 40% of the trials. Thus the percentages given in Table I for zero difference and standard 110 grm. are, for V 1st, 40, and for V 2d, 60. This procedure has been followed in the case of all three tables. It results in the fact that the number of cases on which the percentages in each cell of a table are based is twice as great for zero difference as for the other differences, though the number of cases on which each row of a table is based is the same for all differences.

The form of tabulation adopted enables one to see at a glance, in the case of all variables, how much oftener the variable was indicated as heavier when it came first (column V 1st) than when it came second (column V 2d). The difference in the frequency with which the variable is reported heavier, according to whether it precedes or follows the standard, gives the time-order error. Thus, according to Table I, the variable weighing 3% less than the standard of 110 grm. was indicated as heavier than the standard in 28% of the 200 trials in which it came first and in 45% of the 200 trials in which it came second. In other words, the reports varied as though the same variable were heavier when second, or following the standard, than when first, or preceding the standard. Since, however, the variable was the same, the differences in its effect upon the reports must be due to the change in its temporal position. When the reports indicate that the variable seemed heavier when second than when first, *i.e.* when the percentage of "heavier" trials is greater in the case of the variable being presented second, the time-order error is labelled negative. In the reverse case, *i.e.* when the variable influences the reports as though it were heavier when given first, the time-order error is labelled positive. In the tables the difference in the percentages due to temporal position are given in the column headed "D," which stands merely for 'difference;' but this difference may be used as a quantitative description, though not as an absolute measure, of the direction and magnitude of the time-order error.

Table I, representing the outcome of the two experiments with the orthodox fixed-standard procedure, does not, before comparison with Tables II and III representing the outcome of the varying-standard procedure, bring out anything of particular interest, unless it be that it shows in somewhat emphatic form the existence of a pronounced negative time-order error. In the case of both standards, and every *S* and every variable (a total of 50 cases), the time-order error was negative. On the average, the time-order errors with the two standards are very similar. The percentage of 'heavier' reports with the variable second exceeds that with the variable first, taking the average for all variables, by 18% with the standard 110 grm., and by 19% with the standard 200 grm. There is some tendency for the size of the time-order error to decrease with the increase in the size of the difference between standard and variable. Of course with very large differences the time-order error should disappear, since the judgments should be nearly always correct irrespective of the order of sequence of standard and variable. It is noteworthy, however, that the time-order error is still considerable with a difference (+9%, standard 200 grm.) which, when the variable comes second, gives as high as 97% correct judgments.

TABLE I

DATA WITH CONSTANT STANDARD SHOWING THE EFFECT UPON THE PERCENTAGE OF REPORTS SIGNIFYING VARIABLE HEAVIER OF PRESENTING THE VARIABLE FIRST OR SECOND

The numbers in the columns "V 1st" and "V 2d" are the percentages of trials the variable (V) was indicated as the 'heavier' (including half the 'equal' judgments) in the case of variable 1st and variable 2d, respectively.

The number of trials on which each percentage is based is 200; the total number of trials represented by the entire table is 4,000.

The values in the column "d" show by what percentage the weight of the variable differed from that of the standard.

In the column headed "D" is given the difference in the percentages of heavier judgments due to the temporal position of the variable.

d	Standard, 110 grm.			Standard, 200 grm.			Both Standards
	V 1st	V 2d	D	V 1st	V 2d	D	Av. D
−3%	28	45	−17	28	45	−17	−17
0	40	60	−20	37	63	−26	−23
+3%	56	73	−17	56	77	−21	−19
+6%	65	86	−21	74	89	−15	−18
+9%	78	91	−13	81	97	−16	−15
Av.			−18			−19	−18

Table II gives the results obtained with the varying-standard procedure. No explanation of this table is needed since it is constructed in exactly the same way as Table I. The results with the varying standard agree with those obtained with a fixed standard in showing a pronounced negative time-order error with all differences. The average time-order effect is somewhat greater with the fixed standard than with the varying standard, the average difference in the heavier percentages due to time-order being 18 with the fixed standard, and 14 with the varying standard. The explanation of this difference between the two procedures in the time-order error with zero difference is not altogether clear. It may be in part due to 'accidental variation,' but it probably depends mainly upon the fact, which will be revealed by Table III, that in the case of the varying-standard procedure, the time-order error with any given variable is an average of time-order errors which differ enormously both in size and direction with the weight of the varying standard. It will be shown that with the varying standard the time-order error is largely a function of the weight of the standard.

TABLE II

DATA WITH VARYING STANDARD SHOWING THE EFFECT UPON THE PERCENTAGE OF REPORTS SIGNIFYING VARIABLE HEAVIER OF PRESENTING THE VARIABLE FIRST OR SECOND

Each cell shows the percentage of reports meaning variable heavier (including half the 'equal' reports)
No. of trials: each cell, 400; total 4,000.
Standard, varied from 110 to 200 grm.

d	V 1st	V 2d	D
−3%	36	48	−12
0	44	56	−12
+3%	53	69	−16
+6%	64	81	−17
+9%	76	91	−14
Av.			−14

In order to compare the keenness or the accuracy of discrimination in the case of the two procedures, it will suffice to give the percentage by which the variable had to exceed the standard in order to be indicated as heavier in 75% of the trials. The values have been calculated by linear interpolation between the two variables giving percentages of heavier lying immediately above and below 75. The results, with the differences between variable and standard expressed as percentages of the standard, are as follows.

Standard......	Fixed at 110	Fixed at 200	Av. of Fixed	Varying
V 1st.........	8.31	6.43	7.37	8.75
V 2d.........	3.46	2.57	3.02	4.50
Av...........	5.89	4.50	5.20	6.63

Now with the varying-standard procedure the standards varied by steps of 10 grm. from 110–200, so that the average of all the varying standards exactly equals the average of 110 and 200. Except, then, for an effect due to difference in the two procedures, one might expect that the difference limen with the varying-standard procedure should not differ significantly from the average difference limen with the fixed standards 110 and 200 grm. As a matter of fact, however, with a varying-standard the limen is considerably greater than the average limen for the two fixed standards, the former being 6.63% while the latter is only 5.20%. The former is 27.5% greater than the latter. It appears, then, that with a standard varying over a range of 110–200 grm. the difference limen is about 27.5% greater than it is on the average in this same region with a fixed or constantly repeated standard. Incidentally it may be observed that with the fixed standard of 110 grm. a larger relative difference limen was obtained than with the fixed standard of 200 grm.

The fact that the difference limen is decidedly greater with a constantly varying than with a fixed or constantly repeated standard means of course that the percentage of correct judgments with a given difference between two stimuli is by no means solely a function of the difference between the stimuli. Likewise it cannot be solely a function of the difference in intensity of two sensations corresponding to the two stimuli. Clearly the percentage of correct reports depends upon some consequence of keeping the standard constant or not doing so. The consequence of a constant standard may very well be some sort of adaptation or attunement to the standard; and in that case the percentage of correct reports would reflect to a large extent merely the accuracy or precision of attunement. By this is not meant that, with a given *S* and a given testing procedure, the correctness of judgment is not primarily a function of the percentage difference between variable and standard. The data behind Fechner's law is sufficient to indicate that, aside from well known deviations, this is approximately the case. But the percentage of correct reports obtained from different individuals or the same individual under varying circumstances is certainly not simply a function of the difference between the two stimuli or their corresponding sensations. It depends rather upon a complex internal operation which may be termed the operation of discrimination or comparison; and the preceding data may be interpreted as indicating that a very important determinant of the effectiveness of this operation is some factor which is seriously affected by the change from a constant to a varying standard. The point that it is desired to emphasize, then, is that the preceding data show definitely the influence upon the efficiency of the discriminal act of a factor independent of the difference between the two excitations.

The most important outcome of the present study does not consist in the results so far set forth, but in the facts concerning the relation of the temporal-order error to the weight of the standard in the case of the varying-standard procedure. With the varying-standard procedure, it will be remembered, the varying standard really consisted of 10 different standards used in irregular order. In order to study the relation of the reports to the weight of the standards, the reports obtained with each of the 10 standards were tabulated separately, and then, to secure larger numbers and, consequently, greater reliability for the percentages of the various categories of reports, the data for each pair of adjacent standards were pooled. The data obtained with the varying-standard were thus analyzed or dismembered so that the results could be tabulated separately, first for the two lightest standards, 110 and 120 grm., then for the next heavier standards, 130 and 140 grm., and so on up to the two heaviest standards, 190 and 200 grm. For each of these 5 pairs of standards, the percentage of 'variable heavier' reports obtained with each variable was calculated separately for the two time-orders. The differences in the percentages of 'variable heavier' reports due to the time-order are shown in the lower part of Table III in the columns headed "D." Thus, in the case of the trials in which the standard was 110 or 120 grm. (trials scattered irregularly through the series of trials with varying standard), the variable which was 3% lighter than the standard was indicated as heavier in 55% of the trials in which it was presented before the standard and in 32% of those in which it was presented after the standard. The difference between these two percentages, which indicates the magnitude of the time-order error, is 23, a figure which is given in column D for the variable —3% and standards 110 and 120 grm. In studying the table, it is necessary to keep in mind that the plus and minus signs before the time-order errors in the columns headed D indicate whether the time-order errors are positive or negative. The error is arbitrarily labelled positive when the variable is indicated as the heavier more often when presented first, and negative when it is more often judged heavier when given second.

There are certain very decided and interesting phenomena shown by Table III. The facts may be stated in three general propositions.

(1) In the first place, it is clear that *with the varying standard the time-order error varies enormously with the weight of the standard.* It has been shown by Table II that when data for all the 10 standards constituting the 'varying standard' are massed together, the time-order errors come out negative for all of the variables. Table III, however, shows that the average negative time-order error indicated by Table II is a composite of very large negative time-order errors and smaller but definite positive errors. With the heavier of the 10 standards used in the varying procedure, the time-order is decidedly negative; while with the lightest standards, namely 110 and 120 grm., and with these only, it is positive. For example, the

data with zero difference in the case of the standards 110 and 120 grm. show a positive time-order error which corresponds to a change of 16 in the percentage of reports of 'variable heavier,' while in the case of the standards 190 and 200 grm., they show a negative time-order error corresponding to a change of no less than 50 in the percentage of such reports.

(2) The second noteworthy fact is that *the time-order error in the case of the 190 and 200 grm. weights with the varying-standard procedure is enormously greater than with the fixed standard.* With both procedures the error is negative, but with the fixed-standard procedure, in the case of the standard 200 grm., this error corresponds to a change of 19 in the average percentage of heavier judgments with change in the temporal position of the variable whereas with the varying procedure the change in the average percentage reaches the almost unbelievable figure of 49. With standards 190 and 200 grm., varying-standard procedure, when there is no difference in weight between the standard and variable, the variable is reported heavier in 75% of the trials when it follows the standard and in only 25% when it precedes the standard. If a difference be termed liminal when it is noticed in 75% of the trials, it may then be said that with standards 190 and 200

TABLE III

DATA WITH VARYING STANDARD SORTED TO SHOW THE DEPENDENCE OF THE DIRECTION AND AMOUNT OF THE TEMPORAL-ORDER ERROR UPON THE WEIGHT OF THE STANDARD

Each cell shows the percentage of reports meaning variable heavier (including half the 'equal' reports).

No. of trials: each cell, 80; entire table, 4,000

d	Standard in grm.									
	110 and 120		130 and 140		150 and 160		170 and 180		190 and 200	
	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d
−3%	55	32	33	35	39	40	25	54	20	77
0	58	42	51	49	46	54	44	56	25	75
+3%	73	54	61	54	45	74	61	81	34	83
+6%	83	61	64	76	64	88	61	83	49	96
+9%	86	85	88	83	83	93	60	93	58	98
	D		D		D		D		D	
−3%	+23		−2		−1		−29		−57	
0	+16		+2		−8		−12		−50	
+3%	+19		+7		−29		−20		−49	
+6%	+22		−12		−24		−22		−47	
+9%	+1		+5		−10		−33		−40	
Av.	+16		0.0		−14		−23		−49	

TABLE IV

DATA WITH VARYING STANDARD SHOWING THE NUMBER OF "VARIABLE HEAVIER" JUDGMENTS FOR EVERY *S* AND EVERY DIFFERENCE FOR THE VARIOUS WEIGHTS OF THE STANDARD, FOR EACH TEMPORAL POSITION OF THE VARIABLE

No. of trials: each cell, 16; entire table, 4,000.

Standard	d	Subjects									
		Sy		*Mn*		*Ke*		*Ws*		*Fn*	
		V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d	V 1st	V 2d
110 and 120	−3%	10	3	5	7	9.5	7.5	8.5	4	11	4
	0	11	8	8.5	8	12	4	10.5	7.5	9	11
	+3%	12.5	6	12.5	12	12.5	10	10	8.5	11	7
	+6%	11	6.5	13	12	14.5	9	14.5	9.5	13	12
	+9%	15	13.5	14.5	12	12	14	14.5	13.5	13	15
	Total	59.5	37	53.5	51	60.5	44.5	58	53	59	49
130 and 140	−3%	4.5	5	5.5	6	6.5	1	5	6	5	10
	0	8.5	7.5	5.5	10	11.5	5.5	9.5	7	3	7
	+3%	11.5	3.5	10	9.5	11.5	6.5	7.5	10.5	8	13
	+6%	12.5	14	9	13	12	12.5	10	9.5	8	12
	+9%	15.5	15	15	14.5	12.5	9.5	13.5	14.5	14	13
	Total	52.5	45	45	53	54	35	45.5	47.5	38	55
150 and 160	−3%	8.5	5.5	4.5	4.5	5.5	6	5	10	8	6
	0	6	5.5	7.5	8	8.5	8	6	9	6	10
	+3%	8	12.5	10.5	10.5	7.5	12.5	7	11	3	13
	+6%	8.5	13.5	11	14	11.5	16	10.5	14	10	13
	+9%	13.5	15	15	16	15.5	16	11.5	15	11	12
	Total	44.5	52	48.5	53	48.5	58.5	40	59	38	54
170 and 180	−3%	3	10.5	6.5	5	2	7.5	3.5	12.5	5	8
	0	5	5.5	7	12	3	4.5	9	9	12	14
	+3%	8.5	13	13	11	7.5	13	9.5	12.5	10	15
	+6%	8	14	11	13.5	11.5	12	11.5	15	7	12
	+9%	11.5	15.5	11	15	9.5	16	11	13	5	15
	Total	36	58.5	48.5	56.5	33.5	53	44.5	62	39	64
190 and 200	−3%	3	13	4.5	12.5	4.5	13.5	1	9.5	3	13
	0	2.5	13	1.5	11	5.5	11.5	2	10.5	5	11
	+3%	7	14	7.5	13.5	5.5	12.5	4.5	15	3	11
	+6%	6	15	11	15.5	9	15	8.5	15	5	16
	+9%	7.5	15.5	11.5	14.5	12	16	8	16	7	16
	Total	26	70.5	36	67	36.5	68.5	24	66	23	67

grm. a difference of zero constitutes the lower limen when the variable is first but the upper limen when the variable is second! The time-order thus has an effect equal to the difference between the lower and the upper difference limens. What would be the upper difference limen with the variable first can only be estimated, since with the heaviest variable used, +9%, the variable was indicated as the heavier in only 58% of the cases. This same variable was judged heavier in 98% of the trials in which it came second. Obviously, the varying-standard procedure is a technique which enormously enhances the negative time-order error in the case of the heaviest of those standards which constitute the varying standard.

TABLE V

Data with Constant Standard Showing the Number of "Variable Heavier" Judgments for Every S and Every Difference

No. of trials: each cell, 40; entire table 4,000.

Standard	d	Subjects									
		Sy		*Mn*		*Ke*		*Ws*		*Fn*	
		V_1	V_2	V_1	V_2	V_1	V_2	V_1	V_2	V_1	V_2
110 grm.	−3%	13	16	12	15	8.5	17	13	19	9.5	22.5
	0	15.5	24.5	18	24	22.5	26	13.5	23	13	24
	+3%	21.5	27.5	25.5	27.5	21.5	25	22.5	32	21	34.5
	+6%	21.5	37.5	28.5	36	26.5	29	24	37	28.5	32.5
	+9%	26	38	37	38.5	32	29.5	27	40	33	36
	Total	97.5	143.5	121	141	111	126.5	100	151	105	149.5
	%	48.8	71.8	60.5	70.5	55.5	63.3	50	75.5	52.5	74.8
200 grm.	−3%	14	18	10	18	10.5	15	9.5	20	12.5	19.5
	0	17	26	11.5	26	16	23	17.5	25.5	13	25
	+3%	19	28	22	32.5	25.5	32	23.5	30.5	20.5	31
	+6%	23.5	36.5	32	33.5	32	33	30.5	37.5	29	36.5
	+9%	33	39.5	30.5	38	33	39	37.5	39.5	28.5	38.5
	Total	106.5	148	106	148	117	142	118.5	153	104.5	150.5
	%	53.3	74	53	74	58.5	71	59.3	76.5	52.3	75.3

(3) A third interesting characteristic of the time-order errors with varying standard is that *while they change from positive with the lightest standards to negative with the heavier ones by a fairly regular progression, they are not distributed symmetrically about the standard of middle weight.* The standard above which the errors are negative and below which they are positive is not the standard which lies at the middle of the series, but one which lies half way between the middle and the lightest. The standards lying in the middle of the series are 150 and 160, but these show a negative time-order error. It is only in the region of the standards 130 and 140 grm. that the time-order error is zero. Positive time-order errors were shown only by the one pair of standards lying below this region; namely, 110 and 120 grm., while negative time-order errors prevailed with the

three pairs of standards lying above it; namely, 150 and 160 grm., 170 and 180 grm., and 190 and 200 grm.

Such asymmetrical or unbalanced distribution of the time-order errors would be the consequence if the errors were the resultant of two causes; first, one producing a negative error throughout, independently of the weight of the standard; and secondly, one causing increasingly positive or negative errors with increase in the difference between the weight of a standard and the middle or average weight of all the standards. The errors due to the first cause would remain the same for all weights of the standard, whereas those due to the second would vary symmetrically about the middle weight. The resultant of two such causes would yield errors approximating those obtained. For example, let the first error be assumed to be constant for all standards and to have the magnitude of —14, which is the average negative error found with the varying-standard procedure taken as a whole. Let the second cause be assumed to produce no error at the middle of the series but one progressing along a straight line from +30 at the lighter end to —30 at the heavier end. The resultant errors would be distributed in a manner roughly approximating that of the obtained errors, and showing the same asymmetry. The following tabulation illustrates this fact.

Standards	Error A (constant)	Error B (symmetrical)	Resultant error	Obtained error
110 and 120	—14	+30	+16	+16
130 and 140	—14	+15	+1	0
150 and 160	—14	0	—14	—14
170 and 180	—14	—15	—29	—23
190 and 200	—14	—30	—44	—49

No theory of the process of comparison can be expected to explain all the details and irregularities of a mass of experimental data; but the asymmetry of the time-order errors mentioned above is such a striking phenomenon, and so reliably indicated by the general trend of the errors as shown by the whole table, that it seems that any adequate theory should be able to take account of it.

THEORETICAL DISCUSSION

The facts concerning temporal-order errors now cover such a complicated and important set of phenomena that they must be regarded as constituting one of the chief tests of the validity of any theory of the comparison of successive intensities. Of the numerous theories concerning this operation the one which attempts most definitely to explain these facts is that proposed by Köhler.[2]

While it may not be altogether clear how Köhler's theory would explain the particular facts set forth in the preceding part of the present paper, it seems probable that this theory, if aided by the proper subsidiary assumptions, would lead to expectations in fair harmony with the present data. Köhler's theory has not, however, met with universal indorsement. Fernberger, for example, whose investigations of weight discrimination have been extensive, writes as follows:[3] "This [Köhler's] theory presents certain highly acceptable features. There are other aspects of the present experiment, however, which suggest the possibility of another interpretation." "It is obvious that the *O* might develop a muscular set with which he approached each successive weight of the sort described by Martin and Müller." "It seems reasonable to proceed on the basis of the older theory before propounding a new one." The writer agrees with Fernberger that the older theories, making use of a concept of "set" should not be too hastily discarded. Yet if the older concept is to be regarded as affording a possible source of explanation of the facts of comparison, it must be developed in much greater detail than heretofore. In attempting to work out the theory of set so as to explain the time-order errors, however, the writer has found that it is necessary to make assumptions that from the purely logical viewpoint are very similar to those of Köhler's theory. The theory arrived at substitutes a concept of "set" for Köhler's "traces" and yet otherwise appears to leave intact the greater part of Köhler's theory. Whatever may be the value of such a compromise, since it is not clear just how any theory hitherto proposed would explain the facts concerning the time-order errors which have been described in the present paper, the theory to be sketched will at least have the merit of indicating one way of explaining the facts.

Probably general assent may be secured for Köhler's statement that a comparison of two excitations is itself one unitary event occurring at the instant of the second excitation. Moreover, the happening at the moment of the second excitation is not anything like a viewing by an internal eye alternately of the memory image left by the first excitation and of the actual perception aroused by the second excitation. Introspective reports have thoroughly established the fact that the presence of an image of the first impression is not essential to the comparison. In the case of comparison of two successive sound intensities, Köhler insists that there need be no consciousness of the first present at the instant that the second is experienced. Introspective reports in the case of weight discrimination have repeatedly established the same thing. The first must leave *some* effect, otherwise comparison would be impossible; but introspective evidence is conclusive that this effect is not of a sort which results necessarily in the first experience being continued either as perception or image. The fundamental question, then, concerns the nature of the after-effect left, or set up, by the first excitation.

According to Köhler's theory, the first excitation leaves a "spur," or trace.

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[2] *Op. cit.*

[3] S. W. Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 576.

This trace consists of an accumulation of positive H-ions set loose rapidly by the excitation, and has the remarkable property of "quietly representing" the excitation. It constitutes a "still after-effect" or "still image."[4] This after-effect is not the correlate of any sensory-experience such as sound or weight. It merely "represents" this experience. The accumulation of H-ions is assumed to reach its highest level about 1½ sec. after the cessation of the first stimulus (in the case of sound-intensity discrimination). At this highest level, the accumulation represents an intensity greater than that of the stimulus. Thereafter, naturally, there is a gradual dispersion or absorption of the H-ions so that the "level" of the accumulation gradually sinks. The rate of sinking is slow, and according to a hypothetical curve sketched by Köhler for illustrative purposes, strange to say, never would reach zero. If the accumulation of H-ions existing shortly after excitation is posited as representing the intensity of the excitation responsible for it, then obviously, as the level of the accumulation sinks, it may be said to represent a progressively decreasing intensity. With the lapse of time the trace left by a strong excitation becomes identical with that of a more recent, weaker excitation. The only factor coming into play to bring about comparison at the time of the second excitation is this trace at the level at which it exists at the instant of the second excitation. The impression made on *S* by the first excitation has nothing to do with the comparison except in so far as it affects the condition of the trace at the instant of the second excitation. It must follow, then, that a given second excitation will result in the same judgment when the first excitation is of high intensity but precedes the second by a considerable time as when the first is of moderate intensity but more immediately precedes the second. There is no memory or other awareness of the original condition of the trace; nor does the comparison process involve any reckoning by *S* of the influence upon the trace of the lapse of time since the stimulus responsible for it. *S*'s judgment depends upon the relation between just two things: the first, which may be termed L, is the intensity represented by the trace *at the moment of the second excitation,* and the second, which may be termed E_2, is the intensity of the second excitation. When L is less than E_2, the intensity of the second excitation is experienced as a "springing upwards," and presumably results in the report "second more intense." When L is greater than E_2, the judgment is "second less intense," and if L is equal to E_2, the judgment is "second equal." Since the level of the trace sinks during the interval between the first stimulus and the second, it follows that if the second excitation has the same intensity as the first, the second will meet with a trace-level, L, of lower intensity than its own intensity and therefore be experienced as a step upward and be followed by the erroneous judgment "second heavier." The theory thus explains the fact of the negative time-order error which is obtained under ordinary experimental conditions and also the increase in this error with increase in the lapse of time between the first and second stimulus. The general principle is that the less the concentration of +H-ions remaining as an after-effect of the first stimulus at the time of the second, the greater the relative over-estimation of the second, *i.e.* the greater the negative time-order error.

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[4] For Köhler's use of these terms, "stillen Nachwirkung," and "stillen Bild," cf. *op. cit.*, 147 f.

Of course E_1, that is, the first excitation of a pair, would meet with the trace left by the preceding stimuli, and in particular by the trace left by the last preceding stimulus. Thus E_1 might meet with the same sunken trace level as E_2, and in such a case, if E_1 were of the same intensity as E_2, one might reason that since both E_1 and E_2 encountered the same trace level and were of the same intensity, they would be judged equal. Such reasoning would be incorrect, however, because it overlooks the fact that the judgment does not depend upon the relation of E_2 to E_1, but upon the relation of E_2 to the trace left by E_1 as it is at the moment E_2 occurs; and the condition of the trace at the moment of E_2 is by no means perfectly correlated with E_1, but depends upon many other factors, and especially upon the amount of time which has elapsed since E_1.

That Köhler's traces have the properties and results which he assumes cannot of course be directly proved or disproved. Some of the assumptions appear open to question. For example, it is rather difficult to understand how the amount of accumulated +H-ions left as a trace could ever come to represent the intensity of the excitation, since the trace as an index of intensity is utterly ambiguous. At a given instant the same trace might be the after-effect either of a very weak or of a very intense stimulus, depending upon how long previously the stimulus occurred. A long past, intense stimulus would have the same trace as a very weak but very recent one. No reckoning by *S* of the influence of the lapse of time upon the trace could very well be supposed to play a part in the process of comparison. Of course it may be pointed out that the time-order errors indicate that the trace represents the intensity of E_1 only very incorrectly. But while the time-order error is an important matter, the comparison is not always at the mercy of this error. The comparison may be correct in spite of the time-order error if the difference between the compared intensities is sufficiently great.

Whether or not Köhler's traces should lead to the expectation of errors in cases where as a matter of fact experiment does not reveal them is difficult, if not impossible, to determine, because the assumptions about these traces are not sufficiently definite. If, in the course of time, the trace of E_1 almost completely disappeared, then E_2, even though only a negligible fraction of the intensity of E_1 should by Köhler's theory be judged more intense, since the trace of E_1, having almost disappeared, would represent a lesser intensity than that of E_2. No one, however, has ever reported any conditions which will cause a weight of 10 grm. to be judged heavier than one of 1000 grm. If, however, one alleges, as Köhler apparently does, that the brain after experiencing an excitation never returns to its original condition as regards these accumulations of +H-ions, *i.e.* that the traces never vanish, or at least do not do so for weeks and months, then it would seem that in the course of one sitting the trace of any E_1 should become so inextricably mixed with that of innumerable other preceding E's that all comparison should be mere guess work. Traces varying simply as to different amounts of +H-ions accumulated in the same brain-region could not be kept distinct; the traces left by two excitations differing only in intensity would inevitably, as Köhler himself postulates, through some sort of addition,[5] become simply one trace.

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[5] "Häufung von Einzelspuren," *op. cit.*, 173-174.

This assumption of the addition of traces, while necessary in order to explain the known effects upon any one comparison of the preceding comparison pairs, appears to lead to expectations contrary to all experience. One of a number of situations where the theory seems to lead to consequences contrary to fact, is that represented in Fig. 1.

The diagram represents the situation as it would be if an *S* were asked to compare E_2 with E_1, E_1 being preceded by the much more intense excitation E_x. The assumption is that *S* has been instructed to attend to all three weights, but to compare the third with the second. The dotted curve indicates the gradually sinking trace of the first excitation E_x, of an intensity of 200 grm. The intensity of the excitation represented by the trace as it exists 10 sec. after E_x is indicated by the diagram as having the hypothetical value of 193 grm. Now suppose that 10 sec. after E_x, E_1 occurred as a result of a stimulus of 50 grm. E_1 should apparently leave no trace of its own, but simply cause some increase in the residue of +H-ions left by E_x; so that 20 sec. after E_x the one existing trace should, because of E_1, represent a somewhat greater intensity than if E_1 had not been given. This intensity could hardly be much less than that produced by a weight 186 grm., as indicated in the figure, for the time error with a standard of 200 grm. and an interval of 20 sec. is probably not greater than 14 grm., *i.e.* the difference between 200 and 186 grm. It follows that if E_2 were an excitation due to a weight of 150 grm., its intensity would be less than that represented by the trace at the time of E_2, and the transition experience, being downwards, would result in a report that the third weight was lighter than the second. Now it is true, as the data given in this paper have shown, that in the comparison of relatively light weights preceded by relatively heavy weights, the second weight of the pair of light weights will be underestimated, *i.e.* the time-order will be positive. But it is inconceivable that in the case illustrated above this time-order error would become so great that the stimulus of 150 grm., producing E_2, would be judged lighter than the stimulus of 50 grm., producing E_1. Yet that is what should happen, so far as the writer can determine, according to Köhler's theory. The outcome of the judgment depends according to this theory upon the relation of the level of the trace at the moment E_2 occurs and the intensity of E_2. Neither memory-images of the preceding weights nor any allowance for lapse of time play a part in the comparison. Thus there seems no escape

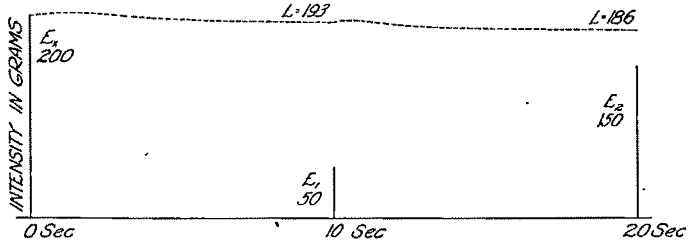

FIG. 1. DIAGRAMMATICAL REPRESENTATION OF SITUATION IN WHICH *S* COMPARES E_2 WITH E_1

from the conclusion that according to Köhler's theory 150 grm. can be made to be judged lighter than 50 grm. by having the latter weight preceded by a weight of 200 grm. Moreover, this outcome remains the same even if a weight of 1 grm. is substituted for that of 50 grm.

Whether or not the difficulties with Köhler's theory which have been mentioned could be cleared up by means of subsidiary hypotheses or by some re-interpretation of Köhler's statements, it remains true that the fundamental assumptions of Köhler's theory are far from proved and that it is therefore permissible to cast about for other explanatory concepts. As already stated it seems to the writer that the concept of set may be made to afford the basis of an explanation of comparison in a manner more satisfactory than Köhler's residues of +H-ions. According to this concept, the effect of the first of a pair of stimuli upon an *S* who has been informed that a second, or following, stimulus is to be compared with the first, is to set up in him a preparation for, adjustment or attunement to, or expectation of, a certain intensity of following stimulus. That such adjustments or expectations may exist without conscious imaging of the intensity adjusted to, is a familiar fact. For example, it is quite possible to arrange a discrimination reaction-time experiment in which *S* as the result of a few practice trials will react to only one intensity or to one pitch (within narrow limits). *S*, at the warning signal, must adopt some sort of expectancy of adjustment toward a stimulus of a particular character, and not simply a general readiness to react; for otherwise he would react to any stimulus instead of exclusively one of the prescribed character. Now in such cases introspective reports concerning the absence of an image of the prescribed stimulus may be just as emphatic as reports of the absence of an image of the first of a pair of comparison weights at the moment of presentation of the second.

Which of many terms is the best to use to describe the phenomenon, set, attitude, expectancy, adjustment, adaptation, or attunement is a matter of opinion. They are all admittedly vague, and any physiological account would be extremely speculative; but whatever word is chosen, its meaning, as here intended, is a readiness for a stimulus of a particular character. Introspective reports will vary concerning the presence or absence of imagery as present in this condition. Frequently, at least, they will indicate the existence of certain motor tensions. The writer believes that such a set, involving certainly non-projection areas and often, at least, motor areas, is a necessary postulate in order to understand comparison. Further, a few very simple assumptions concerning the behavior of properties of such sets will account for all the known facts concerning the time-order errors in the comparison of successive intensities.

The attributes which must be postulated of this expectancy or set in order to explain the complicated facts of comparison, including those concerning the time-order error with both constant and varying standard, are as follows.

(1) In the case of the comparison of intensities, the set existing at the moment of the second excitation is a set towards a certain intensity. This intensity will be represented by the symbol L, and termed the level.

(2) The level of the set tends to approximate the average intensity of all the preceding stimuli of the entire series given from the beginning of the experimental sitting, providing the series does not vary over too great a range. The average of the entire series should be thought of as a weighted average, in determining which, the most recent stimuli have the greater weight and the last stimulus the greatest weight.

It follows, therefore, that the level adopted following a stimulus more intense than the average will at first be raised but will revert towards the average level. Conversely, the level, while suddenly lowered by an exceptionally weak stimulus, will tend to revert to the average. This assumption, which amounts to bringing in the hypothesis of assimilation, would explain the time-order errors in the case of the varying standard—negative when the varying standard is heavier than the average and positive when exceptionally light. It does not explain why the negative errors for the heaviest varying standard are so much greater than are the positive ones for the lightest varying standard. This fact, as well as the negative errors with the regular constant-standard procedure, require one more assumption.

(3) The level of intensity towards which *S* is set tends in all cases to sink towards a lower level, but does not even remotely approach a level of zero as long as the set is maintained. When *S* ceases to expect a second stimulus, then the set or preparation would presumably either disintegrate or fall back to some habitual level.

The plausibility of the above assumptions is a matter of opinion. Particularly might there be question concerning the third assumption, that the intensity towards which *S* is adjusted tends to decrease with the lapse of time before the expected stimulus. It is barely possible that this may be merely a special case of the second assumption, in that there may be a chronic level of adjustment corresponding to the average intensity of habitual stimuli; in which case the tendency of the level to sink would be merely the tendency to adjust to an average habitual intensity. In the case of weight, this habitual level might be that corresponding to the weight of the fore-arm. Another possibility is that sets are constituted of motor strains and that a set to a high intensity means a high degree of strain. It is fairly plausible that a condition of high degree of tension, cerebral or muscular, should with the lapse of time tend to change into one of lesser tension. All that is here alleged, however, is that the known facts concerning comparison of successive intensities point to some attunement or set as an important factor in that act and that these facts may be accounted for by making the above assumptions concerning the behavior of the set.

The above schema, and it is only that, of a theory of comparison of intensities may be summarized very briefly. Calling the intensity of the first and second excitations E_1 and E_2, and the intensity level of the set, L, the judgment of the subject depends upon the relation of E_2 to the L existing at the instant of E_2. If E_2 is greater than L, it is experienced as an excitation of greater intensity, as a "rising up from," to use Köhler's expression. If E_2 is less than L, the second excitation is judged as less, and if E_2 is equal to L, as equal. The intensity level, L, *immediately after* E_1 is regarded as being approximately equal to the intensity of E_1. It may at first, within the first few seconds, even exceed that of E_1, thus constituting an over-adaptation or over-adjustment and leading to positive time-order errors. Normally, however, L decreases with the lapse of time—to what extent can be determined by working out the increase in the normal negative time-order error with increase in the interval between the two comparison stimuli.[6] Lastly, if E_1 is much higher than the average intensity of the series, the level L tends to revert rapidly from E_1 to the average; likewise, if E_1 is much less than the average, L tends to revert rapidly to the average, the reverting in this case however constituting a rise in L instead of the normal decrease.

The following diagrams, in which the height of the vertical lines represents the intensity of the excitation and the height of the dotted line the intensity to which the subject is set, will illustrate the chief phenomena of time-order errors.

Fig. 2 illustrates the explanation of the normal negative time-order error. E_2, though equal to E_1, is greater than L; therefore the judgment is "second heavier." Since the level L decreases with the increase in the length

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[6] For data on this increase see: J. Borak, Über die Empfindlichkeit für Gewichtsunterschiede bei abnehmender Reizstärke, *Psychol. Forsch.*, 1, 1922, 374 ff.; K. Fodor, and L. Happisch, Über die Verschiedenheit der Unterschiedsschwellen für den Gesmacksinn bei Reizzunahme und Reizabnahme, *Arch. f. d. ges. Physiol.*, 197, 1922, 337 ff.; and Köhler, *op. cit.*

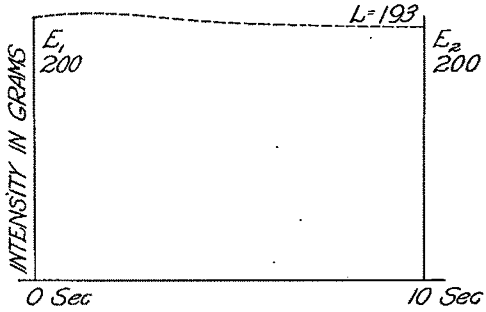

FIG. 2. DIAGRAMMATICAL ILLUSTRATION OF THE NORMAL NEGATIVE TIME-ORDER ERROR

of time between E_1 and E_2, the figure also illustrates the increase in negative error with increase in the interval between the two compared stimuli.

Figs. 3, 4, and 5 illustrate the chief facts in the case of a varying standard. In these three figures it is assumed that *S* has adapted himself to the average of the entire series, taken as 155 grm., and further, for the sake of illustration, that L drops the equivalent of 7 grm. during the 10 sec. between E_2 of one pair and E_1 of the following, and likewise during the 10 sec. between E_1 and E_2 of the same pair. It is further assumed that the

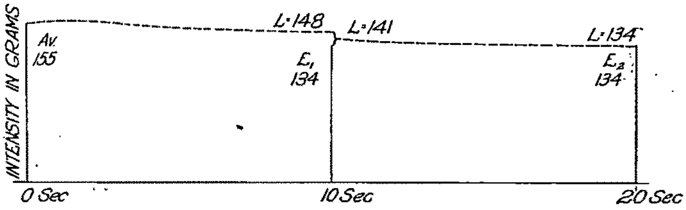

FIG. 3. DIAGRAMMATICAL ILLUSTRATION OF THE TIME-ORDER ERROR WHEN THE VARYING STANDARD IS SLIGHTLY LIGHTER THAN THE SERIES AVERAGE

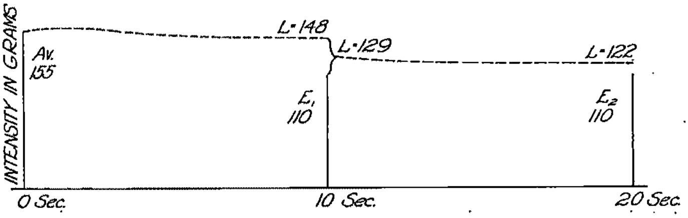

FIG. 4. DIAGRAMMATICAL ILLUSTRATION OF THE TIME-ORDER ERROR WHEN THE VARYING STANDARD IS MUCH LIGHTER THAN THE SERIES AVERAGE

height of L shortly after any E_1 may be taken as half way between the height of L at the moment E_1 occurred and the intensity of E_1, that is, that the last E and the whole preceding series have equal effect upon L, so that L is the average of what it would be if solely determined by the preceding series and what it would be if determined by the last E. This fusion level is likewise assumed to drop 7 grm. between E_1 and E_2. These assumptions, made for the sake of illustration, are undoubtedly inaccurate, and too simple, but they will serve to indicate at least the general direction of certain explanations. According to these assumptions there will result a set or a readiness for an intensity indicated at L by the height of the dotted line above the base-line. The judgment will depend upon the relative height of the level L and of the last excitation E_2. The diagrams thus illustrate the explanation of the following facts concerning time-order errors with the varying-standard procedure: Fig. 3, the varying-standard when it weighs 134 grm. (lighter than the series average of 155 grm.) gives no time-order error, since L and the intensity of E_2 are the same; Fig. 4, the varying standard when very light (110 grm.) gives a considerable positive time error, since L is greater than E_2; Fig. 5, the varying standard when very heavy (200 grm.) gives a negative time-order error far greater than with the fixed-standard procedure (cf. Fig. 2), and also a negative error of much greater magnitude than that of the positive error shown by the 110 grm. standard (cf. Fig. 4).

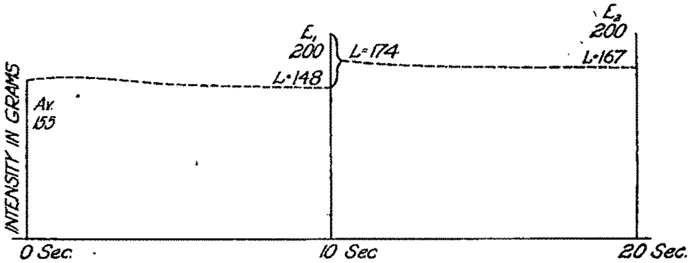

FIG. 5. DIAGRAMMATICAL ILLUSTRATION OF THE TIME-ORDER ERROR WHEN THE VARYING STANDARD IS MUCH HEAVIER THAN THE SERIES AVERAGE

The theory suggested, then, affords an explanation of the striking asymmetry of the time-order errors, in the case of the varying standard, when these errors are plotted in relation to the weight of the standard. Instead of being evenly balanced about the average weight of the whole series, they are distributed about an indifference weight considerably below the average. The explanation of this asymmetry, as likewise the fact that an exceptionally heavy pair of comparison weights shows a much larger negative error than the positive error shown by an equally exceptionally light pair, lies in the assumption that the level, L, even when it corresponds to the average of the entire series from the beginning of the sitting, always sinks between E_1 and E_2. As already pointed out,[7] the asymmetrical distribution of the errors may be regarded as due to two factors, the first causing a uniform, negative, time-order error and the second an error symmetrically distributed, as regards its magnitude, about the middle weight of the series, though positive for standards below this weight and negative for standards above it. According to the above hypotheses the first of these factors would be the sinking of the level, L, which always occurs between E_1 and E_2; the second would be the tendency of an L which was deflected from the general level by an exceptionally light or exceptionally heavy E_1 to revert to the average level.

How, it may be asked, would the assumptions here suggested solve the dilemma illustrated in Fig. 1, in which, according to Köhler's theory, a weight of 50 grm. should be judged heavier than one of 150 grm.? As a matter of fact the assumptions used in the construction of Figs. 2–5 would result in the case of Fig. 1 in a height of L, at the moment of E_2, of only 114.5 instead of 186, so that a weight of 150 grm. would by these assumptions be judged not lighter, but much heavier than one of 50 grm. According to the assumptions, however, a weight of 50 grm. would be judged as heavy as one of 114.5 grm., and a weight of 1 grm. would be judged as heavy as one of 90 grm., both of which consequences, it would seem, could hardly agree with the experimental fact. It should be remembered, however, that attitudinal sets are more under the voluntary control of *S* than are accumulations of +H-ions due to physico-chemical laws. *S* cannot suddenly shake away the accumulation of H-ions left by a preceding relatively heavy weight. He would, however, experience the very weak excitation E_1 following the relatively very intense E_x, as an extremely light weight and might very well, as a consequence, adopt an attitude which did not assimilate with that produced by E_x, but was entirely separate from it, or even contrasted with it. It seems inevitable that when the range of stimuli is very great, a stimulus near either extreme of this range would result in an exception to the rule that *S* tends to adapt to the average of the entire series. It is for this reason that the statement in assumption (2) that the level of intensity to which *S* adjusts himself as the result of E_1 is the average level of the entire series, has been limited to series that do not vary over too great a range. The experimental data given in the first part of this paper show that the range may be as great as that from 110 to 200 grm. Whether the results would remain the same, however, with the varying-standard procedure, in case the standard varied, for example, from 20 to 600 grm., only experiment can tell; but on general considerations one should expect radically different results.

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[7] *Supra* 403.

There are many other facts than those concerning time-order errors which may be readily explained on the basis of the above assumptions, though undoubtedly a considerable number of factors not envisaged by these assumptions come into play in special instances. Among these subsidiary factors are the influence of preceding judgments (not merely preceding stimuli), changes in set due to variation in instruction, imagery, the effect of change in *S*'s understanding of the categories of judgment, etc. So far as the writer is aware there are no facts of comparison which can be accounted for by Köhler's concept of sediment-traces which cannot also be explained in terms of set; and many phenomena seem more easily understood by reference to the latter concept. For example, the size-weight illusion is ordinarily explained through the use of a concept of expectant attitude or set. The set is produced in this case not so much by the first stimulus-object of a pair as by the relative size of the second. If the second stimulus-object is larger than the first it arouses an expectation of a heavier weight, and if smaller, *S* becomes set for a lighter weight. In either case there is a discrepancy between the level, L, of the set and the intensity of the second weight. In the case of a large weight, the level L corresponds to an intensity greater than E_2, and therefore the weight of the large object is underestimated. Again, acknowledgment of the importance of sets or attitudes in successive comparison has the advantage of bringing the explanation of time-order errors in the case of intensity into line with those in the case of the comparison of short intervals of time. The effect of attitude on comparisons of interval has been experimentally investigated by the writer and found to be enormous.[8] The absolute indifference interval of about 0.6 sec. suggests a permanent natural set analagous to the more temporary attunement which it has been assumed above may be established experimentally by continued stimulation by a series of stimuli of not too wide a range of variation. Lastly, it may be suggested that explanations in terms of set or attitude probably account better for individual differences, contradictory judgments by the same individual, and significant variation from day to day,[9] than do traces consisting of accumulations of H-ions. Sets or attitudes are notoriously variable and unstable, whereas the immediate physiological effects of stimulation, while also variable, should on the whole lead to consequences of rather greater uniformity than the highly variable and contradictory judgments resulting from repeated comparisons. The lesser acuity in the case of a varying standard than in that of a fixed standard could be explained on the assumption that a set to an average of widely differing weights would be more indefinite and variable than in the case of a constantly repeated standard.

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[8] H. Woodrow, The reproduction of short temporal intervals, *Exper. Psychol.*, 13, 1930, 496; Individual differences in the reproduction of short intervals, this JOURNAL, 45, 1933, 276-281.

SUMMARY

A comparison was made as regards keenness and time-order errors between two procedures for the study of discrimination of lifted weights. The two procedures, both following the method of constant stimuli, were alike in all respects except that in the first the standard was fixed or constant throughout a sitting while in the second it varied irregularly from trial to trial from one to another of 10 different weights. With the fixed-standard procedure experiments were made with standards of 110 grm. and 200 grm. With the varying-standard procedure the standard varied by steps of 10 grm. from 110 to 200 grm. The chief conclusions drawn from the experimental data, based on a total of 8,000 comparisons by 5 *S*s, were as follows.

(1) With the fixed standard procedure, all variables showed a *negative* time-order error. With the 110 grm. standard, variables were on the average judged heavier 18% more often when they followed the standard than when they preceded it, and with a standard of 200 grm., 19% more often.

(2) With the varying standard procedure, the time-order errors, while on the whole negative, varied enormously with the 10 different weights given the standard. At its lightest weights, 110 grm. to 120 grm., all variables yielded *positive* time-order errors (corresponding to a difference in the percentages of "variable heavier" of 13); at weights 130 to 140 grm., the variables yielded an average time-order error of zero; while at weights from 150 to 200 grm., inclusive, all variables yielded *negative* time-order errors. These negative errors showed a regular increase with increase in the heaviness of the standard, until with the varying standard at weights of 190 and 200 grm., the negative time-order error reached a remarkably great magnitude and far exceeded that with the fixed-standard

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[9] Cf. Woodrow, Quotidian variation, *Psychol. Rev.*, 39, 1932, 254-256.

procedure. With the standard at these last mentioned weights, that is, 190 and 200 grm., a change in the order of the variable and standard caused a change of 49, on the average, in the percentage of trials in which the variable was judged heavier. For example, when the variable was 3% heavier than the standard, the variable was juged heavier in 83% of the trials in which it followed the standard but in only 34% of those in which it preceded the standard.

(3) The difference threshold, taken as a percentage of the standard, was about 27.5% greater for the varying standard than for the average of the two fixed standards.

(4) It is contended that the above facts have considerable significance for the theory of the comparison of intensities of successive stimuli, and that they may best be explained by assuming that the first stimulus of each pair is followed by an attitude of expectancy, or set, as regards the second. Three simple assumptions concerning the behavior of these 'sets' are shown to suffice for the explanation of the facts.

SERIAL LEARNING AND GENERALIZING ABSTRACTION

By LEON ARONS, University of Pennsylvania

While there have been a number of investigations of the higher mental processes,[1] little has been done specifically comparing (in quantitative terms) serial learning of the language type with generalizing abstraction. "The establishment of mere *chains* of verbal reflexes as in the learning of a string of nonsense syllables is one type of linguistic organization."[2] On the other hand, generalizing abstraction as a function of "symbolic intelligence," might be expected, to an extent, to be dependent upon degree of facility at acquiring these "mere chains of verbal reflexes." Briefly, our problem is to investigate the degree of correlation between an *S*'s ability at serial learning with his ability at generalizing abstraction as revealed by the "multiple choice" method.

METHOD AND PROCEDURE

Most investigations of learning (verbal) have made use of nonsense material. The comparative degree of nonsense of this material may be questioned.[3] In addition, for our purpose, it is most desirable to use a method which will give a wide range and distribution of scores. We have therefore chosen material (playing cards) which is relatively meaningless, and have placed the emphasis on acquisition in pattern (serial) form. Differences in 'card background' are minimally significant. All *S*s may be expected to know the names of the cards. This method will also give us our desired scatter of scores.[4]

There is a paucity of experimental data on generalizing abstraction. Moreover, the term itself is somewhat vague, or at least, too general in its connotation. The latter is especially true if we are to consider generalizing abstraction to be synonymous with concept, as Hull seems to do,[5] although at times he uses the term "general-

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I wish to express my indebtedness to the many *S*s who served in this investigation, and to Professor Samuel W. Fernberger and Dr. Francis W. Irwin who offered invaluable criticism and advice.

[1] J. C. Peterson, Higher mental processes in learning, *Psychol. Monog.*, 28, 1920 (no. 129), 1-127; B. F. Haught, Interrelation of some higher learning processes, *ibid.*, 30, 1922 (no. 139), 1-71. For other references see these two studies.

[2] F. Lorimer, *The Growth of Reason,* 1929, 93.

[3] F. C. Davis, The relative reliability of words and nonsense syllables as learning material, *J. Exper. Psychol.*, 13, 1930, 221-234; F. M. Sauer, The relative variability of nonsense syllables and words, *ibid.*, 13, 1930, 235-246.

[4] Our method is similar to Renshaw's (cf. S, Renshaw, Experiment of the learning of 'paried associates,' *J. Appl. Psychol.*, 11, 1927, 226-233).

[5] C. L. Hull, Quantitative aspects of the evolution of concepts, *Psychol. Monog.*, 28, 1920, (no. 123), 1-85. For criticisms of methods used by Grünbaum, Moore, and Fisher, see Hull; for more recent references, see N. H. F. Maier, Reasoning in humans, *J. Comp. Psychol.*, 10, 1930, 115-144.

izing abstraction" interchangeably with "methods by which concepts are evolved." He lists three types: (1) a type exemplified by a child withdrawing from an electric glowing bulb after having been burned by a candle; (2) a type of "generalizing abstraction is involved in the so-called inductive 'steps' of formal instruction originated by Herbart," which he thinks "has strong elements of artificiality," and (3) the "standard" type, which he believes to be the method by which the great majority of actual concepts are evolved. In this latter type, the "formation of the concepts has never been an end deliberately sought for itself . . . always a means to an end—the supremely absorbing task of physical and social reaction and adjustment."[6]

We chose, secondly, the Yerkes multiple choice method,[7] which presents a number of novel settings (situations). One 'key' in each set will sound a buzzer. *S* collects his data (observations) so as to be able to formulate a rule (generalization) which will enable him to predict the buzzer key for any other setting given him. In other words, when the *S*'s behavior becomes 'adequately' generalized with respect to these situations (with the concomitant language 'key'), he is considered to have solved the problem.

Technique. (A) Serial learning. S, seated at a small table in a quiet room, with *E* opposite, was given a deck of playing cards (previously arranged) face down, and the following instructions.

> Here is an ordinary deck of playing cards. You are to learn as many as possible in correct order. Here is how you are to go about it. When you are ready to start, hold the deck face up in the palm of your hand. Call out the top card, the name and the suite, then put it face down on the table in front of you. Then call out the card now on top, and put it face down on the first card. In this manner go through the entire deck. When you have finished going through the deck, I shall ask you to call off as many cards in correct order (starting from the first), as you can. Having recited, you will go through the deck again, and so on. After each leafing of the deck you recite. You have 15 min. to spend in actually going through the deck. Within the limits given by these directions, you can go about it in any manner you choose, as rapidly or slowly as you like, and so on. Don't cram, but go about it as efficiently as possible. Try to work out an optimum method for yourself of going about it. You are not to look back once you have put a card down, and you are to uncover but one card at a time. These cards are not arranged according to any key, so do not waste any time looking for one. Remember as many cards as possible, in correct order starting from the first.

At the end of the first "card period," the *S*'s method of going about the learning was discussed with him, and he was allowed to formulate what he considered the best way of going about it the next time. He was told that a different order of cards would be used the second time, and was asked not to practice in the interim.

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[6] It might be well for us to indicate what we mean by "generalizing abstraction." We would inscribe the highly popular adjective 'dynamic' into the matter, and consider generalizing abstraction as generalized behavior with a language (in the broadest sense) 'key' or 'tab.' In no sense do we intend to suggest a dichotomy between the 'behavior' and the 'key,' nor to reify either term. Behavior is 'generalized' with respect to certain situations. An aspect of the generalizing is the language 'key.' The key may be considered a formula of the whole of this behavior in symbolic terms.

[7] R. M. Yerkes, A new method for studying the ideational behavior of mentally defective and deranged as compared with normal individuals, *J. Comp. Psychol.*, 1, 1921, 369-394.

Time for each leafing of the deck was recorded (stop watch) along with the *S*'s recitations. When the remainder of the time prescribed permitted of but one leafing more, *S* was notified of the fact. The 15-min. periods were kept within 30-sec. limits. Two card periods were used, separated by approximately 48 hr. At the second period *S* was told to go about it on the basis of experience acquired the first time, otherwise the directions were the same. *S* was asked to describe his method of learning after each period.

The order of cards for each period was as follows.

Period 1. 3H, 7D, AH, 8C, KS, 10H, JC, 3S, 5D, 2C, 8D, 10S, 4H, KC, 7C, 4D, 9H, QC, 6D, AS, JD, 4S, 8H, QS, 6C, JS, KH, 3D, 5S, AC, 9S, JH, 4C, 2D, 9C, 7H, 10D, 5C, 2H, 8S, KD, 7C, 9D, 2S, QD, 5H, 10C, 6H, AD, QH, 3C, 6S.

Period 2. 9C, 5D, JS, 3C, AD, 5H, QS, 9D, 8C, 6D, 9H, AC, 6C, KH, 10S, 3D, 7H, 5C, JD, 2C, 8D, 4S, KD, AH, 8H, QH, 6H, QC, 2D, 4C, KS, 8S, 7C, 3S, 6S, KC, AS, 4D, 10H, 2S, 9S, 2H, 5S, 10C, 4H, JH, 7S, 10D, 3H, QD, JC, 7D.

(B) Multiple choice. The Yerkes multiple choice apparatus was used in this part of the experiment. The problems may be said to differ in two ways: (1) according to principle involved; and (2) according to point of reference or application. Such principles as middleness, alternation, number of groups, odd-and-evenness, etc. may be used, and such points of reference, which are more limited, as the ends, the center, etc. In our choice of problems we endeavored to make use of a different principle in every problem, and to avoid the mere complication of principles already given the *S*. Points of reference also were kept simple. We may, therefore, expect results which will give a fairly accurate 'picture' of the *S;* chance factors in the solution having been reduced to a minimum.

The principles made use of in these problems are not new to *S*. In his background will be found such abstractions as middleness, spacial progression, and the like. He must, however, arrive at a generalization adequate to the problem in hand. Here the matter of 'direction' of activities, relational discriminations, and so on, are involved.[8] The verbal generalization finally evolved is a product of these situations, and is an aspect of *S*'s 'generalizing behavior.' A list of the 6 problems used is given in Table I.[9] The explanatory portion following the problem number gives a statement of the solution. The settings are given in their order of presentation to the *S:* "1–7" indicates that keys one to seven inclusive were extended in this particular setting. The parenthetical number designates the correct key for this setting. Two series of numbers for correct keys will be found for Problem V. This problem is one of alternation, and inasmuch as there is an odd number of sets, repetition of sets after the 15 listed sets have been used, will bring about a shift in the keys correct for the next 15 sets. The correct keys for these sets (sets 16 to 30) will be found in the second column of parenthetical numbers. It should be stated that the series of sets listed were repeated. The maximum number of sets allowed will be discussed in a later section. The order of presentation of the problems is based upon a preliminary investigation on 20 *S*s. In this present investigation, the order of presentation was Problems I, III, II, IV, V, and VI.

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[8] Cf. Maier, *op. cit.*

[9] Four of our problems were used by Yerkes: Problem 1—Yerkes No. 3; Problem II—Yerkes No. 6; Problem III—Yerkes No. 1; and Problem V—Yerkes No. 10.

TABLE I

The Multiple Choice Problems and Their Solutions
(Figures in parentheses give correct key)

Problem I. Principle involved: middle key.

1.	1–7	(4)	3.	6–10	(8)	6.	4–10	(7)	9.	1–5	(3)
2.	10–12	(11)	4.	1–11	(6)	7.	1–9	(5)	10.	6–12	(9)
			5.	1–3	(2)	8.	9–11	(10)			

Problem II. Principle involved: starting with key 12, the next key to left at every trial.

1.	10–12	(12)	4.	8–12	(9)	7.	5–8	(6)	10.	3–7	(3)
2.	6–12	(11)	5.	8–10	(8)	8.	4–9	(5)	11.	1–5	(2)
3.	3–10	(10)	6.	1–9	(7)	9.	2–11	(4)	12.	1–12	(1)

Problem III. Principle involved: same key at every trial, *i.e.* no. 5 from left.

1.	1–6	(5)	3.	4–6	(5)	6.	4–5	(5)	9.	1–8	(5)
2.	3–12	(5)	4.	5–9	(5)	7.	4–10	(5)	10.	5–10	(5)
			5.	2–10	(5)	8.	3–6	(5)			

Problem IV. Principle involved: first key from right with even number of keys, and second key from right with odd number of keys.

1.	1–5	(4)	5.	4–7; 9–12	(12)	9.	3–12	(12)	13.	7–9	(8)
2.	3–8	(8)	6.	1–11	(10)	10.	1–3; 7; 9–11	(10)	14.	2–11	(11)
3.	5–6	(6)	7.	4–6	(5)	11.	2–9	(9)	15.	6–12	(11)
4.	8–12	(11)	8.	8–11	(11)	12.	6–7; 9–11	(10)			

Problem V. Principle involved: alternately the key to the left and right of the middle key.

1.	1–7	(3)	(5)	6.	3–9	(7)	(5)	11.	4–12	(7)	(9)
2.	8–12	(11)	(9)	7.	2–4	(2)	(4)	12.	3–5	(5)	(3)
3.	2–10	(5)	(7)	8.	6–10	(9)	(7)	13.	4–10	(6)	(8)
4.	10–12	(12)	(10)	9.	6–12	(8)	(10)	14.	5–9	(8)	(6)
5.	1–9	(4)	(6)	10.	3–7	(6)	(4)	15.	2–12	(6)	(8)

Problem VI. Principle involved: as many keys from the left as there are groups extended.

1.	1–6; 10–12	(2)	6.	3–10	(3)	11.	5–9; 12	(6)
2.	1; 7; 9; 12	(12)	7.	1; 5; 8	(8)	12.	2; 4–6; 8; 10; 12	(8)
3.	4–12	(4)	8.	1–3; 6–8; 10; 12	(6)	13.	1–5; 7; 11–12	(3)
4.	4–8; 11–12	(5)	9.	1; 3; 5; 7; 9; 11	(11)	14.	1; 4; 7; 9–12	(9)
5.	2–5; 8; 10–11	(4)	10.	1–12	(1)	15.	2; 4; 6; 8; 10; 12	(12)

The apparatus was placed on a small table. Its operation was explained to *S*, in the following manner.

See this setting? (*E* pushes out Set 1, Problem I) and this set? (*E* presents Set 2), and this one (Set 3)? In each set there is a key, which on being pressed down, sounds a buzzer. This key is the correct key for that particular set, and, of course, in any given set there is but one key which will sound the buzzer. Now in all of these settings, and I have more, there is a common factor which determines which will be the correct key. In other words, you can work out a rule which will tell you which key will be correct for any setting I may give you. Is this clear? (*E* repeats material of directions if necessary.)

Your task is to work out a rule in as few 'sets' as you possibly can. Push down whatever keys you wish, while you look for the correct key. Analyze the situation as you go along. Think aloud. Take your time. Do not rush. I shall give you as many sets as you want. Tell me when you are ready for a new one. As soon as you have a rule to offer, announce it at once, and I shall then give you some sets to test it out. If your rule works, you have solved the problem. Remember to watch carefully for the critical factor determining which is the correct key for each set, and to think aloud.

These directions were repeated if necessary. After the completion of the first problem, *S* was given the additional instructions:

You have just solved one problem. Shall I reread your directions? Every problem will be presented in the same manner as the one you have just finished. Each, however, depends on a different factor for its solution. Do not forget that. Remember that it is important that you think aloud, and that you announce your solution to me just as soon as you can.

Beginning with Problem IV, the *Ss* were allowed to use notes, but they were told that this privilege did not signifiy that the problems were getting more difficult but that they were merely allowed this aid as some *Ss* had complained that "solving a problem was just a matter of a good memory." "Note taking" was to be construed merely as a convenience to the *S*, and to be done in any manner he desired, or not at all, if he so saw fit. The same directions given at the beginning still held.

The "thinking aloud" enabled *E* to keep in close touch with *S*'s progress. If *S* found it difficult or distracting, it was not insisted upon. The privilege of taking notes on the latter problems (Problems IV, V, and VI) makes the situation less artificial, and helps to eliminate the objection made by many *Ss* during the preliminary experiments, viz. that solving the problems was a matter of memory.

(C) Experimental conditions. The experimental work was conducted in a quiet room. Preliminary work with 20 *Ss* was done during the school year 1928-1929. The results of these experiments are not included, however, in this report. The results here reported were obtained during the years 1929-1931 from 60 *Ss*, of whom 49 were undergraduate students, 6 graduate students *(KS, JC, TR, MeB, AG, MP)*, 2 members of the staff (*EMT, MM*), 2 young school teachers (*IK, LG*), and 1 commercial student (*CS*). The sexes were equally divided. The ages of the *Ss* varied from 16-30 yr.; most (50), however, being between 17-22 yr. All were naïve, at the outset of the experiment, in this kind of work.

As has been indicated, there were two card periods, each separated by intervals of approximately 48 hr. The average time necessary for the solution of the problems was roughly 2 hr. In view of fatigue, the *Ss* were not allowed to work more than an hour at a time. Because of individual differences in the rate of working, no rigid rule could be followed regarding the number of multiple choice problems to be completed in one experimental sitting. In general, however, Problems I, II, III, and IV were solved in the first sitting, and V and VI in the last.

(*D*) *Method of scoring. (1) Cards.* Correct cards were counted until more than three successive errors were made. Errors were of name, suite, and omission.

(2) Multiple choice. Scoring was done in terms of the number of 'sets' necessary until an adequate solution was formulated.[10] The scores represent the difference between this value and the number of sets necessary to illustrate the problem.

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[10] The number of 'sets' has been used rather than the number of 'errors' (moves): (1) because of the emphasis on the set as a situation; and (2) because of the complications in Problem IV, where on the basis of 'errors,' an *S* going through 40 sets might receive a better score than another *S* who experimented with various keys finally getting the problem in 20 sets.

TABLE II

Raw Scores for the Different Ss in the Different Problems

S	Multiple Choice						Cards	
	I	II	III	IV	V	VI	1	2
MM	1	1	1	3	2	2	17	31
IK	0	1	2	4	4	2	15	34
RA	1	1	1	7	8	23	20	30
KS	2	1	2	4	1	NC	12	10
HF	1	1	7	3	9	19	6	9
EMT	1	1	1	37	1	3	11	14
FR	3	1	2	7	23	23	11	16
JO	1	1	1	9	37	3	11	29
LL	3	1	2	26	28	14	21	30
CF	1	2	2	38	0	NC	8	12
MeB	0	0	4	25	NC	4	11	14
BZ	1	1	11	29	10	35	18	31
BC	1	16	3	23	26	9	14	24
Le	1	1	8	45	39	3	24	35
LS	1	1	4	DNC	19	5	27	35
RN	1	1	4	DNC	30	3	19	38
ML	1	DNC	4	DNC	38	31	12	32
LA	1	1	4	7	9	DNC	12	14
ET	1	2	3	17	2	DNC	13	33
ES	2	1	8	4	15	DNC	7	15
FG	2	1	4	3	27	DNC	15	28
AG	1	1	1	43	1	DNC	12	9
RP	5	0	1	11	39	DNC	17	29
IB	0	2	1	8	39	DNC	13	37
ErB	4	2	3	29	27	DNC	10	18
RC	1	13	3	23	NC	DNC	6	10
BS	1	10	2	DNC	DNC	1	13	13
SF	1	DNC	DNC	3	DNC	4	8	13
JR	2	1	DNC	DNC	DNC	40	13	27
NR	3	2	13	DNC	24	DNC	4	5
AF	1	1	2	DNC	NC	DNC	12	28
DR	1	2	6	DNC	NC	DNC	11	14
EK	1	5	19	DNC	NC	DNC	15	10
MP	1	1	2	1	DNC	DNC	16	20
HH	3	2	2	5	DNC	DNC	14	19
JS	1	2	5	1	DNC	DNC	11	16
LG	1	2	3	14	DNC	DNC	12	16
TR	1	3	3	15	DNC	DNC	22	32
VK	1	1	7	15	DNC	DNC	15	17
WL	1	2	13	10	DNC	DNC	16	21
SJ	1	1	2	38	DNC	DNC	16	18
SS	2	15	10	6	DNC	DNC	20	32
EJ	6	3	19	16	DNC	DNC	7	11
BK	2	1	DNC	DNC	7	DNC	12	15
EmB	1	1	DNC	DNC	9	DNC	8	18
AW	5	1	DNC	DNC	21	DNC	22	29
DB	6	DNC	11	38	DNC	DNC	18	24
SG	2	1	DNC	32	DNC	DNC	9	15
RH	1	0	1	DNC	DNC	DNC	11	18
CL	1	1	1	DNC	DNC	DNC	26	49
EL	1	0	5	DNC	DNC	DNC	7	15
SK	6	2	3	DNC	DNC	DNC	7	11
EdL	1	7	7	DNC	DNC	DNC	16	29
PJ	1	1	18	DNC	DNC	DNC	25	37
CS	1	14	4	DNC	DNC	DNC	4	9
MaB	1	9	10	DNC	DNC	DNC	15	15
EF	1	DNC	4	DNC	DNC	DNC	9	17
GK	2	2	DNC	DNC	DNC	DNC	11	17
AC	3	2	DNC	DNC	DNC	DNC	9	15
LR	2	18	DNC	DNC	DNC	DNC	13	16
Mean	1.70	2.98	5.08	16.6	14.9	12.5	13.5	21.3

These subtraction values are as follows: Problem I, 1; II, 2; III, 1; IV, 2; V, 2; and VI, 5.

Unknown to *S*, the number of sets to be given for each problem was limited: Problems I, II, and III to 20 sets; and Problems IV, V, and VI to 45 sets. After the allotted number of sets had been used by an *S* (without resulting solution), he was given a DNC unless his "thinking aloud" seemed to indicate that a solution was near. In this event, he was given several more sets (usually 10). In all cases but one *(Le)* these extra sets did not result in a solution. The actual solutions were divulged indirectly to *S*s receiving DNC by means of pointers, and special sets, in such a way that they would think themselves responsible for most of the solution.

NC represents an adequate but inferior solution. These will be discussed later in greater detail.

(E) Method of treating results. (1) Cards. Inasmuch as Period 1 with the cards may be considered a period of practice in method for the *S,* the results for Period 2 alone have been used. This second period is complicated by the inclusion of a row of Hearts (A, 8, Q, 6) in the center of the deck, to serve as an objective and incentive. Scores, therefore, have been ranked.

(2) Multiple choice. The problem of DNC and NC has been attacked in two ways. For intercorrelations, for which the results of individual problems are used, scores have been given rank orders. DNC and NC, hence, receive the median rank of their group. At best this method is somewhat gross, as can be seen from the fact that an *S* with a score of two receives a rank of 39, and a rank of 18.5 for a score of one (Problem II). The results of Problem I have not been used for the reason that they show but little scatter, and hence, have low value as a comparatively sensitive measure of individual differences. Moreover, Problem I serves as an introductory problem. Obviously, these individual problem rankings are inadequate for combination into battery form, for all or any portion of the total number of problems. Further difficulty is encountered in using these individual ranks in dealing with the problem of DNC. A DNC for Problem III receives a rank of 56, for Problem VI, a rank of 40.5. It is thus possible for an *S* to receive a 'worse' total ranking by missing an 'easier' problem and solving the latter problems, than by completing the earlier problems and not completing the latter ones.

It therefore becomes necessary to use some method of weighting based upon some criterion of difficulty, in handling these DNC. The use of the mean does not seem feasible with these data. An inspection of the means for each problem reveals that the mean reaches a peak in Problem IV, and then diminishes, whereas the number of DNC does not keep pace. The following shows this in unambiguous terms:

Problem	I	II	III	IV	V	VI
Mean	1.70	2.98	5.08	16.6	14.9	12.5
No. DNC	0	4	9	24	27	40

The number of DNC for Problem VI is almost double that for Problem IV, although the mean for Problem VI is much lower. In this connection, it should be stated that DNC and NC do not enter into the computation of these means. The means for the latter problems are based on fewer cases as a consequence.

The criterion of difficulty chosen was the number of *S*s completing the given problem. With 56 as a base (Problem II), the weighting indices are as follows:

Problem	II	III	IV	V	VI
Weighting Index	1.000	1.098	1.555	1.697	2.800
Reciprocal	1.0000	.9107	.6429	.5893	.3571

Treating the scores now becomes a matter of evaluating *S*'s performance on the basis of (a) the problems completed, and (b) the score obtained in them. On the basis of (a) an *S*'s performance will fall into one of 15 groups on the basis of summation of weighting indices for those problems he completes. Thus an *S* completing all the problems will fall in the first ranking group (Table III) with a summative ranking index of 8.15. The position of this *S* within the group will depend upon his performance in the individual problems (b). These scores, in terms of sets, are equated by the use of the reciprocal of the individual problem indices, and summed. By means of these two steps it is possible to rank the *Ss* in a very clearly differentiated manner. In only one instance do 2 *Ss* receive the same ranking (*GK* and *AC*).

TABLE III

Summative Ranking Indices in Terms of Problems Completed

Problem						Battery	
II	III	IV	V	VI	f	A	B
					14	5.350	8.150
		+			2	3.795	6.595
+		+			1	2.795	5.595
				+	9	5.350	5.350
		+	+		1	2.098	4.898
+	+		+		1	1.555	4.355
	+	+	+		1	1.000	3.800
		+		+	4	3.795	3.795
			+	+	10	3.653	3.653
	+	+		+	3	2.697	2.697
+			+	+	1	2.653	2.653
	+		+	+	1	2.555	2.555
		+	+	+	8	2.098	2.098
+		+	+	+	1	1.098	1.098
	+	+	+	+	3	1.000	1.000

Table III gives the summative ranking indices and *S* frequencies in terms of problems completed. Blank spaces indicate that the problem was solved; plus signs represent DNC. For reasons given earlier, the results for Problem I are not used. Because of the large number of DNC for Problem VI, the summative ranking indices are given for two batteries—A and B. Battery A includes Problems II, III, IV, V; and B, Problems II, III, IV, V, and VI.

TABLE IV

	DNC Distribution in Problems				
Problems	II	III	IV	V	VI
II	(4)				
III	1	(9)			
IV	2	7	(24)		
V	3	6	14	(27)	
VI	2	7	19	24	(40)

The distribution of the DNC for the various problems is given in Table IV. The parenthetical number represents the total number of DNC for the corresponding problem number listed at the head (and side) of the column. The distribution is given by the other values listed, for example (line 2 of table): of the four *Ss* receiving DNC in Problem II, one did not complete Problem III,

TABLE V

Rankings for the Different Ss in the Different Problems

	Multiple Choice						Battery		Cards
S	I	II	III	IV	V	VI	A	B	2
MM	22.0	18.5	5.0	4.5	5.5	2.5	1.0	1.0	12.5
IK	2.0	18.5	14.5	8.0	7.0	2.5	3.0	2.0	7.0
RA	22.0	18.5	5.0	13.0	9.0	14.5	4.0	3.0	14.5
KS	45.0	18.5	14.5	8.0	3.0	19.5	2.0	4.0	55.0
HF	22.0	18.5	39.0	4.5	11.0	13.0	6.0	5.0	58.0
EMT	22.0	18.5	5.0	31.0	3.0	5.5	11.0	6.0	46.5
FR	52.0	18.5	14.5	13.0	17.0	14.5	9.0	7.0	36.5
JO	22.0	18.5	5.0	16.0	24.0	5.5	13.0	8.0	17.5
LL	52.0	18.5	14.5	27.0	22.0	12.0	18.0	9.0	14.5
CF	22.0	39.0	14.5	33.0	1.0	19.5	12.0	10.0	51.0
MeB	2.0	2.5	30.5	26.0	31.0	8.5	20.0	11.0	46.5
BZ	22.0	18.5	45.5	28.5	13.0	17.0	17.0	12.0	12.5
BC	22.0	55.0	23.0	24.5	19.0	11.0	21.0	13.0	23.5
Le	22.0	18.5	41.5	36.0	27.0	5.5	23.0	14.0	5.5
LS	22.0	18.5	30.5	48.5	15.0	10.0	26.0	15.0	5.5
RN	22.0	18.5	30.5	48.5	23.0	5.5	28.0	16.0	2.0
ML	22.0	58.5	30.5	48.5	25.0	16.0	40.0	17.0	10.0
LA	22.0	18.5	30.5	13.0	11.0	40.5	5.0	18.0	46.5
ET	22.0	39.0	23.0	23.0	5.5	40.5	7.0	19.0	8.0
ES	45.0	18.5	41.5	8.0	14.0	40.5	8.0	20.0	41.5
FG	45.0	18.5	30.5	4.5	20.5	40.5	10.0	21.0	20.5
AG	22.0	18.5	5.0	35.0	3.0	40.5	14.0	22.0	58.0
RP	56.5	2.5	5.0	18.0	27.0	40.5	15.0	23.0	17.5
IB	2.0	39.0	5.0	15.0	27.0	40.5	16.0	24.0	3.5
ErB	55.0	39.0	23.0	28.5	20.5	80.5	19.0	25.0	29.5
RC	22.0	52.0	23.0	24.5	31.0	40.5	22.0	26.0	55.0
BS	22.0	51.0	14.5	48.5	47.0	41.0	50.0	27.0	49.5
SF	22.0	58.5	56.0	4.5	47.0	8.5	55.0	28.0	49.5
JR	45.0	18.5	56.0	48.5	47.0	8.0	57.0	29.0	22.0
NR	52.0	39.0	47.5	48.5	18.0	10.5	29.0	30.0	60.0
AF	22.0	18.5	14.5	48.5	31.0	40.5	24.5	31.0	20.5
DR	22.0	39.0	37.0	48.5	31.0	40.5	27.0	32.0	46.5
EK	22.0	48.0	50.5	48.5	31.0	40.5	20.0	33.0	55.0
MP	22.0	18.5	14.5	1.5	47.0	40.5	30.0	34.0	26.0
HH	52.0	39.0	14.5	10.0	47.0	40.5	31.0	35.0	27.0
JS	22.0	39.0	35.5	1.5	47.0	40.5	32.0	36.0	36.5
LG	22.0	39.0	23.0	19.0	47.0	40.5	33.0	37.0	36.5
TR	22.0	46.5	23.0	20.5	47.0	40.5	34.0	38.0	10.0
VK	22.0	18.5	39.0	20.5	47.0	40.5	35.0	39.0	33.0
WL	22.0	39.0	47.5	17.0	47.0	40.5	36.0	40.0	25.0
SJ	22.0	18.5	14.5	33.0	47.0	40.5	37.0	41.0	29.5
SS	45.0	54.0	43.5	11.0	47.0	40.5	38.0	42.0	10.0
EJ	59.0	46.5	50.5	22.0	47.0	40.5	39.0	43.0	52.0
BK	45.0	18.5	56.0	48.5	8.0	40.5	41.0	44.0	41.5
EmB	22.0	18.5	56.0	48.5	11.0	40.5	42.0	45.0	29.5
AW	56.5	18.5	56.0	48.5	16.0	40.5	43.0	46.0	17.5
DB	59.0	58.5	45.5	33.0	47.0	40.5	44.0	47.0	23.5
SG	45.0	18.5	56.0	30.0	47.0	40.5	45.0	48.0	41.5
RH	22.0	2.5	5.0	48.5	47.0	40.5	46.0	49.0	29.5
CL	22.0	18.5	5.0	48.5	47.0	40.5	47.0	50.0	1 0
EL	22.0	2.5	35.5	48.5	47.0	40.5	48.0	51.0	31.5
SK	59.0	39.0	23.0	48.5	47.0	40.5	49.0	52.0	52.5
EdL	22.0	49.0	39.0	48.5	47.0	40.5	51.0	53.0	17.5
PJ	22.0	18.5	49.0	48.5	47.0	40.5	52.0	54.0	3.5
CS	22.0	53.0	30.5	48.5	47.0	40.5	53.0	55.0	58.0
MaB	22.0	50.0	43.5	48.5	47.0	40.5	54.0	56.0	41.5
EF	22.0	58.5	30.5	48.5	47.0	40.5	56.0	57.0	33.0
CK	45.0	39.0	56.0	48.5	47.0	40.5	58.5	58.5	33.0
AG	52.0	39.0	56.0	48.5	47.0	40.5	58.5	58.5	41.5
LR	45.0	56.0	56.0	48.5	47.0	40.5	60.0	60.0	36.5

two did not complete Problem IV, three did not complete Problem V, and two did not complete Problem VI. It will be noted that in all cases but two, there is an increase in the number of DNC as we go from the easier to the more difficult problems. This, too, is in accord with our criterion. The two inversions are minimal.

In Table V will be found the raw data converted to ranks. This method of combining the scores has several advantages. The weighting index is not arbitrarily fixed, but varies directly with the actual results. One may consider 56 points to be available for each problem solved, the number of *S*s solving a given problem determining the 'credit' to be given each *S* for that problem. The number of *S*s solving a given problem will vary with the number of DNC for that problem. Yet, it will not be necessary to deal directly with the DNC in treating the data, either by way of assigning arbitrary scores to them, using a method of ranking, or percentiling. In addition, our method can be used conveniently to combine any problems in the battery, so that one need not depend on the efficacy of partials in working out the effect of each problem on the battery. A further advantage of this method is that within each ranking group, an *S* is compared only with other *S*s who have successfully completed the same problems as he.

NC have been regarded as successful solutions, in determining the summative rank index of the *S* under consideration. In computing the *S*'s rank within the rank group, NC were given the maximum number of sets possible for that problem. As has been stated earlier, these NC cases represent an adequate but 'inferior' solution. For example, one *S* (*EK*) formulated the following as the solution of Problem V: "When there are 3 keys, it's the first; 5 keys, the second; 7 keys, the third; 9 keys, the fourth; 11 keys, the fifth. Each time you count from the opposite side that you counted from before." The solutions offered by the other 4 NC for this problem are similar to *EK*'s. Two *NC* will be found for Problem VI (*KS* and *CF*). In both instances a solution was offered, "when there's 1 group, it's the first key; 2 groups, the second key." Nothing was offered for 3 and 4 groups, although the correct key was found in every instance without error. The NC total 7 in number and fall in Problems V and VI only.

The question arises—can these final scores in terms of rank, be regarded as scores? Strictly speaking, the answer is no. The most telling criticism of ranking is that differences in rank and in score are not commensurate. Our data, however, reveals comparative continuity both in respect to differences in the same summative rank group, and differences between groups. In any given group the differences from *S* to *S*, for the highest part, are both clean cut and comparatively regular. The differences from group to group are not great when considered in terms of the frequencies of *S*s falling into the various groups. It seems to the writer that the limitations usually imposed by a method of ranking are at a minimum in his treatment, and that in a looser sense, although a comparatively accurate one, these ranks may be construed as scores.

The foundation of our method of treating the multiple choice results is the criterion chosen for difficulty. A consequence of this criterion is that an *S*'s rank is determined primarily by the problems he completes, and only secondarily by his proficiency (in terms of sets necessary for solution) in these problems. As a result, no matter how well an *S* may acquit himself in the problems he solves, a second *S*—who may require the maximum number of sets, but who on the other hand solves either more problems or an equal number of greater "weight"—will receive a higher rank than the first. This consideration presents a great difficulty to any plan for converting the *S*'s complete performance to score form. We have tried several other methods of treating the results, but have been unable to overcome this difficulty without losing the advantages gained by this ranking technique. Among these other methods have been: (a) using the total number of sets possible, as a base for weighting indices (the proportionality of each index to the others is not changed), adding the reciprocals to the summed equated values of individual problem scores; (b) using a value derived by dividing the total number of sets possible by the summative rank index, and adding the equated scores to this value as a base; and (c) adding the maximum number of sets for each problem not solved to the *S*'s summed equated scores. All these methods gave scores to some *S*s which were at variance with his criterion of difficulty. Using larger although still proportional (with reference to the criterion) gaps between ranking groups, introduces factors not dependent on the data themselves. An artificial discontinuity is introduced between *S*s falling in one group and *S*s falling into an adjacent group. It should be remembered that these groups consist of *S*s who have solved the same problems. These groups may be considered as the 15 performance patterns made by the *S*s in solving the problems. These 'patterns' are in no way dependent upon any criteria of difficulty.

It seems reasonable to assume that the data has continuity, and that the continuity between groups (obtained by our ranking procedure) is more nearly adequate than the discontinuity between groups attendant on the introduction of a "gap" arbitrarily made large enough to prevent overlapping between groups, as is necessary in accordance with our criterion.

RESULTS

(1) *Cards.* An examination of the gross results (Table II) indicates an average improvement of 57.8% for Period 2 over Period 1, with an S.D. of 2.68 for Period 1, and 4.74 for Period 2. All *S*s but 5 (*KS, AG, BS, EK, MaB*) improved in the second period. Of these *S*s, 3 showed losses. Improvement in the remaining 55 *S*s varies from 1-24 cards. The average improvement for all cases is 7.8 cards. In terms of the standard errors of the means, the reliability of the difference is expressed by 7.8 ± .703. Period 2 correlates 0.777 ± .034 with Period 1 (product-moment).

The method seems adequate to our purpose. The range is large, much larger than with most 'learning methods' currently used. It did not prove necessary to teach the names of the cards to any *S*. The conditions imposed upon *S* in learning the cards, lessen the artificiality of the laboratory conditions usually imposed. The conditions more nearly approximate those of his everyday learnings in that the *S* is free to set his own pace.

(2) *Multiple choice.* In view of the diversity of 'principles' in the various problems, it will be interesting to work out the intercorrelations of problems of the battery. These values will be found in Table VI. Individual problem rankings have been used in the computations. The limitations of these values have been discussed above on page 423. These correlations are, therefore, somewhat gross and give us little more than trends.

How valuable is each problem to the battery? We have gone about answering this question in two ways: (a) the correlation of each problem

TABLE VI

Intercorrelations of Problems

Problems	II	III	IV	V
III	.33 ± .078			
IV	.20 ± .084	.34 ± .077		
V	.44 ± .070	.34 ± .077	.30 ± .079	
VI	.37 ± .075	.39 ± .074	.35 ± .076	.55 ± .061

with the battery; (b) the effect on the battery when the individual problem is omitted.

(a) The correlation of each problem with the battery,[11] both when A and B are used, is given in Table VII.

(b) With the method of battery combination outlined above (p. 424), it is possible to combine any of the problems used. Hence, we can correlate a battery consisting of Problems III, IV, V, VI, with a battery consisting of Problems II, III, IV, V, VI; a battery of Problems III, IV, V with II, III, IV, V and find the effect of Problem II, and so on. These values are given in Table VIII. Battery A consists of Problems II, III, IV, V; and B, of Problems II, III, IV, V, VI. Parentheses indicate omission, hence (II) under A designates that this correlation value is of Problems III, IV, V with a battery consisting of Problems, II, III, IV, V, and so on.

TABLE VII

Correlations of Each Problem with Batteries

Problem	Battery	
	A	B
II	.45 ± .069	.39 ± .074
III	.57 ± .059	.53 ± .063
IV	.65 ± .050	.56 ± .060
V	.81 ± .030	.78 ± .034
VI	.48 ± .067	.78 ± .034

TABLE VIII

Correlations to Show the Effect of Problem on Battery when Problem in Question Is Omitted

Problem omitted	Battery	
	A	B
(II)	.99 ± .002	.99 ± .002
(III)	.96 ± .007	.97 ± .005
(IV)	.92 ± .013	.94 ± .010
(V)	.79 ± .033	.90 ± .017
(VI)		.87 ± .021

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[11] All correlations, except as otherwise stated, have been done by the method of Rank Differences: $\rho = 1 - 6\Sigma(R_x - R_y)^2/N(N^2 - 1)$; $r = 2 \sin[(\pi/6)\rho]$.

From Table VII it will be seen that, when the entire battery (B) is considered, Problems V and VI correlate rather highly and equally with the battery. Moreover, except in the case of Problem VI, the correlations in Batteries A and B vary but little. The greater difference in the case of Problem VI (0.48 and 0.78) may be due to the fact that the effect of Problem VI is not felt in Battery A. The scores of individual problems were taken care of by rankings, in the computations of these correlations.

In determining the effect of individual problems on the battery by means of (b) the procedure is somewhat indirect. When a particular problem is omitted from a battery, the greater the correlation with the complete battery, the less the effect (or disturbance) may be said to be due to this problem, and hence, the less the problem under consideration contributes to the battery. Table VIII clearly reveals that Problem II contributes least, and Problem VI the most (when Battery B is considered). The intermediate differences are not great, however. The equivalent values for Battery A vary but slightly from those for B, except in the instance of Problem V.

It would seem, from an inspection of Tables VII and VIII, that the battery of problems given the *S* might be reduced without suffering especially in efficacy. Some of the possible combinations of problems and their correlations with Batteries A and B are given in Table IX. The best combination for both batteries would seem to be a combination of III, IV and V; with IV and V almost as efficient. These combinations should be verified experimentally, since the results of our preliminary experiment give evidence that the "order of presentation" is a factor in their difficulty to the *S*.[12]

(3) *Correlations between cards and multiple choice problems.* The correlation between the results in card learning and Battery A is 0.08 ± .087, and between card learning and Battery B is 0.10 ± .086. The correlation between card learning and the different multiple choice problems is: Problem II, 0.23 ± .083; III, 0.19 ± .084; IV, 0.09 ± .086; V, 0.07 ± .087; and VI, 0.30 ± .079.

The correlations for the batteries are very low. The correlation of 0.30 of Problem VI with the cards, is based upon data in which 67% of the *S*s (40) receive the same ranking. It is curious that the correlations of individual problems are lowest for Problems IV and V whose results give the greatest scatter. These correlations (.09 and .07) are virtually identical with the battery correlations (.08 and .10), although these two problems do not correlate to a commensurate extent with the two batteries (see Tables VII and VIII). The probable errors are all high, and further reduce the minimal significance of these correlations.

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[12] See also W. Brown and F. Whittell, Yerkes' multiple choice method with human adults, *J. Comp. Psychol.*, 3, 1923, 305-318, esp. 315-316.

TABLE IX

CORRELATIONS OF VARIOUS PROBLEM COMBINATIONS WITH BATTERIES A AND B

Problems	Battery A	Battery B
II, III, IV	.79 ± .033	.64 ± .051
II, III, V	.92 ± .013	.84 ± .026
II, IV, V	.96 ± .007	.87 ± .021
III, IV, V	.99 ± .002	.91 ± .015
IV, V, VI	.83 ± .027	.96 ± .007
IV, V	.94 ± .010	.89 ± .018

(4) *Sex differences.* (a) *Cards.* The data when classified according to sexes reveal an average of 24.4 correct cards for men, and 18.2 for women (Period 2). A proportionate difference is found when the results for Period 1 are considered: 15.5 correct cards for men and 11.1 for women.

(b) *Multiple choice.* These results when analyzed as to sexes, also give the men general superiority. In Table X are listed the average number of sets necessary for solution; the number of *S*s on which these averages are based—the number of *S*s successfully completing the respective problems; the number of DNC; and the number of NC. Neither DNC nor NC enters into the computation of these averages.

TABLE X

SEX DIFFERENCES WITH PROBLEMS

	Problems II		III		IV		V		VI	
	M	W	M	W	M	W	M	W	M	W
Av. no. sets	2.03	4.00	4.26	6.00	20.3	12.6	16.6	19.3	7.4	20.4
No. completing	29	27	27	24	19	17	18	10	11	7
No. of DNC	1	3	3	6	11	13	9	18	17	23
No. of NC	0	0	0	0	0	0	3	2	2	0

It will be noted that, with the exception of Problem IV, the men are definitely superior to the women both from a standpoint of the average number of sets, and the number of DNC. In the aggregate there are 41 DNC for men to 63 for women. In Problem IV alone, do the females prove superior to the males, although the DNC are almost equally distributed.

These sex differences may be considered to be summed up by the sex distribution in the batteries: In Battery B, of the first 30 ranks, 17 are occupied by men (av. 11.5), and 13 by women (av. 20.7). For the entire battery the average rank of the men is 26.17, and of the women, 34.83. The difference is even more marked in Battery A. Here, of the first 30 ranks 19 are occupied by men (av. 14.5), and 11 by women (av. 17.3). The average rank for this entire battery is 25.7 for men, and 35.7 for women.

The method of 'standard error' does not seem an adequate instrument in determining the reliability of the differences of the respective averages of men and women. Our experimental technique has placed a premium on scatter. The data, for the greatest part, show well distributed scores which do not fall into a distribution even analagous to a Gaussian distribution. Our statement of definite sex differences is based on three lines of evidence: (1) respective number of DNC for men and women (multiple choice); (2) the respective averages (multiple choice and cards); and (3) the rank distributions in the batteries and cards. The matter of the possibility of sampling error will be further discussed in a later paragraph.

In view of these sex differences it may be of advantage to correlate the cards with multiple choice, when the sexes are taken separately. These correlations have been computed by correlating the respective ranks of cards and multiple choice, by the product-moment method.

Sex	M	W
Battery A	—.175 ± .119	.181 ± .119
Battery B	—.123 ± .121	.275 ± .114

These correlations, while not high, are suggestive in spite of the high P.E. It will be remembered that Problem VI correlates highest of the individual problems with the cards; and that, of the 20 *S*s completing this problem, 17 were men. We should expect, therefore, an accentuation of the r for the men, when Problem VI is omitted (Battery A). It is evident that such has been the case.

From these results on sex differences it seems highly possible that there may be present definite correlation trends between serial learning and generalizing abstraction when the sexes are treated separately. These trends will be obscured when the results are taken in the aggregate. Our results, however, are based on too few *S*s to make a positive statement.

We have the following evidence to submit that these sex differences are *not* due to the sampling of *S*s used in the investigation: (1) Sampling would not account for the superiority of the women with Problem IV. (2) A sampling error seems incompatible with the negative correlation trend of Cards with Multiple Choice for men, and with the higher positive trend of correlation for women. (3) S.A.T. scores (College Entrance Board) for 42 of the 60 *Ss* (24 women and 18 men) give the 'usual' distribution of the sexes: men, av. 482.1; women, av. 479.3. This item, while not inclusive, seems indicative.

An examination of the nature of Problem IV may give us a few suggestions regarding the respective performance of men and women. In this problem, the principle is 'odd-even,' with the right end of the set as the point of reference. In solving this problem, most *Ss* went through these steps: (1) found point of reference, (2) attempted to work out a sequence to predict the appearance of the first and second keys as the correct key, (3) failing in this, changed method of attack. All DNC went through the first two steps listed. Except for those few *Ss* completing the problem within very few sets, much of the difference in scores may be accounted for in terms of the persistence of the sequence attitude, in spite of the contradictions given the *S* by his own data. There exist, consequently, the possibilities that the sex differences in the performance of this problem may be due to 'persistence' differences between the sexes, or, to a feminine aptitude for odd-even relationships.

SUMMARY AND CONCLUSIONS

A study was made of serial learning and generalizing abstraction, as revealed by the learning of the serial order of a deck of playing cards and the Yerkes multiple choice method respectively. The results on 60 *Ss* show:

(1) A non-significant, although positive correlation between serial learning results and generalizing abstraction results: 0.08 ± .087; 0.10 ± .086.

(2) Sex differences in both serial learning and generalizing abstraction. These differences do not seem entirely accountable for in terms of a sampling error.

(3) Correlations between serial learning and generalizing abstraction results, when the sexes are treated separately, are positive for women (0.181 ± .119; 0.275 ± .114); and negative for men (—0.175 ± 119; —0.123 ± .121). There are too few cases, and the P.E. are quite high. The values are greater, however, than those obtained when the results are taken irrespective of sex, and seem extremely suggestive.

CONFIGURATIONAL TENDENCIES IN RORSCHACH RESPONSES

By SAMUEL J. BECK, Boston Psychopathic Hospital

One of the principal diagnostic indices in the Rorschach technique for personality probing is the *W*, or *whole*, response (Rorschach's *G*, or *Ganz*). As the term indicates, this type of response is one in which the subject reacts and gives meaningful interpretation to the entire ink-blot figure, rather than details of it. There are 10 ink-blot figures in the series composing the test. Of the whole response Rorschach says:

> "Die Zahl der G ist vor allem als Indikator der dispositionellen Energie des Assoziationsbetriebs zu betrachten, oft auch als Indikator des bewussten oder unbewussten Willens zur komplizierteren Leistung (Abstraktions- oder Kombinationsleistung: grosse Zahlen guter, primärer G haben oft philosophisch Veranlagte, grosse Zahlen guter, kombinatorischer G produzieren häufig Phantasiebegabte). Ein Optimum dieser Fähigkeiten ist eine weitere Komponente der Intelligenz."[1]

His table of norms shows the incidence of *W* at various levels of intelligence to be as follows:[2]

Superior	10 or more *W*'s
High average	7–10 *W*'s
Average	4–7 *W*'s
Low average	3–4 *W*'s
Moron	1–3 *W*'s
Imbecile	0–2 *W*'s

It would follow from this discussion and these norms that wholes (*W*'s) are a function of intelligence. The writer's experience with the test in a number of groups has, however, clearly indicated a necessary qualification of this principle. For it becomes quickly manifest that some of the ink-blot figures are much more frequently interpreted as wholes (*W*'s) than others. Thus the fifth in the series is almost always seen as a whole (*W*); the fourth and first very frequently. The tenth on the other hand is almost never interpreted as a whole (*W*), and the ninth and eighth with great rarity. If the whole (*W*) response is an indicator of intelligence, is it equally valuable as such an indicator in figures yielding such unequal number of wholes (*W*'s)? It is possible, in fact, that in the fifth figure the *W* is a sign of lack of, or lack of functioning of, intelligence, on the theory that it is most easily interpretable as a *W*, and that breaking it up into details is an index to a heightened intellectual activity, capable of breaking the stimulus up into meaningful parts. In a word, frequency of *W*'s in the respective cards varied. The blots vary in configuration, shading, and color. Presumably the difference in frequency of *W*'s is related to these variations. To the extent that the whole re-

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[1] H. Rorschach, *Psychodiagnostik*, 1921, 49.

[2] *Ibid.*, 40 f. Rorschach's terms are, respectively: "Künstler, Intelligente, Durchschnitt, Unintelligente, Debile, Imbezile." He does not state the classifications to have been made on the basis of any psychometric instrument.

sponse is based only on configuration the notion of its value as indicator of intelligence is in fact susceptible to *reductio ad absurdum*. For, let us assume, as one of the figures, a solid black square. All responses to it would very likely be *W*s. Would they still be an indicator of intelligence?

If, in other words, frequency is an index to difficulty value, and difficulty value is an index to level of intellectual functioning, then the value of *W*, as indicator of height of associational energy, varies in the respective figures. That is, the value of *W* as indicator of associative energy, varies with the conditions under which *W* is obtained, the variation in these conditions being represented by the variation in configuration, shading, and color in the respective figures. What, then, is the *W* value for each figure? As value varies with the conditions, what light is thrown on the fluctuation of this psychological process, associative energy—or more accurately, organizing energy[3]—with change in conditions? In examining the material forming the basis of this report, quantitative findings were available making possible some tentative conclusions on this 'organizing energy,' not only in so far as it issues in production of whole responses, but also in associating various details into larger, meaningful combinations. These organized combinations fall into fairly clear-cut groups. Thus the combination may consist of (a) adjacent details; (b) distant details, separated either by white space or by solid portions; (c) white spaces combined with adjacent solid portions; (d) portions of the figure were broken up, and the elements so obtained recombined into meaningful percepts, an analysis-synthesis process; (e) whole, *W*, responses; (f) elements of the figure were successively seen in meaningful relation to one another, until the entire figure was included in the response, but the reaction was at no time to the whole of the figure; (g) what may be described as a "striving towards a whole," without it being achieved, as when one *S*, reacting to the tenth figure, said: "The drawing doesn't seem to be held together; they [the details of it] are scattered; they give you a feeling of being thrown together more than put there." Another *S*, in responding to the ninth figure complained of its "lack of integration." Incidence of the combinations classified under the first five headings was sufficiently high to make possible statistical treatment. The other two groups were represented by too few for such treatment. Examples of the respective groups appear on the page opposite.

Fig. 1 reproduces a portion of the tenth Rorschach figure. The response was: "These green figures (A) are like beetles, or scarabs, . . . worshipping a central figure (B); they seem to be about to fall down; to prostrate themselves." Thus two details which are rarely associated with each other were seen in relation by this *S*. The mental act included a leap across an intervening white space. The example is cited as illustrating the association of distant details.

This response is also interesting in that it seems to illustrate processes emphasized by the *Gestalt* group. Thus Detail A was not seen alone, nor was Detail B, but the two together have a meaning, in this particular response, not attaching to either of the parts independently but to both as one larger whole. It may be mentioned in passing that many of the organized combinations reported on in this paper offer evidence of the operation of this *Gestalt* principle. It is not, however, within the scope of the present report to consider this element.

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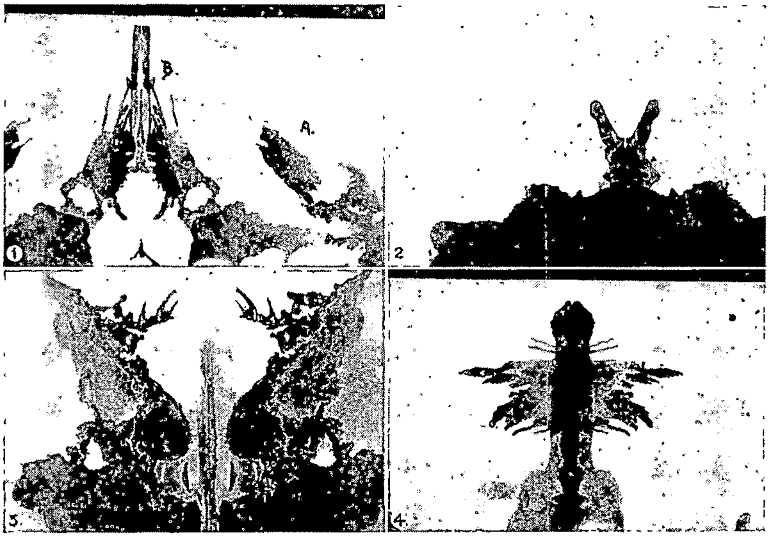
[3] Rorschach's language is "Assoziationsbetrieb." It seems clear from the text that "Assoziation" is used in the sense not only of content recalled by the stimulus, *i.e.* the usual sense of the word "association," but also in the sense of associating various details together with reference to one another, *i.e.* organizing them.

In Fig. 2, is reproduced a portion from the fifth figure in the test series. The response was: "These little things might be two animals kissing each other." This was classified as an association of details adjacent to each other. An example of organization of solid material with white space, both entering into the meaningful interpretation, is illustrated in Fig. 3, taken out of the ninth figure in the series. The response was: "This might be a tulip, or it might be a plant similar to a sensitive plant, and it catches insects," the inner space being the portion where the insects are trapped as the projecting details close. The analysis-synthesis process is exemplified in the response cited to the portion from the sixth figure in the test series, reproduced in Fig. 4. It "looks like some one stuck up on a crucifix," said *S*, explaining that the lateral flares might be arms sticking out. The inner middle portion which is darker than the flares is the body. Thus the entire detail is first broken up and then recombined. Determination of combinations classified in this group could not be made with the same certainty as for the other groups. Evidence was sought by questioning the *S*s. But this evidence is probably not always reliable. Thus it is possible that in the example cited the reaction was first, to the entire detail, and the apparent analytic process only a rationalization by *S*. The decision as to classifying responses in this group was thus, in a number of cases, made by the examiner on equivocal evidence. To that extent it was a subjective judgment on his part.



FIGS. 1–4. EXAMPLES OF THE INK-BLOT TEST FIGURES

Fig. 1 is a portion of Rorschach's tenth figure; Fig. 2, of his fifth; Fig. 3, of his ninth; and Fig. 4, of his sixth.

Whole responses, as has already been stated, are those in which all elements of the presented material are brought together in one meaningful percept, as "a butterfly," among the most common responses to the first figure. But when a subject, reacting to the same figure, said, "An owl sitting up on a tree," he combined all the elements of the figure without at any time reacting to it as a whole. The middle section of the whole figure was seen as an owl, and the lateral portions as the tree.

The various kinds of organizing processes were not mutually exclusive and *a priori* there is no reason why they should be. Thus a response in the eighth figure of the test series may include a bear stepping over water (white combined with solid), and he is stepping onto a rock, which makes a scene reflected in the portion below (adjacent details). More than one kind of associative process may thus be found in the same response. When so found they were each classified in their respective groups.

Observers. The responses forming the material for the present report were obtained from 39 intellectually very superior individuals: 3 psychologists of senior standing with long records of scientific achievement; 4 psychologists of junior rating; 7 graduate and senior students in psychology from Harvard and Radcliffe; 2 industrial psychologists; 4 social workers; 5 graduate students in social work; 7 physicians, and a social work administrator, a high Federal government official, a mathematician, a statistician, a lawyer, a businessman, and an office secretary. The group proved extremely productive, yielding a total of 2215 responses, with a range from 14 to 139, and a mean of 56.79.

In the present study the usual Rorschach technique of classifying the responses was disregarded in favor of a purely quantitative treatment of organizing acts resulting in meaningful combining of elements in the figures presented to the subject. The procedure consisted in obtaining, for each of his 10 figures, (1) the total number of responses; (2) the total number of organizing acts; and (3) the total number of responses in which more than one organizing act occurred. From these data it was possible to calculate the total number of responses in which no organizing process occurred, as well as the total number of organizing processes which came to expression.

As an example, the findings for the tenth of Rorschach's figure were as follows:

Whole (*W*) responses	19
Combination of distant details	32
Combination of white with solid details	13
Combination of adjacent details	41
Analysis-synthesis processes	3
Combinations into *W* without any one being *W*	4
Total organizing acts	112

However, some of these combinations appeared in the same response, and it was found that there were 106 responses in which one or more associations were affected. There were a total of 331 responses to this figure. We add and subtract as follows:

Total responses	331
Responses in which combinations were found	106
Responses in which there were no combinations, *i.e.* only individual details were reacted to	225
Total organizing acts	112
Mental acts that came to expression	337

It is possible now to obtain a sigma value for each type of associational activity, using the method described by Garrett.[4] Thus in the case of the tenth figure we would get the following values:

	No.	*%*	*Sigma Value*	*Sigma Distance from zero point*
Zero point of associational activity	225	66.7	—.43	
Wholes	19	5.6	1.58 + .43	2.01
Distant details	32	9.5	1.31 + .43	1.74
White with solid	13	3.8	1.77 + .43	2.20
Adjacent details	41	12.1	1.17 + .43	1.60
Analysis-synthesis	3	.09	2.36 + .43	2.79

The same procedure was applied to all 10 figures. The results thus yield the sigma value of each kind of associative activity for each figure.

Results. The results of this treatment of the Rorschach test figures are given in Table I, which gives the sigma values for every one of the figures for every type of associative organization.

TABLE I

SIGMA VALUES FOR EVERY TEST FIGURE FOR EVERY TYPE OF ASSOCIATIVE ORGANIZATION

W = whole response; *A* = combination of distant elements; *B* = combination of white space with solid material; C = combination of adjacent material; *F* = analysis-synthesis process.

Type of organization	Rorschach test figure									
	I	II	III	IV	V	VI	VII	VIII	IX	X
W	.46	1.32	2.02	.70	.38	.90	.92	1.63	2.00	2.01
A	2.21	2.09	1.44	1.35	1.96	2.34	1.15	1.15	1.78	1.74
B	1.39	1.69	1.70	1.91	1.59	2.47	1.55	1.60	1.96	2.20
C	1.60	1.16	1.28	1.57	1.06	1.21	.48	1.20	1.20	1.60
F	2.36	2.84	2.37	2.41	1.66	1.69	2.06	2.33	2.26	2.79

Sigma values are thus seen to differ with each figure, for the various associative processes. To the extent that sigma value is an index to difficulty, the difference in difficulty of performing each of the above associated processes is indicated. This difficulty is seen to vary with variation in conditions, as represented by the variation in the 10 figures.

The central tendency and scatter for every type of associative organization is given, for the test series as a whole, in Table II.

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[4] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 101-104.

TABLE II

Central Tendency and Scatter for Every Type of Associative Organization

W = whole response; *A* = combination of distant elements; *B* = combination of white space with solid material; C = combination of adjacent material; *F* = analysis-synthesis process.

Type of organization	Median	P.E.	P.E.$_M$
W	1.120	.6175	.244
A	1.760	.3875	.153
B	1.695	.1825	.072
C	1.205	.1575	.062
F	2.345	.2575	.102

The differences in medians indicate a difference in difficulty as between types of organization, when the test series as a whole is considered. How real this difference is appears in Table III which shows for all the different types the difference in the median sigma values, the P.E. of the differences, and the ratio of the difference to the P.E.

Conclusions that seem justifiable from these findings are: the whole (*W*) response is not more valuable as indicator of organizing activity than responses in which only details are combined. In fact it is less so. But there is no significant difference between the mean sigma value for this response and that for combining adjacent details (*C*). Organization of adjacent details would therefore seem to have the same value as indicator of the associative process as organization of all elements of the material into a whole. The organization of distant details (*A*) and that of white with solid material (*B*) are seen to have significantly higher sigma values than the whole response, with respective chances of 91 and 92 in 100 of a real difference, but not enough for absolutely reliable indication of a real difference. The synthesis-analysis (*F*) process has a sigma value statistically reliably greater than the whole response. Comparing the other

TABLE III

Comparison of the Various Types of Associative Organization

Types compared	Diff.*	P.E.$_{\text{diff.}}$	D/P.E.$_{\text{diff.}}$
W–A	.640 (*A*)	.289	2.22
W–*B*	.575 (*B*)	.255	2.25
W–C	.085 (C)	.253	.33
W–*F*	1.225 (*F*)	.266	4.61
A–*B*	.065 (*A*)	.169	.38
A–C	.555 (*A*)	.165	3.36
A–*F*	.585 (*F*)	.184	3.17
B–C	.490 (*B*)	.095	5.14
B–*F*	.650 (*F*)	.125	5.20
C–*F*	1.140 (*F*)	.119	9.58

* The letter in parentheses indicates the process having the higher of the two sigma values.

processes it is seen that organization of distant details is reliably of higher sigma value than organizing adjacent elements, the chances being 99 in 100 that the difference is real. But there is no difference in value between organization of distant details and that of white with solid ones. Since the white details are always adjacent to those with which they are combined, this finding speaks for the difficulty of including a white detail into an organized larger whole. *Gestalt* thinking, with its emphasis on figure, ground, and the contrast of one to the other in the larger percept, would seem to get some support from this finding. This greater difficulty in organizing a white space with an adjacent solid element is clearly brought out in the fully reliable difference between the sigma values for this process, and that of combining adjacent solid material (the *B* and the *C* processes). Finally, it is clear that the analysis-synthesis process is by far the most difficult. Statistically absolute certainty is found that its sigma value is higher than the organization of adjacent details; organization into wholes; organization of white spaces with other details. As compared with organization of distant details, the difference is not absolutely reliable, but the chances are high, almost 98 in 100, of a real difference.

Light on the respective processes is thrown also by the scatter shown in the 10 figures of the test series, for each kind of organization process. The whole response, median 1.12, P.E. .6175 shows a very wide scatter. The ratio of the median to its P.E. is only 1.81. The great dissimilarity of the findings themselves (Table I) reflect the same wide scatter. Production of whole responses is seen thus to be a very variable process. With change of conditions, as represented by the different test figures, the sigma value of the whole response changes. Not so much variation is found in respect to any of the other processes. This appears from the individual sigma values in the respective figures; from the medians; and more especially from the ratio of the medians to their P.E. These values are given in Table IV.

TABLE IV

RATIOS OF THE MEDIANS OF THE DIFFERENT TYPES OF ASSOCIATIVE ORGANIZATION TO THEIR P.E.

Type of organization	$M/P.E._{dis}$
A	4.54
B	9.28
C	7.65
F	9.15

Very narrow scatters are thus indicated, especially as compared with that for whole responses. It seems justifiable to conclude that in so far as the group in the experiment employed the organizing processes under consideration, the varying conditions as represented by the different test figures did not have great effect on the sigma value of the processes. The difficulty of producing processes other than wholes seems, in other words, to remain fairly constant, irrespective of conditions. The findings are graphically portrayed in Fig. 5. The *A, B, C,* and *F* curves follow the form of a straight line much more closely than that for *W*.

Frequent reference has been made in this report to the "conditions" under which the processes were elicited, *i.e.* the figures in the test series forming the stimuli in the experiment. In so far as these can be described in terms of configuration, shading, color, what information do they give as to the relation between these conditions and the sigma values of the various organizing processes observed?

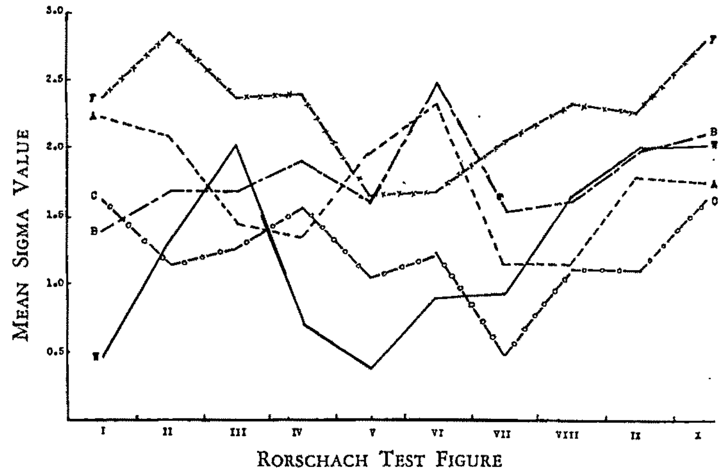

FIG. 5. MEAN SIGMA VALUE FOR EVERY ONE OF THE RORSCHACH TEST FIGURES FOR EVERY TYPE OF ASSOCIATIVE ORGANIZATION

A precise description of the test figures in terms of every-day meaning is *ex hypothesi* impossible, since the figures start by being meaningless. They are ink-blots, whose form is left to chance, except that all are symmetrical, a characteristic effected by folding in the center the paper on which the figures were originally made.[5] The outer outlines of each half of the test figure are irregular. The amount of white space in each is irregular as compared with the others. The appearance they present to the eye, with reference to their solidity, shading, and color is about as follows (Roman numerals refer to the ordinal position of the figure in the test series):

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[5] For a more detailed description of the technique, see Rorschach, *op. cit.*, 4, 10 ff.

Solidity:	
Definitely solid	I, IV, V, VI.
Discrete elements interconnected	II, VII.
Definitely broken up	III, VIII, IX, X.
Color:	
All black	I, IV, V, VI, VII.
Principally black, with red details, relatively of small amount	II, III.
All colored	VIII, IX.
Principally colored, with some black of relatively very small amount	X.
Shading:	
Shading differential marked	IV, VI.
Shading differential present but slight	I, II, III, V, VII, X.
No obvious shading differential other than color	VIII, IX.
White Space within figure:	
Prominent	I, II, III, VII, VIII, X.
Present in small amount	IV, IX.
None	V, VI.

Considering now the quantitative findings in the light of these descriptions, it seems justifiable to conclude that material is more easily organized into meaningful wholes the less broken up it is. Test figures I, IV, V, solid, have the lowest sigma values in producing whole responses. VII, has almost the same sigma value as VI, which, also solid, is fourth in order. The sixth in order of *W* sigma value, test figure II, is like VII in that it consists of discrete elements interconnected. Thus interconnected discrete material stands next to solid material in respect to ease of organization into a whole. Both of these findings are as would be logically expected. Another constant factor is, however, the presence of color in the 5 test figures having the highest sigma value. There is a perceptible break, too, between VII, fifth in respect to the sigma value of its *W*, 0.92; and figure II, sixth in respect to its *W* sigma value, 1.32. The question presents itself, does presence of color, even when in the relatively small amount found in II and III, hamper organization into wholes?

This is as far as the descriptive aspect of the test figures shows any definite relation to the quantitative findings. In respect to the other processes, no unequivocal relationship is perceptible. In organizing distant details, for example, the highest sigma values are obtained in I, II, and VI, which may be considered as expected: material easily reacted to as wholes fails to stimulate into seeking non-obvious relationships. But on that reasoning VII and IV should also show high sigma values for this process. They are nevertheless lowest and third lowest respectively. Whether, as above remarked, the amount of *A, B, C,* and *F* process is likely to remain constant, irrespective of conditions, or whether this lack of differentiation is due to uncontrolled conditions in the test series, this writer is not prepared to say.

Uncontrolled conditions do exist, however, and until experimental procedure should show them to be of no or little weight in influencing the findings, it is obviously premature to conclude that irrespective of conditions, a group such as is represented by those participating in the present experiment, will tend to combine distant details in about the same amount, to combine white and filled spaces in about the same amount, combine adjacent details in the same amount, and analyze-synthesize in the same amount.

The uncontrolled elements in this experiment are, principally, color, and ordinal position of the figures. Thus the highest sigma values for whole reactions are found in II, VIII, IX, X, and III. Color composes VIII, IX, and practically all of X, and enters into, although in only a relatively small degree in II and III. Is color an element interfering with organization into a whole? Is the position of VIII, IX, and X, towards the end of the series, a factor? Shading and amount of white space within the figure are also uncontrolled in this experiment. To what extent does contour, within a figure, produced by differential shading, promote response to details and to that extent militate against organization into wholes? If white space is important in breaking up a figure and so making difficult the organization into wholes, why the high sigma value for *W* in test figure IX, with its relatively small white space? Again, is it because of the colors, facilitating separation and militating against organized interpretation? or is it ordinal position, with possible fatigue of the subject?

These are among the outstanding questions that present themselves, and that cannot be answered until checked by control experiment.

SUMMARY

The Rorschach test, an ink-blot series, was administered to 39 individuals of very superior intelligence. Their responses were studied with reference to the evidence of organization of details of the test figures into larger meaningful combinations.

(1) The principal types of organization found were of (a) wholes, (b) distant details, (c) white spaces with solid material, (d) adjacent details, and (e) analysis-synthesis.

(2) Sigma values for each organization process in each figure of the test series were obtained. These were found to vary for each process in each figure. The analysis-synthesis process was found having a sigma value reliably higher than all the other processes except the organization of distant details; and significantly higher than organization of distant details. A significant and practically reliable difference in favor of the organization of distant details is found as against combination of adjacent details; a significant difference is also found in the sigma value of the organization of distant details as against wholes. The organization of white spaces with adjacent solid ones also has a sigma value significantly higher than combination of adjacent solid material, and organization into wholes. The median sigma value of the wholes does not significantly differ from that for organization of adjacent solid details into meaningful relationship. The sigma values for wholes in the respective figures varies sufficiently to assign different values for whole responses in each figure, when the test is used diagnostically. Organization into wholes was found definitely to vary with change in the conditions as represented by the figures in the test series. The other organization processes did not show so much variation. Their sigma values remain fairly constant for all figures in the series. Unchecked variables in the experiment are, principally, color and ordinal position of the test cards.

PSYCHOLOGICAL SIGNIFICANCE OF CEREBRAL BIRTH LESIONS

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Human birth is attended by many dangers, one of which is the possibility that serious damage may be done to the intracranial portion of the central nervous system. This damage may be produced during the period of labor or it may be the result of abnormalities occurring during the later stages of pregnancy. Since the effects of very early postnatal cerebral pathology are much the same as those incurred prenatally or during labor, it is customary to consider intracranial birth lesions as including all those which develop within approximately one or two months before or after birth, as well as those occurring during delivery.

It is impracticable to discuss here the pathology or the etiology of cerebral birth lesions. In this paper we are more immediately concerned with the significance of such lesions in relation to mental development.[1] It is impracticable even to discuss the details of these effects, except to note that birth lesions may produce (1) motor impairment, (2) retardation of intelligence, (3) disturbance of personality and conduct, with (4) consequent handicaps in learning. These various aspects of behavior may be involved singly or in combination, and the effects may be mild or severe. Impairment in central sensory mechanisms is a presumptive further consequence, but it has received comparatively less attention.

The motor difficulties which follow from cerebral birth lesions constitute the most conspicuous feature of the condition. These signs also provide the principal basis for clinical diagnosis. Among these, spasticity and athetosis are the most important.

For practical purposes, spasticity is defined as essentially simultaneous contraction of reciprocal muscle groups, with hypertonicity. Athetosis may be described as successive involuntary movements superimposed upon fairly good voluntary coördination. These two types of motor difficulty are seldom seen alone, and are often combined with overflow (synkinesia), ataxia (incoördination), or tremors.[2] The motor difficulties may be due to dis-

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[1] For discussions of pathology and etiology see E. A. Doll, W. M. Phelps, and R. T. Melcher, *Mental Deficiency Due to Birth Injuries,* 1932; and H. Ehrenfest, *Birth Injuries of the Child,* 2nd ed., 1931.

[2] W. M. Phelps, Cerebral birth injuries, their orthopedic classification and subsequent treatment, *J. Bone & Joint Surg.,* 14, 1932, 773-782.

turbances in tone, timing, reciprocation, power, or steadiness, and the involvement may affect the trunk or extremities separately or in combination.[3] Speech may or may not be involved.

These motor disturbances have important bearing on the neuromuscular background of behavioral development. Both maturation and learning are involved, because the lesion presumably interferes with the growth of neurological elements and with the development of expressive behavior. It is evident that the motor handicaps tend to lessen with age. This suggests delayed growth, repair, or substitution of central neural elements. Improvement takes place also under physical therapy, and the mechanism of such improvement is an important problem of learning.

It is evident that the site of the lesion or its extent may have an important bearing on the temporal and directional phases of neuromuscular development. In our subjects the characteristic developmental motor sequences of normal infancy and childhood are followed as an average tendency, although these developments are grossly delayed.

The development of speech in these subjects is of special importance, not only with reference to cerebral localization but also with respect to the motor basis of ideational development. Neuromuscular defects of speech are present in practically all of our mentally subnormal subjects, but among the mentally normal birth-injured, speech defects are apparently less frequent. However, serious impairment of speech, as such, seems to have relatively little influence, among the birth-injured, on the development of intelligence as expressed in comprehension and ideational response. Our subjects as a group show no significant correlation between speech-impairment and intellectual impairment.

The question arises whether retardation in motor development has a permanent effect upon the degree or rate of such development when it does take place. Whether there are critical genetic periods for the expressive development of particular motor or intellectual functions is of some interest here. If so, does the inhibition of such expression at the normal period of maturation interfere with the rate or degree of subsequent acquisition of such functions? From our results, we cannot offer a definite answer, but apparently the ultimate level of behavioral development cannot safely be predicted from a given point of temporal retardation.

Impairment of intelligence is one of the important consequences of cerebral birth lesion.[4] This is not, however, a necessary consequence, since many subjects show no impairment of intelligence except indirectly in the expressive handicaps of speech and movement. The level of intelligence ranges all the way from absolute idiocy to superior adult levels.

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[3] *Idem.*, Dyskinesia, *Yale J. Biol. & Med.*, 3, 1930, 39-48.

[4] Doll, Phelps, and Melcher, *op. cit.*

From 6 to 10% of all of our mentally deficient subjects at Vineland have motor handicaps due to cerebral birth lesions. The percentage of persons with birth lesions in the mentally normal population is not known, but is probably not more than 1/10%.[5] Assuming that as many as 2/10% of the general population are birth-injured (which is probably a high figure), and assuming that only 6% of the feeble-minded are birth-injured (which is probably a low figure), then birth injury would be 30 times as frequent among the feeble-minded, in general, as among the non-feeble-minded, in general.

It is generally assumed that about 2% of all children of school age are mentally deficient. Apparently at least 40% of all birth-injured children of school age are mentally deficient.[6] From which it would seem that mental subnormality is perhaps 20 times as frequent among birth-injured children as among physically normal children.

As noted above, intelligence among the birth-injured ranges from complete idiocy to superior adult levels. What is not so clear is the relation of the site and extent of cerebral lesions to intellectual development. This problem is complicated by the relation of speech to intelligence and the uncertainties of cerebral localization with respect to speech, and it is further complicated by the relation of handedness to speech and to intelligence, in terms of cerebral localization. One can only surmise to what extent a cortical hemorrhage might spread along the brain surface or within its depths and across its lobes. The final answer to these questions will probably be learned from post-mortem examinations.

In the meantime, it is apparent that there is no significant correlation between the level of intelligence and the severity of the motor impairment. Preliminary results suggest, however, that our spastic subjects show somewhat more mental impairment than do our athetoid subjects, and this result may possibly be related to the cortical damage among spastics as contrasted with the presumptive damage in the basal nuclei among athetoids.

The problem of intelligence in these cases is further complicated by the difficulty of measuring intelligence in the presence of expressive handicaps of speech and movement. Skill, patience, tact and experience are necessary if one is to avoid underestimating or overestimating the intellectual level among subjects with motor handicaps. Where the motor impairment does not involve speech, or is confined to movements which do not involve manipulative performance, there is, of course, no serious difficulty of mental measurement. In many cases one is immediately impressed with the obvious normality of intelligence in subjects where speech and movement are almost totally absent, although this impression cannot easily be sustained by current methods of objective examination.

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[5] This estimate is based on general reports that seven children per thousand are cripples, supplemented by the fact that at the Spalding School (Chicago) one cripple in seven is a congenital paralytic. The estimate is only a tentative working figure.

[6] One-third of the congenital paralytics at the Spalding School are reported mentally subnormal. G. B. Smith reports three-fourths of infantile cerebral accidents mentally subnormal.

The relation of handedness to intelligence becomes, under these circumstances, a matter of special importance, but one of more difficulty than at first appears. For example, in hemiplegia it sometimes appears that the affected side is, nevertheless, the preferred side in manipulative performance. But if the opposite side is preferred, one cannot be sure whether this is a native preference, possibly inherited, or one acquired through necessity.

Left-handedness seems to be somewhat more common among the feebleminded than among the mentally normal, but this is not obvious from casual observation. At any rate, the number is certainly not so great as to favor the thesis, advanced by some brain surgeons, that the neurological seat of the intelligence is in the left frontal lobe, or even in the left hemisphere. On the other hand, it seems quite apparent that left-handedness is very much more common among our birth-injured mentally deficient subjects than among our feeble-minded group in general, but this fact has not yet been correlated with the localization of the motor damage. It is clear that further study of these subjects should shed important light on the thesis of unilateral localization of intelligence. This information can then be correlated with that gained from cerebral hemisection and other surgical procedures in cases of brain pathology.

The development of speech in these cases is another field of special interest because of the relation of language to thought and the impairment in language development in subjects with grave motor handicaps of speech. We have seen obviously normal intelligence in young children with almost total speech handicap, and obviously superior intelligence in adults who have never developed intelligible speech.

Mental growth is of particular interest in these subjects, because of its unusual character, being seriously delayed or irregular in many cases, but apparently unimpaired in others. A study of our group as a whole shows a decided tendency for the intelligence to improve with age, not only absolutely, but also relatively. Increasing development in the later years and continuing beyond the usual age-limits is particularly noteworthy.

Thus all but one of twelve cases studied for fairly long periods of time showed unusual mental growth not characteristic of the ordinary feebleminded.[7] Individual cases are especially impressive. Thus, one child with a Binet mental age of 4 at life-age 8 (I.Q. 50), gained four years in mental age during the next four years, a rate of growth which was twice the I.Q. prediction. Another child, with a Binet mental age of 6 at life-age 12 (I.Q. 50), made four years' mental growth in the next four years. A third case, with I.Q. 100 at life-age 5, made no appreciable mental development during the next three years, but during the following five years increased five years in mental age.

These particular growth records are independent of the difficulties of mental examination and there is no evidence that anything unusual occurred in the treatment or training. Instances of remarkable functional improvement late in life apparently capitalizing a continuing maturation of intelligence not previously expressed in behavior are not unusual. The remarkable extent to which intelligence, if present, can surmount the motor handicaps of speech and movement is another of the notable features in these subjects.

As previously noted, disturbances of personality and disorders of conduct are among the consequences of birth lesions.[8] It is not clear whether these disturbances are the direct consequences of structural or physiological involvements, or whether they are the indirect result of the frustration in behavior imposed by the motor handicaps. It appears, however, that personality or disposition may be affected independently of intelligence and movement.

In view of the uncertainties of the constitutional bases of conduct and personality, this problem is even more difficult of interpretation than that of intelligence. Some authorities suggest cortical localization of personality and this suggestion may ultimately receive some support from the study of the birth-injured.[9]

In our subjects, it appears that the sympathetic nervous system is often involved, with consequent visceral disturbances which could produce grave disturbances of mood or behavior. According to present evidence, it seems unlikely that the glands of internal secretion are disturbed by cerebral birth lesions, although this is a problem which merits further attention. It has been known for some time that visceral disturbances can produce focal infections which affect cerebral function.

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[7] Doll, Phelps, and Melcher, *op. cit.*

[8] P. L. Schroeder, Behavior difficulties in children associated with the results of birth trauma, *J. Amer. Med. Assn.*, 92, 1929, 100-104.

[9] R. J. A. Berry, Cerebral cortical structure and its relation to mental disease, *Brit. Med. J.*, 1, 1931, 837-839.

Our own observations in the direction of personality and conduct are too limited to warrant much emphasis. At the present time, these observations are clinical rather than statistical. They support the general assumption of dispositional involvement. These involvements are readily observed in the mentally low-grade patients, where excitability, irritability, distractibility, hyperactivity, apprehensiveness, and the like, are common traits. In our mentally high-grade cases we are more impressed with the peculiarities of attitude as reflected in poor insight, poor social judgment, obtrusiveness, and egocentricity. Favorable involvement of personality is reflected in a conspicuous persistence and concentration of effort present in some cases, even in the face of distractibility and irritation provoked by continuous frustration of voluntary effort.

SUMMARY OF DATA

Thirty-nine cases of mental deficiency, about 9% in an institutional population of 440, have been classified as associated with birth lesions. Ten of these are without gross motor signs; the remainder show various types and degrees of paralysis. They have been grouped as *definite* (12), *probable* (15), and *possible* cases (12). This classification is based on the relative importance of various aspects of the natal and developmental histories.[10] There is a strong presumption that in both the motor and non-motor groups the birth lesion is a probable cause of the mental deficiency as well as of the motor handicaps.

Motor group. In the group with motor signs, 83% are found to have positive birth histories, 65% have positive neonatal histories, 83% have positive genetic motor developmental histories, 83% have negative other pathology, 90% have negative heredity (for both motor and mental status), and 100% have positive symptoms for present status.

As to the type of involvement in the motor group, 14 subjects are classed as spastic, 9 as athetoid, 1 as mixed, 2 as intention-tremor, and 3 as undifferentiated. Of the spastics, 4 are paraplegias, 6 are quadriplegias, 1 is a right hemiplegia, 2 are left hemiplegias, and 1 is a triplegia.

Broadly speaking, the spasticity is of cortical pyramidal origin, while the athetosis is of basal ganglion origin.

Of the subjects with motor signs, 83% are males. The median life age is 16 years, the median mental age 6.8 years, and the median I.Q. 55. These distributions correspond very closely to the institutional group as a whole.

Of the subjects with motor signs, 66% are first-born, 24% are premature births, and 41% are left-handed. In the total institutional group, 22% are first-born, no instances of prematurity are found in a control sample, and 12% of a control sample are left-handed.

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[10] E. A. Doll, Birth lesion as a category of mental deficiency, *Amer. J. Orthopsychiat.*, 3, 1933, 1-13.

STATISTICAL DATA ON BIRTH-INJURED, MENTALLY DEFICIENT SUBJECTS

	No. cases	%	Med. L.A.	Med. M.A.	Med. I.Q.	(%) Male	(%) First-born	(%) Premature	(%) Left-handed
Spastics	14	36	19	7.0	51	93	72	21	36
Athetoids	9	23	15	6.7	61	56	44	33	44
Other motor	6	15	15	6.5	60	100	83	17	50
Total motor	29	74	16	6.8	55	83	66	24	41
Non-motor	10	26	16	4.0	29	90	80	30	10
All B-I cases	39	9	16	6.7	51	85	69	26	33
Institution	440	—	18	6.9	54	62	22*	0*	12*

* Sampling

	No. cases	Positive Clinical Status	Positive Birth History	Positive Neonatal Condition	Positive Motor History	Negative Other Pathology	Neg. Heredity
				(Percentages)			
Motor cases							
Definite	12	100	92	83	92	100	91
Probable	13	100	77	54	77	77	85
Possible	4	100	75	50	75	50	100
Total motor	29	100	83	65	83	83	90
Non-motor	10	100	90	90	90	80	100
All B-I cases	39	100	85	72	85	82	92

Correlations

	R	r		P.E.r	
Mental age—life age	.34	.51		.09	
Mental age—motor facility	−.05	−.07		.13	
(same, corrected for life age)			.06		.13
Mental age—speech facility	−.01	−.01		.13	
(same, corrected for life age)			.10		.12
Mental age—genetic score	.53	.74		.06	
(same, corrected for life age)			.67		.07
Life age—motor facility	−.16	−.25		.12	
(same, corrected for mental age)			−.24		.12
Life age—speech facility	−.11	−.17		.12	
(same, corrected for mental age)			−.20		.12
Life age—genetic score	.32	.48		.10	
(same, corrected for mental age)			.17		.12
Motor facility—speech facility	.20	.31		.11	
(same, corrected for M.A. and L.A.)			.27		.12
Motor facility—genetic score	.07	.11		.12	
(same, corrected for M.A. and L.A.)			.29		.12
Locomotion—manipulation	.38	.56		.09	
(same, less 4 paraplegias)	.86		.98		.01

Non-motor group. Ten subjects have been classified in the birth-lesion group without gross motor signs. Nine of these have positive birth histories, positive neonatal histories, and positive genetic motor retardation. Eight have negative other pathology. All have negative heredity. Two are classed as probable, and 8 as possible birth lesions. One is female and 9 are males. The median I.Q. is 29, (life age 16, mental age 4.0). Eight are first-born; 3 are premature; and 1 is left-handed.

Spasticity vs. Athetosis. Nine athetoids in the group of 29 cases with motor signs have a median I.Q. of 61 (life age 15, mental age 6.7). Fourteen spastics in this group have a median I.Q. of 51 (life age 19, mental age 7.0), a fact which may bear some relation to the presumptive cortical basis of spasticity as contrasted with the basal-ganglion defect in athetosis. Of the athetoids, 44% are left-handed, and of the spastics, 36%. Since athetosis represents principally an involvement of the basal ganglia, this fact suggests that handedness may be related to subcortical as well as to cortical localization.

Three of the 14 spastics (20%) are hemiplegias. Two of these are left hemiplegias, and 1 is right. (Sachs and Peterson, in a group of 156 hemiplegias, found 52% right-sided, and 48% left-sided.)[11] The I.Q. of the right-sided hemiplegia is 49, the average I.Q. of the two left-sided hemiplegias is 62. Among 6 spastic quadriplegias, 3 are left-handed, 2 are right-handed, and 1 is ambidextrous.

Handedness. Twelve of the 29 motor cases (41%) are left-handed, 15 cases (50%) are right-handed, and the remainder ambidextrous. (This may be compared with the figures for hemiplegias of Sachs and Peterson—52% and 48%—suggesting that birth lesions are not unilaterally selective.) The median I.Q. of the left-handed group is 50 (life age 15, mental age 5.6), while for the right-handed group it is 53 (life age 17, mental age 7.4). This slight difference is due to a slightly lower percentage of spastics in the left-handed group as compared with the right-handed group (42% and 53%), the athetoids being equal numerically in the two groups.

In a control group of 33 subjects, alphabetically selected, 4 subjects (12%) were left-handed, 26 subjects (80%) right-handed, and the remainder ambidextrous. The median I.Q. of the left-handed was 60, and of the right-handed 59. (Thus left-handedness is four times as frequent in the birth-lesion group as in the institutional group, and is about twice as great for the latter as compared with the general population.) In the non-motor group, one of the 10 cases is reported left-handed.

Correlations. In order to obtain an idea of the trend of relationship of the motor handicaps of these subjects to age and mental age, the 29 subjects with motor signs were ranked according to facility in locomotion, manipulation and speech, and these were correlated with life age, mental age, and with each other. These ranks were based on the composite judgment of several observers. The correlations are not very dependable because of the subjective nature of the ranks and the small number of cases. The coefficients were obtained by the Pearson footrule formula $R = 6\Sigma g/n^2$, and these values were converted into the Pearson r. The latter coefficients were then corrected for related variables.

The correlation between mental age and life age is $r = 0.51$, P.E. 0.09. The correlation between mental age and motor facility (locomotion and manipulation combined), corrected for life age, is $r = 0.06$, P.E. 0.13. Between mental age and speech facility, corrected for life age, the coefficient is $r = 0.10$, P.E. 0.12. The correlation between life age and motor facility, corrected for mental age, is $r = -0.24$, P.E. 0.12. Between life age and speech facility, corrected for mental age, the coefficient is $r = -0.20$, P.E. 0.12. The correlation between motor facility and speech facility, corrected for both mental age and life age, is $r = 0.27$, P.E. 0.12. The correlation between facility of locomotion and facility of manipulation is $r = 0.56$; this coefficient is increased to $r = 0.98$ when four paraplegias without handicap of manipulation are eliminated.

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[11] B. Sachs and F. Peterson, A study of cerebral palsies of early life based upon an analysis of 140 cases, *J. Neur. & Ment. Dis.,* 17, 1890, 295-332.

The correlation between facility in locomotion and manipulation is positive, high, and reliable, revealing a marked tendency toward generalized motor handicap except in the case of paraplegias. The coefficients of correlation between motor facility and speech facility and each of these with life age and mental age tend toward an absence of correlation. From clinical observation, one would expect motor handicap and speech handicap to be positively correlated with each other and negatively correlated with both age and mental age in a fairly marked degree.

Genetic development. An attempt was made to study the genetic behavioral development of the 29 subjects with motor lesions in terms of general functional ability. The behavioral status of these 29 subjects, as obtained from a tentative scale of functional development, shows about a 4-year level of performance. This is about 3 years below the median Binet mental age of the group. The correlation between this genetic score and Binet mental age, corrected for life age, is $r = 0.67$, P.E. 0.07. The correlation between life age and genetic score, corrected for mental age, is $r = 0.17$, P.E. 0.12. The correlation between genetic score and motor facility, corrected for both mental age and life age, is $r = 0.29$, P.E. 0.12. These coefficients show a definite tendency for the genetic score to be more dependent upon mental age than upon either life age or motor facility.

The psychological study of children with birth lesions should be especially important in relation to current theories of learning. The neuromuscular handicaps in the motor cases, together with the involvement of intelligence and personality in other cases, afford an unusually fertile field for special study. The organization of behavior in these children, following as it does the general motor sequences of normal infancy, but at very much retarded periods or rates, is of value, not only for the slow-motion study of behavioral development, but also for its neurological relations. The neuromuscular theory of learning, the maturation of learning, the linguistic correlates of learning, the control of voluntary and involuntary movements through growth and muscle-training, are problems which are immediately brought into relief in the study of the birth-injured.

SPATIAL PERCEPTION IN THE REGION OF THE OPTIC DISK

By FREDERICK C. THORNE, Columbia University, and
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The blind spot has, since its discovery by Mariotte in 1668,[1] been the subject of many investigations and the focal point of numerous controversies.[2] Many of the questions thus raised during the past 265 years are still undecided, not because they have been neglected, but because, on the contrary, so many different answers have been returned that generalization is difficult if not impossible. One of these questions—the specific problem of this study—concerns the spatial values of the retina in the region of the optic disk. Numerous studies have been made of this problem, and we venture to add one more to the list because the results and statements of the different investigators have been widely at variance.

Logically, there seem to be three possible answers to our question: the spatial values of the retina in the region of the optic disk might (*1*) be distorted by way of contraction, (*2*) be distorted by way of expansion, or (*3*) be unchanged except for the distortion characteristic of peripheral vision. Not only have all these possibilities received experimental validation, but, strangely enough, this gamut has been exceeded. Some investigators have found (*4*) that the spatial values are equivocal, decreasing or increasing as conditions are variously altered. The experiments leading to these very different conclusions may be grouped as follows.

(1) Contraction or shrinkage. The question regarding the spatial values of the retina in the region of the optic disk was first raised by Volkmann in 1846.[3] To demonstrate his theory of local signs, Volkmann cited the fact that a figure subtending the blind spot appeared very much shortened. Though he ignored, or at least overlooked, this observation when he came, seven years later, to different conclusions respecting the spatial values of the region of the optic disk, other experimenters (Von Wittich,[4] Funke,[5] Bull,[6] Brückner,[7] Ferree and Rand,[8] Werner,[9]

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* Accepted for publication May 30, 1931. From the Psychological Laboratory of Columbia University.

[1] E. Mariotte, *Mém. de l'acad. de Paris, trans. phil.*, 3, 1668, 668.

[2] It has also beeen a continual source of surprise and amusement to the laity. The "merry monarch" Charles II of England (1630-1685) is said to have used it in jest to show his courtiers "how they would look when their heads were off their shoulders" (Helmholtz, *Physiological Optics*, 2, 1925, 29), and students and others today evince amusement as well as surprise when it is first brought to their attention.

[3] A. W. Volkmann, Sehen, in Wagner's *Handwörterbuch der Physiologie*, 3, 1846, 265-351.

[4] W. H. von Wittich, Studien über den blinden Fleck, *Arch f. Ophth.*, 9, 1863, 1-31.

Lohmann,[10] Hofmann,[11] and Helson[12]) have brought forward evidence of contraction or shrinkage.

(2) Expansion or magnification. The phenomenon of expansion has been reported in only one study, that by Ferree and Rand. In addition to noting the fact that "complete shrinkage takes place when the stimuli observed are on the edge of the blind spot,"[13] they also observed that "there is a rapid falling off of the influence of the blind spot upon spatial values as the stimuli recede from its edges,"[14] until finally a point is reached where there is no distortion. This phenomenon, they explain, is due to the fact that "the shrinkage in the visual field produced by the drawing together of [the edges of the blind spot] is compensated for by a magnification of spatial values in the region immediately surrounding."[15]

(3) No change. Despite Volkmann's priority, as documented above,[16] Weber is usually credited with having made, in 1852, the first observations on the spatial values of the region of the optic disk.[17] He found no changes other than those characteristic of peripheral vision in general. The following year (1853) he,[18] Volkmann,[19] and Fick and DuBois-Reymond,[20] working independently, corroborated this finding, and since then—despite reports by careful workers to the contrary—the weight of opinion and the preponderance of experimental evidence has been against deformation. In addition to the studies reported in 1853, the following have confirmed Weber: Aubert,[21] Woinow,[22] Wundt,[23] Helmholtz,[24] Titchener,[25] Landolt,[26] Brückner,[27] Nussbaum,[28] Feinberg,[29] Bard,[30] and Hartmann.[31]

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[5] O. Funke, Zur Lehre vom blinden Fleck, *Ber. d. naturf. Gesell, z. Freiburg i. Br.,* 3, 1864, 12 f.

[6] O. Bull. *Perimetrie,* 1895, 8.

[7] A. Brückner, Über die Sichtbarkeit des blinden Fleckes, *Arch. f. d. ges. Physiol.,* 136, 1910, 610-657.

[8] C. E. Ferree and G. Rand, The spatial values of the visual field immediately surrounding the blind spot and the question of the associative filling in of the blind spot, *Amer. J. Physiol.,* 29, 1912, 403 f.

[9] H. Werner, Untersuchungen über den blinden Fleck, *Arch. f. d. ges. Physiol.,* 153, 1913, 475-490.

[10] W. Lohmann, Der blinde Fleck in seinen Beziehungen zu den Raumwerten der Netzhaut, *Arch f. Augenhk.,* 81, 1916, 181-196.

[11] F. B. Hofmann, *Die Lehre vom Raumsinn des Auges,* 1, 1920, 190-198.

[12] H. Helson, the effects of direct stimulation of the blind-spot, this JOURNAL, 41, 1929, 345-397. An excellent summary of the literature on the blind spot is given in this study.

[13] Ferree and Rand, *op. cit.,* 403.

[14] *Ibid.,* 405.

[15] *Ibid.,* 406.

[16] Volkmann, *op. cit.*

[17] E. H. Weber, Über den Raumsinn und die Empfindungskreise in der Haut und im Auge, *Ber. d. k. Sächs Ges. d. Wiss.,* 4, 1852, 185.

[18] Weber, Über Grösse, Lage und Gestalt des sogenannten blinden Flecks im Auge und die davon abhängigen Erscheinungen, *ibid.,* 5, 1853, 149-158.

[19] A. W. Volkmann, Über einige Gesichtsphänomene, welche mit dem Vorhandensein eines unempfindlichen Fleckes im Auge zusammenhängen, *ibid.,* 5, 1853, 27-50.

[20] A. Fick and P. DuBois-Reymond, Über die unempfindliche Stelle der Netzhaut im menschlichen Auge, *Arch. f. Anat., Physiol., u. wiss. Med.,* 1853, 396-407.

[21] H. Aubert, Über den blinden Fleck, *Jahresber. d. Schles. Gesell.,* 1854, 25-28.

[22] A. Woinow, Über das Sehen mit dem blinden Fleck und seiner Umgebung, *Arch. f. Ophth.,* 15, 1869, (pt. 2), 155-166.

(4) Equivocal. Some investigators, again, hold the opinion that the spatial values in the region of the optic disk are equivocal. This view was early advanced by Funke. He found, in 1864, that these spatial values varied sometimes in one direction and sometimes in another, depending upon the conditions of the experiment.[32] In the following year, 1865, Aubert wrote, "although I have had great experience in indirect division, . . . I am bound to state publicly that I am not able to reach any decision as to how the field of view is completed at this point" [*i.e.* at the blind spot].[33]

The claim of equivocality was not advanced again until 1912, when Ferree and Rand reported an experiment showing that the spatial values varied specifically with the areas stimulated.[34] Some areas showed 'shrinkage,' some 'expansion,' and some no change at all. Though their results would seem to reconcile the discrepancies of the earlier studies, they have never been confirmed. They not only stand alone, but they have been met with the charge of being artifacts. Nussbaum claims, and his criticisms have never been refuted, that Ferree and Rand's results are due to faulty fixation and involuntary movements of the eyes.[35]

PROBLEM

This diversity of the results is a challenge for further work. The problem is specific. The different findings of the earlier investigators cannot, however, be resolved through a repetition of the usual procedures. All that the old procedures can possibly yield are results aligning the experimenter with one or another of the groups mentioned above. A new approach must be made. Many experimenters have compared spatial perception in the region of the optic disk with perception in neighboring or symmetrically

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[23] W. Wundt, *Grundzüge der physiologischen Psychologie,* 2, 1893, 104; 2, 1902, 510.

[24] H. von Helmholtz, *Handbuch der physiologischen Optik,* 1896, 717-727. Eng. trans. 3rd ed., 3, 1925, 210.

[25] E. B. Titchener, *Experimental Psychology,* 1 (pt. ii), 1901, 28 f. Titchener writes, "Although . . . shrinking has in fact been reported by some observers, it seems to be due rather to a 'suggestion' from the blind spot itself than to actual observation. . . . The blind spot, then, has the same spatial value as the surrounding portion of the retina."

[26] E. Landolt, Beobachtungen über die Wahrnehmbarkeit des blinden Fleckes, *Arch. f. Augenhk.,* 55, 1906, 108-118.

[27] A Brückner, Über die Sichtbarkeit des blinden Fleckes, *Arch. f. d. ges. Physiol.,* 136, 1913, 610-657.

[28] F. Nussbaum, Über die Raumwerte in der Umgebung des blinden Flecks, *Arch. f. Augenhk.,* 87, 1921, 142-151.

[29] N. Feinberg, Experimentelle Untersuchungen über die Wahrnehmung im Gebiet des blinden Flecks, *Psychol. Forsch.,* 7, 1926, 22 ff.

[30] L. Bard, Le problème de la tache aveugle et son importance en physiologie générale, *Arch. d'ophth.,* 44, 1927, 5-13.

[31] E. Hartmann, Le problème de l'effacement de la tache aveugle dans le champ visuel, *Ann. d'ocul.,* 164, 1927, 329-336.

[32] Funke, *op. cit.*

[33] H. Aubert, *Physiologie der Netzhaut,* 1865, 257 f. Quoted from the English translation of Helmholtz's *Physiological Optics,* 3, 1925, 207.

[34] *Op. cit.*

[35] Nussbaum, *op. cit.*

located retinal areas, but no one has, as far as we have been able to discover, checked these comparisons with a control experiment upon two retinal areas *both outside the blind spot.* Until such a check is made, no final conclusion can be drawn regarding spatial perception in the region of the optic disk. We report in this study the results of this new attack upon the problem.

METHOD AND PROCEDURE

Two experiments were made. In the first, the Main Experiment, we elicited comparisons of the size of two objects, one falling in the region of the optic disk and the other in a symmetrically located area on the opposite side of the fixation-point (A and B in Fig. 1). In the second, the Control Experiment, both objects were placed symmetrically *above* and on opposite sides of the fixation-point (C and D in Fig. 1).

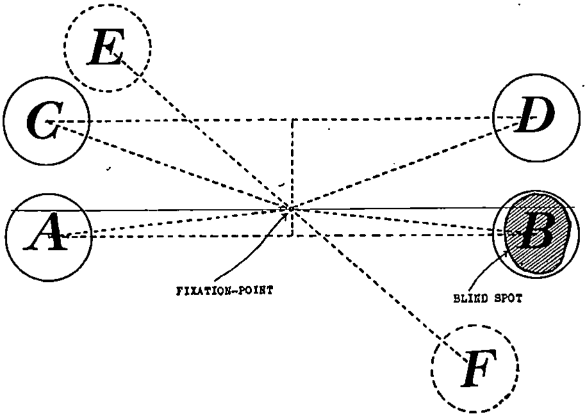

FIG. 1. DIAGRAM SHOWING THE SPATIAL RELATIONS OF THE OBJECTS COMPARED

Disks were suspended at A and B in the Main Experiment, and at C and D in the Control

The distance of the objects from the fixation-point was varied slightly for the different *O*s according to the distance of *O*'s blind spot from the fixation-point. The 'blind spot distance' was used in the Main Experiment, but in the Control Experiment the distance was slightly greater, being the hypotenuse of a right triangle one side of which equaled the 'blind spot distance,' and the other of which was sufficiently large to keep object D (see Fig. 1) outside of the blind spot. The inter-object distance, however, was the same in both experiments. Both distances could not be kept constant without introducing a radical change. If the fixation-distance was held constant, the inter-object distance would be less; and if the inter-object distance was held constant, the fixation-distance would be greater. We could, of course, have kept both of these distances constant by placing the objects, in the Control Experiment, *on a diagonal,* as for example at E and F in Fig. 1; but we hesitated to introduce such a radical difference in the conditions of the experiment as a new orientation of the objects might involve. We decided, then, upon the slight lengthening of the fixation-distance as the least disturbing change.

The objects were presented on a large white campimeter, which extended nearly to the limits of monocular vision. The campimeter was illuminated by two 400-watt daylight globes. *O* was seated 110 cm. from the fixation-point (a ½-cm. black dot), his head held firmly by means of a biting board[36] so adjusted that his right eye (his left being blindfolded) was directly in line with the fixation-point.

TABLE I

SHOWING, FOR EVERY O, SIZE OF STANDARD AND VARIABLE AND PERCENTAGE OF REPORTS 'LARGER' IN EACH SPACE ORDER IN BOTH EXPERIMENTS

			Reports			
			Main Experiment		Control Experiment	
O	Diameter of Standard (in cm.)	Diameter of Variable (in cm.)	1st Order	2nd Order	1st Order	2nd Order
C	24	22	0	2	0	0
		23	9	29	13	16
		24	57	54	52	53
		25	86	78	92	92
		26	100	89	99	100
D	25	23	0	2	4	14
		24	32	14	24	44
		25	62	50	54	54
		26	94	76	80	86
		27	100	88	84	98
H	23	21	0	6	12	18
		22	24	56	36	44
		23	58	98	82	84
		24	90	100	94	96
		25	100	100	100	100
J	25	23	16	14	26	0
		24	34	38	34	18
		25	70	52	72	56
		26	94	80	88	92
		27	100	100	98	94

The blind spot of the right eye of every *O* was plotted centrifugally along 24 radii. Then a standard and 5 variable objects—black disks—were selected for every *O*. The radius of the standard disk was in every case ½-cm. larger than the longest radius of the *O*'s blind spot. The diameters were 24, 25, 23, and 25 cm. for *C, D, H,* and *J* respectively, The diameters of the 5 variables differed by steps of 1 cm., the median equaling the standard (see Table I). The method of constant stimulus differences was used.

Observers. The *O*s were 4 in number: Mr. R. Hamlin (*H*), graduate student in psychology; Mr. W. B. Curtis (*C*) and Mr. H. E. Johnson (*J*), students in medicine; and the senior author (*D*).

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[36] The biting board was a small piece of balsa wood, ¼ in. thick. This wood is soft and readily takes and holds the impressions of *O*'s teeth. Such a biting board is as serviceable as one of wax, and is much more easily made.

Procedure. Preliminary experiments acquainted the *O*s with procedure and task. A standard disk was hung in place, and then a variable, screened from *O* by *E*'s body. At the signal "Ready," *O* tightened his fixation, and at "Now," *E* stepped aside toward the periphery of *O*'s visual field, and thus exposed the second disk.

O was directed to keep his open eye upon the fixation-point and to compare the size of the second disk (the one exposed by *E* when he moved out of the visual field) with that of the first in the terms 'greater,' 'equal,' or 'smaller.' A few reports of 'doubtful' were returned by every *O*, but these comparisons were later repeated without *O*'s knowledge.

Two series, one for each space order, were conducted in both the Main and the Control Experiments. The standard was at the right in the first space order, and at the left in the second. Every *O* gave 100 reports for every one of the 5 variables in each of the two space orders.

RESULTS

The gross results of the study are presented in Table I, which shows the percentage of reports classed as 'larger,' distributed according to experiment, time-order, variable, and *O*. The percentages given in this table include all the reports of 'larger' and one-half of the reports of 'equal.'

The 'limens' or constants, representing the point of subjective equality,[37] are given in Tables II and III and in Fig. 2. Table II gives these values and their measures of precision (h), for every *O* for each space order in both experiments. The third column gives the diameters of the *O*'s standards, and

TABLE II

SHOWING FOR EVERY O, LIMENS AND MEASURES OF PRECISION FOR EACH SPACE ORDER IN BOTH EXPERIMENTS

(Values in cm.)

O	Experiment	Standard	1st space order (Standard at right)		2nd space order (Standard at left)	
			L	h	L	h
C	Main	24	23.9895	.8436	23.9960	.4893
	Control	24	23.9319	.8614	23.8899	.8901
D	Main	25	24.5526	.6835	25.2279	.5577
	Control	25	25.0661	.4693	24.4965	.4915
H	Main	23	22.7458	.6972	22.1202	.7596
	Control	23	22.2529	.6872	22.0501	.6559
J	Main	25	24.3369	.5955	24.7045	.4262
	Control	25	24.4581	.3917	24.8800	.6360

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[37] For other methods of obtaining the point of subjective equality see E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 288 f.

the fourth and sixth columns the diameters of the variables that were required to be equal the standards in size. These data are more clearly shown in Fig. 2, which gives the results for every *O* in their proper spatial relation.

We consider first the results of the Main Experiment. In the first space order, the objects in the region of the optic disk are 0.01, 0.45, 0.25, and

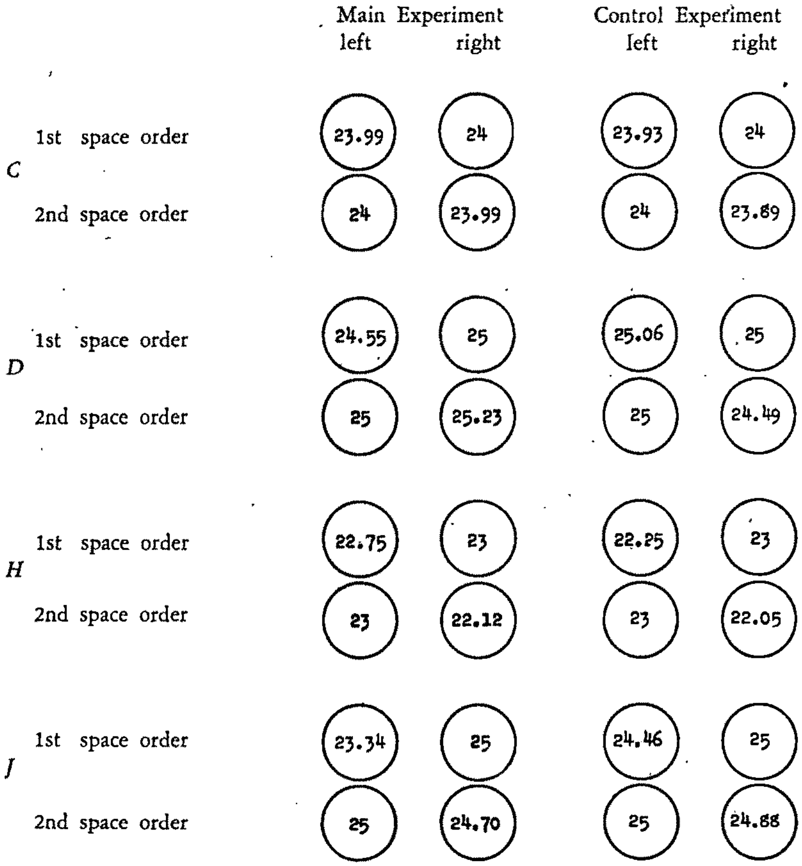

FIG. 2. LIMENS REPRESENTING POINT OF SUBJECTIVE EQUALITY

0.66 cm. *larger* for *C, D, H,* and *J* respectively than those in a symmetrically located retinal area. Not only are these differences small, but they are negated for 3 of our 4 *O*s, by the fact that in the second space order the objects in the region of the optic disks are *smaller* by 0.01, 0.88, and 0.30 cm. for *C, H,* and *J* respectively than the objects with which they were compared. If the results of the first space order suggest contraction, then those of the second space order suggest expansion. Considered together, however, the results indicate *no change* at all. They merely show that these 3 *O*s were predisposed for some reason or other toward the variable. Why, we can only guess—perhaps *E's* movement in exposing the variable directed *O*'s attention upon it—but in any case the results clearly indicate that, for these 3 *O*s at least, spatial perception in the region of the optic disk is not different from perception in a symmetrically located region of the retina. Had we not interchanged the positions of the standard and the variables, we might have concluded in favor of shrinkage or expansion, depending upon the space order used. Our experiment indicates, therefore, the futility of basing conclusions upon results obtained under a single set of conditions. The mere variation of space order gives different results and radically alters their interpretation.

For *D,* unlike the other *O*s, the objects in the region of the optic disk are larger in both space orders, by 0.45 cm. in the first and by 0.23 cm. in the second. But rather than inferring a contraction of the spatial values in the region of the optic disk, and appealing to individual differences, we are of the opinion, because of what is known of the spatial disposition of attention[38] and *D*'s disposition in particular,[39] that we are dealing here with a constant space error. We shall, however, return to this point later.

The results of the Control Experiment upon functional areas of the retina are similar. In the first space order, the variable (at the left) was 0.07, 0.75, and 0.54 cm. smaller for *C, H,* and *J* respectively than the standard, and in the second space order, the variable (now at the right) was 0.11, 0.95, and 0.12 cm. smaller. As in the Main Experiment, the variable was consistently smaller than the standard, showing again that these 3 *O*'s were predisposed toward it.

D's results in the Control Experiment stand again in contrast to those of the other *O*s, showing a *spatial* disposition that persists in both space orders. But now it is the object to the right of fixation, whether the standard or variable, that is smaller (0.06 and 0.51 cm. respectively for the first and second space orders). *O* is oppositely disposed in this experiment and in the Main Experiment, and thus some doubt is cast upon the interpretations drawn there. It suffices here merely to note the discrepancy.

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[38] K. M. Dallenbach, Position vs. intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-287; also R. S. Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left-handed observers, *ibid.,* 35, 1924, 267-269; A. M. White and K. M. Dallenbach, Position *vs.* intensity as a determinant of the attention of left-handed observers, *ibid.,* 44, 1932, 175-179.

[39] *D* has a pronounced attentional disposition to the left (Dallenbach, *op. cit.,* 284 f.).

We now compare the results of the two experiments. In order to eliminate the positional effects of the standard and the variables, the results of the two space orders were combined in each experiment. The composite limens, together with their measures of precision (h), σ values,[40] and reliability are given in Table III. This table does not disclose any striking differences between the comparisons made in the two experiments (0.09, 0.11, 0.28, and 0.14 cm. for *C, D, H,* and *J* respectively). Moreover, the differences lie, as shown in Table III under the caption "D," in opposite directions for the 4 *Os*: in the direction of the Main Experiment for *D* and *H,* and in the direction of the Control Experiment for *J*. *C* is practically balanced between the two experiments. All the differences, with the exception of *H*'s, are too small to be statistically reliable. *H*'s difference, on the other hand, is reliable, as the value obtained for D/σ diff. indicates mathematical certainty. *H*'s results, however, offer small comfort to those claiming anomalous spatial perception in the region of the optic disk, as they merely show that he compared more accurately at A and B (Fig. 1) than at C and D—a result that might have been expected, since the latter were slightly further removed from the center of fixation than the former.

TABLE III

SHOWING, FOR EVERY *O*, LIMENS AND MEASURES OF PRECISION IN EACH EXPERIMENT, DIFFERENCES BETWEEN LIMENS IN EACH, AND SIGNIFICANCE OF DIFFERENCES

O	St.	Main Experiment			Control Experiment			D	σ diff.	*D*/σ diff.
		L	*h*	σ	*L*	*h*	σ			
C	24	23.99	.67	1.06	23.91	.85	.83	+.0818	.0424	1.92
D	25	24.89	.62	1.14	24.78	.48	1.48	+.1090	.0591	1.84
H	23	22.43	.73	.97	22.15	.67	1.06	+.2815	.0447	6.29
J	25	25.52	.51	1.39	24.67	.51	1.38	−.1484	.0616	2.40

D's results in the Main Experiment, which showed differences of 0.45 and 0.23 cm. in the two space orders in favor of the region of the optic disk, might be regarded as evidence of shrinkage—a larger object in the optic disk equaling a smaller one in a symmetrically placed retinal area—but we were of the opinion, as remarked above, that these results were due to a constant space error, to a disposition of this *O* toward the left. When we turn to the results of the Control Experiment, however, we find that this explanation will not suffice, because these results show a disposition in the contrary direction to the right—the object to the left now being larger than that to the right. A constant space error will not therefore explain *D*'s results. Neither, for that matter, will the hypothesis of anomalous spatial values in the region of the optic disk, for the difference between *D*'s liminal values in the two experiments is, as shown in Table III, little better than chance. We are at a loss to explain *D*'s results, but explanation not involving the optic disk is not our immediate concern. *D*'s results are not significantly different in the two experiments, consequently we feel warranted in concluding that for him, as well as for the other 3 *O*s, spatial perception is not unlike in the region of the optic disk and in peripheral vision generally.

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[40] Calculated by the formula: $\sigma = 1/h(2)^{1/2}$.

SUMMARY AND CONCLUSIONS

The results of this study may be briefly summarized. We found in the Main Experiment, in which spatial perception in the region of the optic disk was compared with perception in a symmetrically located area of the retina, a space error constant for one *O* and lying in the direction of the variable object for the other 3 *O*s.

Similar results were obtained in a Control Experiment, in which the spatial perception of two retinal areas symmetrically located outside of the blind spot was compared.

A comparison and an analysis of the results of these two experiments led us to the conclusion, in confirmation of Weber and of the experimenters who have followed him, that there is no difference in spatial perception between the region of the optic disk and the retinal periphery in general.

The suggestion is offered that the earlier discrepant conclusions may be due in large part to the fact that they were based upon results obtained under but one set of objective conditions.

THE DEPENDENCE OF AUDITORY EXPERIENCE UPON WAVE AMPLITUDE

By Forrest Lee Dimmick, Hobart College

The vibratory motion which acts as the stimulus for sound may vary in frequency and in amplitude. The single attribute of pitch was held to parallel frequency and the single attribute of intensity to correspond to amplitude. When careful observation was brought to bear upon stimulus-frequency and its attribute, pitch, a number of psychological characteristics were found. Rich has given us an account of this expansion of 'pitch' into several qualitative attributes,[1] but no one has raised the question of the simplicity of the attribute of intensity. In view of what has happened to visual intensity,[2] it is apparent that this must be done. In the past, experiments that dealt with auditory quality used primarily periodic stimuli or tones, while most of the work on intensity avoided tones and used aperiodic stimuli or noises, such as falling balls or the sound pendulum.[3] To a large extent this has been due to the technical difficulties involved in measuring the stimulus. It is easy enough to determine the height of fall of a steel ball and to calculate the energy released as a function of that distance. To measure the amplitude and energy of a tuning fork or an organ pipe is a more difficult matter. Recent developments, however, in telephony and transmission engineering have put amplitude under precise control, and practical problems in those fields have formed the bases of several fundamentally psychological investigations. Unfortunately, however, the emphasis has been entirely upon the stimulus and the observational conditions have been neglected.

Fletcher in his recent book brings together many data that are of interest to psychologists. Some of them, however, can not be used as they stand because of the unusual experimental methods by which they were obtained. This is especially true of the values given for the minimum perceptible differences of sound intensity obtained in two different experiments with comparable methods, the second of which was carried on in the Bell Telephone Laboratories by R. R. Riesz apparently under Fletcher's direction.[4] In both investigations there was presented to the *O*s a pure tone

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* Accepted for publication May 20, 1932. This study was made possible by a grant from the National Research Council for the purchase of apparatus.

[1] G. J. Rich, A Study of tonal attributes, this Journal, 30, 1919, 121 ff.

[2] E. B. Titchener, Visual intensity, this Journal, 34, 1923, 310 f.
C. Stumpf, *Die Attribute der Gesichtsempfindungen,* 1917.
L. T. Troland, *Psychobiology, II. Sensation,* 1930, 222 ff.

[3] Titchener, *Experimental Psychology. Instructors Manual, Quantitative,* 1905, 55 ff.
H. Fletcher, *Speech and Hearing,* 1929, 145.

[4] *Op. cit.,* 145 ff.

that could be made to fluctuate in intensity or to beat. Knudsen,[5] the earlier investigator, did this with a mechanical device which increased and decreased rythmically the resistance in the output circuit of a vacuum tube oscillator. The amount of rythmical change or Δ R was varied until it was determined at what point the resultant tone fluctuated just audibly. From the known constants in the circuit and the measured output of the receiver the root mean square (r.m.s.) pressure changes that could just be heard were calculated. Riesz extended Knudsen's work by introducing into the circuit with the standard frequency, a frequency differing by 3 d.v. per sec. from the standard, thus producing beats.[6] The voltage of the added frequency was adjusted to the point at which beating or fluctuation could just be heard. Fletcher describes Riesz' procedure as follows: "The minimum audible voltage of one attenuator, say B_1 (the standard tone) was determined, B_2 (the mistuned frequency) being set far below the minimum audible voltage. B_1 was then set at any desired voltage and B_2 adjusted until the observer signaled that he heard beats. The setting of B_2 was changed each time the observer signaled whether the tone seemed to fluctuate. After about 20 such adjustments the operator was able to locate with considerable certainty the setting of B_2 for which the observer was just able to detect a fluctuation in intensity."[7] From the minimum voltage of the mistuned frequency necessary to produce beats the change in the r.m.s. pressure in the ear was calculated as the intensive difference limen.

The use of this value for the intensive D.L. is a novel one. The implied physical argument that the only stimulus-change involved in the above procedures is that of intensity and that the perceptuation of fluctuation or beating depends upon a certain minimum degree of such stimulus change is obviously valid. On the other hand, the perception of fluctuation or beating is by no means a perception of simple sensory change in loudness. The intensive D.L. has always been taken to imply the perception of two discrete, successive sounds as different or as different in intensity. It is conceivable that the stimulus-difference necessary for the perception of fluctuation may prove to be the same as that for the perception of discrete difference, but that requires experimental demonstration.

In the following experiments we undertook to determine a group of discrete, intensive D.L. for tone by the usual psychophysical method for the purpose of comparison with and evaluation of the fluctuation D.L. cited by Fletcher.

Method and procedure. We used as a source of sound the output of a beat-frequency oscillator fed into a 12 in. dynamic loudspeaker. The electrical output of the oscillator and the sound output from the speaker were carefully checked for the presence of audible harmonics. The former was examined directly with a cathode-ray oscilloscope and proved to be an almost perfect sine wave form. The tendency of the beat-frequency type of oscillator to drift or change in pitch was checked frequently while it was in use by reference to a standard frequency setting and never varied more than 2 d.v. in an experimental period. The wave form and the r.m.s. pressure given by the loud speaker for every value of voltage fed into it were observed and measured. A condenser microphone and amplifier calibrated to read in bars gave us the quantitative values and its electrical output as examined in the oscilloscope indicated that the speaker introduced no distortion of the original wave form. The speaker was situated in a sound-proof room with thickly padded walls which absorbed a high percentage of the sound that struck them so that the effect of standing waves was negligible at our stimulus-intensities. The *O* sat 3 m. in front of and facing the speaker. He maintained a constant position of his head during the presentation of stimuli. Using the method of constant stimulus differences, after the usual preliminaries, we determined the D.L. with 6 *O*s for a single frequency, 256 d.v., at 4 amplitudes.

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[5] V. O. Knudsen, The sensibility of the ear to small differences of intensity and frequency, *Physical Rev.*, 21, 1923, 84.

[6] Fletcher, *op. cit.*, 146 ff.

[7] The fundamental difference between Knudsen's and Riesz' methods seems to be that, whereas the intensity change in a beating tone is relatively gradual, the fluctuations produced mechanically were abrupt.

Results. (1) Limens. Table I gives the absolute and relative D.L. in r.m.s. pressure (bars), in power (microwatts), and in the relative units of power ratio to the standard of 1 microwatt. For comparison, Reisz' values at the same pitch and the same intensities are put into the fourth section of the table.[8] In either unit, it is obvious that the limens Fletcher reports are much smaller than ours.[9] Our relative D.L., the ratios of the power increments to the standard powers, are more than twice as large as his. Such wide differences must be due to the difference in the observational problem of judging either intensive likeness or difference between two discrete tones, or the smooth or fluctuating character of a single tone. Certainly the differences between the two sets of results are so great as to be of importance in practical problems as well as in psychological. The parallel courses of the two curves shown in Fig. 1 indicate that the two types of data may be related and may possibly depend upon the same aspect of the auditory mechanism, but Fletcher's results cannot be taken as equivalent to the 'just noticeable difference' or intensive D.L. as that term is used and understood in psychology.

It is evident that although the absolute values of the D.L., either in bars pressure or in microwatts power, increase as the standards increase;

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[8] Fletcher, *loc. cit.*

[9] It has become customary in transmission engineering to express sound intensities in units of their power ratios to a given standard. The usual standard is one microwatt. Since the literature contains values expressed in bars r.m.s. pressure and since Fletcher gives Riesz' relative D.L. in microwatts, we have tabulated our results in all three units. The various measures are related according to the following formulae: (1) $J = p^2/415$; (2) $\propto$ (db) $= 10 \log_{10} J$; (3) $\Delta\propto = 10 \log (1 + \Delta J/J)$. The values of $\Delta J/J$ and $\Delta\propto$ quoted from Riesz are calculated from his tables and formulae. Fletcher, *op. cit.,* 151.

the rate of increase does not follow the simple form of Weber's law.[10] This fact is even more apparent in the relative D.L. The ratios of the D.L. increments to their standards decrease rapidly as the standards increase. This same fact is noted by Fletcher in reference to Riesz' D.L. of fluctuation where it is further complicated by frequency.[11] The mathematical relation between sensation (S) and stimulus in microwatts (J) for our results[12] is expressed in the formula: $S = 7.2 \log (J^{.79} + 4.6)$. Because of the limited number of determinations upon which it is based, this formulation is offered only tentatively as suggestive of the general form of Weber's Law that may be applicable.

TABLE I

D.L. OF AUDITORY INTENSITY AT 256 D.V.

Standard (r.m.s. pressure in bars)	p	.054	.09	.126	.174
	$p+\Delta p$	.0756	.1176	.1637	.2174
	Δp	.0216	.0276	.0377	.0436
	$\Delta p/p$	.40	.31	.30	.25
Standard (microwatts $\times 10^{-6}$)	J	7.03	19.52	38.26	72.95
	$J+\Delta J$	13.77	33.33	64.57	114.10
	ΔJ	6.74	13.81	26.31	41.15
	$\Delta J/J$	.96	.71	.69	.56
Standard (decibels-power ratio to 1 microwatt)	α	−51	−47	−44	−41
	$\Delta\alpha$	2.75	2.38	2.28	1.99
Standard in Sensation Level (db above minimum audible power)	S.L.	24	28	31	34
Riesz' D.L. of fluctuation calculated from his formulae					
Standard (microwatts $\times 10^{-6}$)	J	7.03	19.52	38.26	72.95
	$\Delta J/J$	.44	.34	.28	.27
	$\Delta\alpha$	1.58	1.27	1.07	1.04

(2) *Analysis of the intensive judgment.* In the course of our experi-

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[10] M. Guernsey, A Study of liminal sound intensities, this JOURNAL, 1922, 554. Guernsey made a number of determinations of j.n.d. ratios beginning at the lowest audible intensity. She concludes that "Weber's law as applied to audition apparently holds true with a fraction of about one-third throughout the middle range of intensities. The fraction is larger for low tones and for very high tones. The fraction is also larger near the limen." This seems to be in general agreement with Riesz and with us, but it is impossible to make a direct comparison of results since her's are given in terms of change of resistance in her circuit and were obtained, apparently, by a method of continuous change.

[11] Fletcher, *op. cit.*, 149.

[12] I am indebted to Professor Walter H. Durfee for the mathematical determination of the formulae.

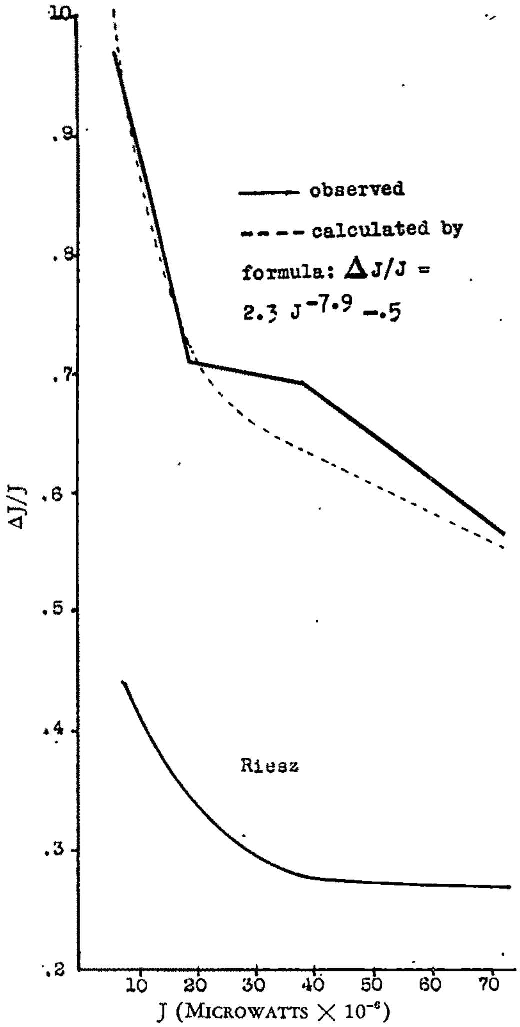

Fig. 1. The Relative D.L. as a Function of Stimulus-Intensity at 256 D.V. from Riesz' Data and from Ours

ments we asked the *O*s to describe the experimental bases upon which they were making their judgments. From these reports it is evident that the difference between two tones is not as simple as, for example, that between a lighter and a darker gray. All *O*s agreed that there is some sort of more-less aspect in the tones that is obvious when the tones differ considerably, but that this is not the only mode of differentiating between them, and further that it is not necessarily the basis of judgment when the difference between the tones is small. In such cases the judgment approaches one of just 'different' instead of 'different in intensity.' One *O* reported that the weaker tone is "smaller," "further away," "less tonal," "simpler," "shorter," "duller or lower." Another characterized the louder tone as "more spatial," "thicker," "not as distant," "clearer," "double," "well rounded," "higher in pitch." All the *O*s applied comparable terms to the tones and reported that when the members of a pair were only slightly different, one or more of the other aspects or criteria were used in making the judgment. In addition, visual and motor accompaniments were often present and invariably carried some stimulus reference such as, "you pushed harder for the first tone."

TABLE II

COMPARISON OF LIMENS OF INTENSITY AND OF VOLUME

(Values in bars p and microwatts $\times 10^{-6}$, J)

		Intensity	Volume	Volume/Intensity
D	Δp	.037	.132	3.5
	ΔJ	26.4	122.1	4.6
C	Δp	.043	.072	1.6
	ΔJ	30.7	56.2	1.8
M	Δp	.029	.082	2.8
	ΔJ	19.4	65.6	3.4

When we had completed the determination of the series of intensive limens, we selected three of the most common characterizations or secondary criteria; namely, volume, brightness, and pitch, and determined the limens for them with 3 *O*s, at a single intensity (0.126 bars; 38.3×10^{-6} microwatts; —44 db.). The *O*s had no difficulty in judging volume and gave much larger limens than for intensity. These judgments, therefore, are fundamentally different from those of intensity. The volume aspect which seemed to the *O*s as definite as intensity was characterized as "an impression of size," "total extensity," "number of elements." It seems safe to state, then, that volume changes with amplitude as well as intensity.[13]

The conclusions that may be drawn from the brightness D.L. are not so positive. Their magnitudes approximate those for volume so that the brightness judgments certainly are not the same as the intensity judgments. The reports of the *O*s indicate that brightness is probably different from

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[13] H. M. Halverson, Tonal volume as a function of intensity, this JOURNAL, 35, 1924, 360. Our results are in close agreement with Halverson's though a quantitative comparison cannot be made since his and our sets of values cannot be reduced to the same units.

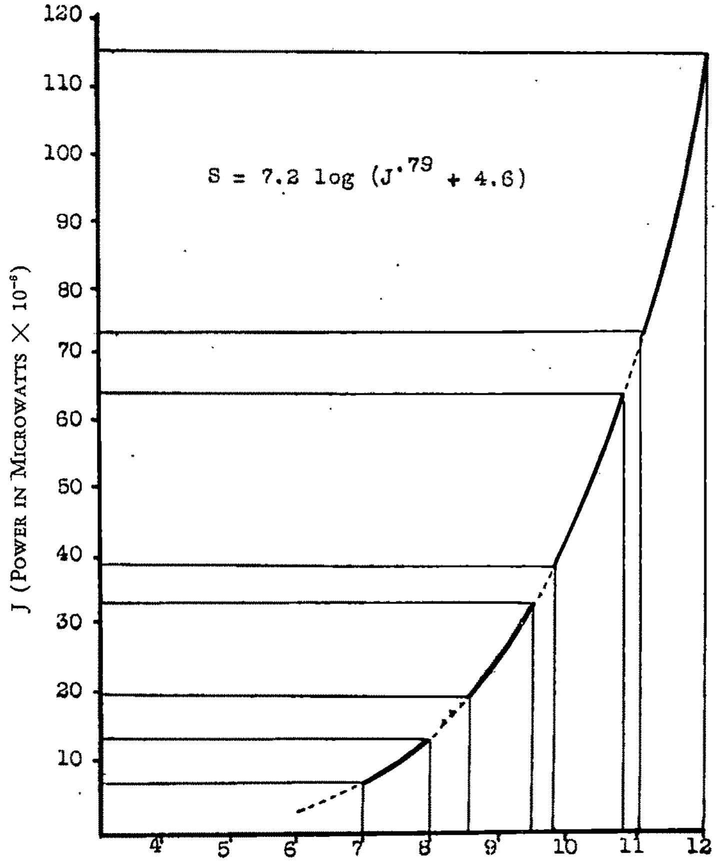

FIG. 2. SENSORY-LOUDNESS AS A FUNCTION OF STIMULUS-INTENSITY
(The solid parts of the curve represent the observed j.n.d.)

volume as well. Bright tones are "clear," "sharp," "pointed," "shrill," while dull tones are "unclear," "flat," and lack "sharpness."

The determination of pitch D.L. proved impossible. While all *O*s insisted that there were frequent changes in pitch, often so great as to obscure the intensity change, they showed no close functional relation to the stimulus-intensities, though there appeared a general tendency for louder tones to be heard as higher.[14]

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[14] W. R. Miles, Accuracy of the voice in simple pitch singing, *Psychol. Monog.*, 16, 1914, (no. 69), 13-66. Miles noted the tendency of a singer to reproduce a more intense tone as higher in pitch than the same tone sounded more weakly.

SUMMARY

Our experimental results show four things.

(1) The "minimum perceptible differences" given by Fletcher are not the same as the usual intensive D.L.

(2) It is apparent that for the determinations we made the simple form of Weber's Law does not hold. Our results give the formula, $S = 7.2 \log (R^{.79}79 + 4.6)$.

(3) The assumed one to one relation between physical amplitude and psychological intensity is too simple to be adequate. Changes in wave amplitude produce changes in auditory volume, brightness and pitch, as well as in intensity.

(4) Intensity judgments themselves are perceptual and, unless great care is taken in control of the observational setting, may depend so much upon secondary criteria that they do not reflect accurately either psychological intensity or physical amplitude.

INSTRUCTION AS A FACTOR IN 'INCIDENTAL' LEARNING

By JOHN G. JENKINS, Cornell University

The problem of 'incidental' learning—that is to say, learning which occurs in the absence of a specific intent to remember—has undergone an extraordinarily large amount of experimental investigation during the past fifty years. Although Myers[1] reports that the first experiments were conducted by Cattell in 1893, we find that Ebbinghaus was aware of the existence of the problem early in the preceding decade,[2] and that he attempted to maintain a constant attitude towards his nonsense syllables during his investigation. Since all of the researches on the psychology of testimony had been regarded as significant for this problem, the experimental literature contained more than one hundred titles by 1913,[3] and at least fifty more have been added during the two succeeding decades. These studies have shown such close agreement that Meumann was able to state that "if we have the will to imprint the observed data upon memory in order that we might subsequently be able to reproduce them, we actually retain them more readily; and when the will to remember is lacking, the incorporation into memory fails to take place, or if it does occur, it is a matter of sheer accident."[4]

A study of the literature reveals, however, such marked discrepancies between the results of the various investigators that one is led to accept Bergson's dictum that incidental memory is a 'capricious' affair. In a recent study, Shellow has taken note of this apparent capriciousness and has collected evidence which leads her to conclude that "incidental memory is only 'capricious' when we do not know its determining factors, but once these are analyzed and the laws governing them formulated by experimental research, we find incidental memory as logical and as inevitable as the better accredited form of direct memory."[5] Her contribution to our knowledge of the determining factors is summarized in one sentence which points out that the materials retained in incidental memory are those "which had become fixated because their own intensity was sufficient to cause fixation, or because they had in some way become linked up with

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* Accepted for publication November 21, 1931.

[1] G. C. Myers, A study in incidental memory, *Arch. Psychol.*, 4, 1913, (no. 26), 3.

[2] H. Ebbinghaus, *Memory*, Eng. trans. H. A. Ruger and C. E. Bussenius, 1913, 11.

[3] Myers, *op. cit.*

[4] E. Meumann, *The Psychology of Learning*, Eng. trans. J. W. Baird, 1913, 74.

[5] S. M. Shellow, Individual differences in incidental memory, *Arch. Psychol.*, 10, 1923, (no. 64), 75.

the individual's habitual interests and past experience, and had become incorporated with them."[6] The present study is designed not so much to add further evidence to a mass of experimental data already bulky as to attempt a search for additional determining factors with a view to reducing still further the apparent capriciousness of incidental memory. To this end a standard technique was employed, patterned closely after the conventional methods of many of the earlier investigations. Since the most critical technique seemed to be that of comparing the scores made by the same individuals with and without the intent to learn and to remember, the method was shaped accordingly.

Procedure. One individual sat in a small booth. Through a window before him he had a clear view of the drum of a Spindler and Hoyer memory apparatus as it revolved before a large reading-glass under constant illumination. Outside the booth a second observer sat in such a position that the syllables on the drum were invisible to him; he then prepared deliberately to learn the syllables as they were read to him by the first observer.[7] When the two *O*s were in position, the following common instructions were read aloud to them.

> You are to take part in an experimental investigation of auditory memory in which an attempt is to be made to determine the effectiveness of memorizing from the sound of the syllables.

To anyone trained in psychological research, the vacuity of this statement would have been obvious, and suspicion would undoubtedly have been aroused. Since untrained undergraduates were to serve as *O*s, it seemed better, however, to attempt some such simple deception, rather than to tell them nothing of the nature of the problem and so to leave them at the mercy of the first guess that appeared on the scene. The success of this procedure will be apparent presently; we may remark now that all *O*s accepted our explanation in good faith and did not indulge in further speculation as to the purpose of the investigation.

The *O* who was to read aloud the syllables without intent to learn them (hereafter referred to as the 'reader' (*R*), took his seat in the booth and received the following additional instruction.

> Read each syllable clearly and distinctly as it appears in the window. Pronounce all vowels as short. Remember that the success of the experiment depends upon the accuracy of your reading.

The last part of the instruction was given with a view to reducing still further the probability that this *O* would attempt to learn the series. The other *O*—who was to make a deliberate attempt to learn, and who will be called hereafter the 'memorizer' (*M*)—took up his position outside the booth and was further instructed as follows.

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[6] *Ibid.*, 75.

[7] Two undergraduates, Mr. S. B. Schreuder and Mr. G. C. Zahn, secured the observers, read the instructions, and collected the reports. Without their faithful and painstaking efforts this study would not have been possible, and the author wishes fully to acknowledge his debt of gratitude to them.

> Listen carefully to the syllables as they are read. Make every effort to learn them as rapidly as possible. Try to anticipate each syllable before it is read. When you can do this for each syllable, have the reader stop while you repeat the series aloud. If you fail on the first trial, the reading will be continued until you are successful.

The syllables were now exposed (at a rate of 22 sec. for the 20 syllables) and read aloud by *R*, with a brief pause at the end of each series. When *M* had made a single successful reproduction of the series, both observers were released with the request that they return to the laboratory after 24 hours "to complete the learning." When they were seated in their proper places after a lapse of 24 hr., the following instructions were read to them.

> Write down on the slip of paper every one of the syllables you can remember. Try as hard as you can and spend as much time as is necessary.
>
> In addition, use the rest of the sheet of paper to tell all you can about your experiences in learning and in reproducing. Describe your attitudes and tell about any cues or other devices you may have used either in the memorizing or in the reproduction. Any descriptive comment you may make, no matter how irrelevant it may seem, will be of value to the experimenter. Above all, do not discuss this experiment with anyone. Its future success depends absolutely upon the maintenance of secrecy regarding the procedure as observers will be chosen from all parts of the student body.

The success of this and of our earlier precautions is indicated by the absence of any foreknowledge on the part of our *O*s. None of them had been forewarned of the actual purpose of the investigation; all *R*s expressed surprise and not a few indignation at being asked to recite upon what they had never been instructed to learn.

Since the procedure called for a comparison of the scores made by the same individual observing with and without the intent to remember, the person who had acted as *R* was now drafted to serve as *M*, while a new *O* took his place in the booth to read aloud a new series of syllables, unaware that he would be asked later to reproduce the syllables he had been reading. Data will be presented on 24 individuals who served in the dual capacity as *R* and *M*. Thus we shall be able to compare at any time the scores of the same individual as he observed under the two sets of instructions, and we shall not be compelled to limit ourselves to gross comparisons of groups.

RESULTS

An initial glance at the results (Table I) will show that our observers when acting as *R* (and hence free, as we presume, from any intent to learn) recalled on the average only 10.8 ± 3.6 syllables, while the same individuals recalled 15.9 ± 2.4 syllables when they were serving as *M* and making definite efforts to learn the series. Now this result, taken in its gross form, is in complete agreement with the common outcome of the earlier investigations in indicating that incidental memory is definitely inferior to deliberate intentional memory. Indeed we find no evidence at all of the alleged 'capriciousness' of incidental memory until we turn to the detailed consideration of the results for each individual observer. This information is supplied in Table I.

Granting that individuals will vary widely in retentiveness, the differences between the reports of the various observers are at least as striking as the general trend of the data recorded. Thus we note immediately that two *O*s actually reported *more* syllables when acting as *R* than they did as *M*, and one more who recalled the same number under the two attitudes, while the other *Os* vary widely in the differences to be noted between the two situations. This appears at first sight to be indeed a capricious and irregular affair, and, without the reports from the *O*s, it would be likely to remain so. The first clue to an explanation was discovered in a report made by one *O* in the early stages of the investigation. This *O*, while acting as *R* and while presumably free from any intent to learn—if the assumption of earlier investigators is valid—reported in part as follows:

TABLE I

Showing the Number of Syllables Recalled by each *O* while Serving as *M* and as *R*

(*O*s in the first column report that they made no attempt to learn as *R*)

O	Syllables recalled as		*O*	Syllables recalled as		*O*	Syllables recalled as	
	R	*M*		*R*	*M*		*R*	*M*
An	6	18	*Ar*	13	11	*Mo*	20*	16*
Ed	8	17	*Bo*	9*	13*	*No*	18*	19*
Ko	7	14	*De*	9	12	*Po*	9*	20*
La	7	13	*Fu*	8	15	*Pr*	14*	16*
Li	6	15	*Gl*	11*	16*	*Sa*	12*	13*
Mi	14	18	*Ho*	9	15	*Sc*	20*	20*
Ry	10	18	*Ka*	6*	17*	*Sz*	14	19
Tr	6	10	*Mc*	15*	19*	*Wi*	9	18
Av.= (Col. 1)	8.0	15.4				Av.= (Cols. 2 & 3)	12.3	16.2
A.D.	2.0	2.4				A.D.	3.5	2.3
			Av. of all=	10.8	15.9			
			A.D.	3.6	2.4			

> "After I had mastered the pronunciation, the experiment began to get a bit boring. So, since X looked as if he'd be all afternoon in learning, I tried memorizing them myself . . . I had learned them before X made his first attempt to recite them."

Here, then, is an *O* who, in spite of the fact that we were employing conditions established as standard for freedom from intent to remember in earlier researches, had actually embarked upon a deliberate attempt to memorize the syllables. Her report indicates that she is furnishing *not* a comparison of incidental learning and intentional learning, but simply a review of two cases of deliberate memorizing. Since we should never have suspected this without her report on attitude, it becomes imperative that we search the reports of other observers to discover whether any other subject has violated the major premise of the experimental conditions.

This search is, unfortunately, not as simple a problem as it would appear at the outset, for an organism is instructed, as Bentley has pointed out,[8] not alone by directions issued to it by someone else (formal instruction) but as well by itself (self-instruction) and by the occasion or situation confronting it (occasional instruction). The case we have just presented is obviously one in which the organism was acting under self-instruction; and we shall be concerned presently with numerous other examples of this sort. Meanwhile the reports must be searched for traces of occasional instruction. Since it is common knowledge that nonsense syllables are by no means 'nonsense,' and since everyone will grant that an untrained *O* will endow these syllables with meaning even if given specific instructions to the contrary, this aspect of the problem scarcely requires special mention.[9] This is, however, only one form of occasional instruction. The reports of our observers present much evidence to show that the occasion was constantly prompting the subject in other ways to remember this or that syllable as he read the series aloud. Thus one syllable called for memorization because it was difficult to pronounce; a second, because it indicated that a pause was to follow; while a third was persistently missed by the *M*. These examples and many others contribute to the notable fact that not a single *O* failed to indicate occasional instruction to learn certain of the syllables at a time when he was presumably free from any such instruction.

Moreover, if we concern ourselves for the moment only with self-instruction and scrutinize in this light the reports available, another noteworthy fact comes into view. Out of the 24 individuals with whom this study is concerned, 10 make definite, positive statements to the effect that they made deliberate attempts to learn *while acting as R*, and, according to the fundamental assumption of this traditional method, while they were wanting in any such intention. (It must be recalled that this report of occasional and self-instruction to learn comes to light in every instance without the aid of any question more specific than those contained in the general instruction to report upon attitude.)

It is possible to test roughly the significance of these reports on instruction by considering briefly the contrary and doubtful cases in Table I in the light of the *Os*', protocols. It is found that, with but a single exception (*Ar*), those who report as many (or more) syllables while acting as *R* state definitely that they made deliberate attempts to learn the series. Those who reported self-instruction to learn are indicated by asterisks after the number of syllables recalled.

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[8] M. Bentley, *The Field of Psychology*, 1924, 384-396.

[9] Cf. G. L. Freeman, The rôle of context in associative formation, this JOURNAL, 42, 1930, 173-212.

Sc (who reported 20 syllables under each attitude) states simply that "I attempted to learn in groups of four . . . and was able to say them over before *M* had learned them." *Mo* (who recalls 20 as *R* and 16 as *M*) begins his report with the statement "I attempted to learn the list," and tells in detail how he went about the process. *No* (18-19) and *Sa* (12-13) describe the way in which they went about memorizing while reading. *Po* notes with regret that he did not have time as *R* to memorize all of the syllables, which may account for the fact that he recalled only 9 in that capacity, whereas he retained 20 as *M*. *Gl* (11-16), *Ka* (6-17), *Pr* (14-16), and *Bo* (9-13) report 'half-hearted' or occasional attempts to learn. We cannot tell how far their results are affected by this amount of self-instruction, but we may be certain that the results of these *O*s do not belong among data representing the facts of 'incidental' learning. Finally we must exclude the results of *Mc* since he remarks innocently that he tried to learn the series by associating each syllable with its neighbor.

The case of *Ar* appears at first sight to fall badly out of line with our general conclusions, for she reports 13 syllables as *R* and only 11 as *M*, although she insists that she made no effort to learn during the former case. Even though she reports a wealth of occasional instruction while acting as *R*, her report as *M* provides a definite clue for the smaller number of syllables recalled under definite instructions to learn. Scrutiny of her report indicates that she made up an intricate mnemotechnical sentence for remembering the syllables as they were read to her. She repeats this in detail and succeeds in remembering 20 syllables with its aid (whereas she had remembered but 14 when acting as *R*). Her report shows, however, that she has warped the syllables to fit the scheme in the former situation. The result is that 9 of the 20 syllables recalled as *M* are incorrect, while only one of the 14 remembered as *R* (without mnemomic aid) is incorrect. On closer inspection, then, her case appears to be one of special difficulty, rather than of opposition to the general trend we have noted.

We have no way of knowing the extent to which self- and occasional instruction cut across the results obtained by earlier investigators; but there is no reason to suspect that these forms of instruction were less prevalent or less important in their studies than in ours. Realizing that such a small number of cases can have no final quantitative value, we may nevertheless search our own records for cases in which the observers state definitely that they did *not* attempt to learn. We find 8 who say that they made no attempt to memorize. If we isolate their results (as has been done in the first column of Table I) we find that they recalled only 8.0 ± 2.0 syllables while serving as *R* (compared with 12.3 for the rest of the group) while, as *M*, the number of syllables retained remains practically unchanged (15.4 ± 2.4 compared with 16.2 for the rest of the group). Even this decrease is, of course, not significant of results for the uninstructed organism, since everyone of these *O*s reported occasional instruction to learn, even though there was no self-instruction to this effect.

SUMMARY

The results of this study are two. (1) Further experimental support is given to a result often verified in the earlier experimental literature; that

is, that 'incidental' learning appears to be markedly inferior to intentional learning. It is shown, however, that, in this investigation at least, the gross results are diluted by the presence of data from subjects who were self-instructed to learn in a situation which supposedly involved nothing but incidental learning.[10] The reports of the observers contain a further suggestion that occasional instruction (instruction arising from the stimulus-situation in some of its aspects) may also have cut across and modified the result in every case. It is obvious that the conditions reported by Shellow may conveniently be reported under the general heading of occasional instruction. Since the general arrangement employed here is closely analogous to that used in many of the earlier researches, there is a strong suggestion that these researches may have been subject to similar dilution. This would imply that the difference between incidental and intentional learning may be much larger than has been supposed.

(2) The second, and perhaps the more important, result of this study is its indication that post-experimental reports from observers are vital necessities whenever the interpretation of the results hinges upon the attitude assumed by the observer during the experiment. Weber and Bentley have pointed to the importance of occasional and self-instruction,[11] and Anderson's experiments on attitude have led him to conclude that "specific instruction, designed as a means of assuring constancy of attitude and performance in experimentation, is by no means adequate even where observers are highly trained and carefully instructed. As an approach to an adequate control of attitudinal conditions it appears that frequent determinations of attitude must be made throughout any series of observations . . . and that self- and occasional instructions have quite as much to be taken into account as determinants of our psychological performance as have verbal commands of the formal kind."[12] Until such post-experimental reports on attitude are given, one may well be hesitant in accepting results obtained under the tacit assumption that the observers were successful in making and maintaining a critical shift in attitude demanded by the experimenter.

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[10] During a preliminary presentation of this report at a sectional meeting of psychologists one member of the group arose to suggest that all of our trouble would have been avoided if we had instructed our observers *not* to learn the syllables when they were serving at readers. His faith that negative formal instruction would succeed where positive formal instruction had failed and that it would cancel all self- and occasional instruction is worthy of report.

[11] O. F. Weber and M. Bentley, The relation of 'instruction' to the psychosomatic functions, *Psychol. Monog.*, 35, 1926, (no. 163), 15.

[12] O. D. Anderson, An experimental study of observational attitudes, this JOURNAL, 42, 1930, 368 f.

THE INFLUENCE OF ELECTRIC SHOCK IN MIRROR TRACING

By M. C. Barlow, University of Utah

Few studies in the field of motivation have been primarily concerned with the effect on learning of electrical shocks (pain) used as punishment for errors. Those that have touched upon this problem, moreover, employed methods and apparatus that have inadequately controlled the essential variables of the learning process. So as to escape the methodological errors of these former studies, the writer devised an apparatus and a technique that gave reliable measures of the speed and accuracy of learning. Specifically, the problem was to measure the influence of electrical shock upon the learning of a complex motor skill—mirror tracing of a 6-pointed star.

PROCEDURE

Method. The problem demands complicated apparatus, which has the special merit of making a continuous record of 4 variables at a time.[1] The records are made on waxed paper drawn over a revolving drum from two movable spools. Four telegraph sounders of low resistance, equipped with sharpened pieces of piano-wire fastened to the movable arms so as to serve as markers, were set in such a way that the markers stood in line on the moving paper. When *S* closed the different electric circuits, the markers which might register 5 or 6 responses each per sec., broke lines on the paper. A seconds pendulum operated one of the markers. Another of the markers was connected electrically to metal plates a, a_1, a_2, etc., in the non-conducting floor of the track of Fig. 1. The third marker was connected to 3 consecutive rays on the outside of the track, in series with the 3, in order, on the opposite side, but on the inside of the star, that is 1, 2, 3, 4P, 5P, and 6P. These were always the non-shock sides. Similarly, the shock sides were 1P, 2P, 3P, 4, 5, and 6. An AC variable speed motor of 1500 r.p.m. and a speed reducer with a ratio of 1 to 100, pulled the waxed paper under the markers. The shock was inflicted automatically through electrodes on the wrist of the *S*, when the stylus touched the shock-side of the pathway. It continued until he moved the stylus away. The shock was delivered from a lighting circuit of 110 volts which passed through a transformer with a maximum of 10,000 volts, and a rectifying tube, the filament of which was heated by the current from a toy transformer connected in series to the same lighting system. The temperature of the filament controlling the amount of DC current was regulated by a variable resistance coil of 10 ohms.

The *S*s, during the experiment, sat behind a screen, tracing with a stylus the brass star shown in Fig. 1. The greatest diagonal width of the star is 7 in.; the open pathway between the notched sides is ½ in.; the tracing-stylus, ¼ in. in diam. at the base. The stylus has a thin plate-like collar of 1-in. diam., which pre-

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* Accepted for publication October 22, 1931.

[1] For a description and diagram of the apparatus cf. *Popular Mechanics*, 55, 1931, (no. 2), 208.

vents *S*'s fingers from touching the metal part of the stylus which conducts the electric current. The shock continued only during the contact of the stylus on the shock-side of the star.

Subjects. The *S*s were selected at random from a class in introductory psychology. In the experimental group, Group I, there were 23 students; and in the control group, Group II, there were 30.

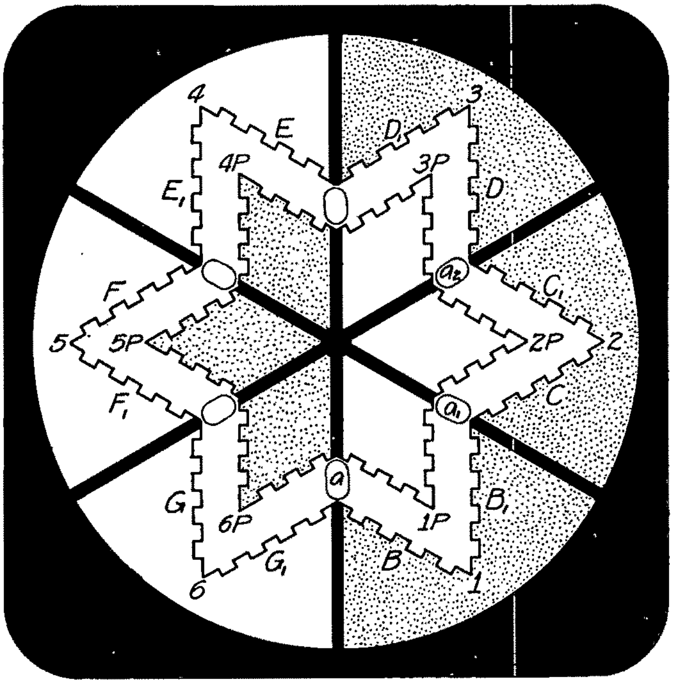

FIG. 1. DIAGRAM OF THE TRACING-STAR
The star, made of brass, is mounted on a bakelite base. The inside and outside track of every one of the 6 rays are insulated separately and connected by underposts in any combination desired.

Instruction. The *S*s of group I were given the following instructions:

> The metal plate before you has an open pathway shaped like a star. With a pencil-like stylus you are to follow this pathway as you see it in the mirror by looking over the cape which prevents you from seeing the star directly. Trace the groove until you can move continuously around without coming into contact with the brass edges on either side. When you touch a side you may see a spark and hear a sound. The spark will not injure you, but you will receive a harmless electric shock when the stylus touches the inside notches on the right half of the pathway, and the outside notches on the left half. Do your best always; errors will count against you. The starting point is the vertex of the angle of the inner star that appears farthest from you. Move through the pathway with the maximum of speed which is favorable for the quickest elimination of all errors. If you get tired, or

> restless, stop a short time; but you are to begin again where you leave the path. In case you jerk the stylus out of the trail return where you leave it. Remember, the task is to learn to move around and around the star without touching the sides. Put the stylus in place and begin when I say "Go."

The *Ss* of Group II were given the same instructions except that the part referring to the electrical shocks was omitted. The strength of the shocks was 0.5 milliampere at 10,000 volts. All the *Ss* made 20 circuits of the star.

RESULTS

(1) Speed. Infliction of pain by electrical shocks as punishment for errors slows down the rate of learning. Table I shows the average time taken by the *Ss* in completing the 20 circuits of the star.

TABLE I

SHOWING FOR EACH GROUP OF *Ss* THE AVERAGE TIME (IN SEC.) TAKEN IN MAKING 20 CIRCUITS OF THE STAR, AND THE AVERAGE NUMBER OF ERRORS ON THE SHOCK AND NON-SHOCK SIDES

Group	Time		Errors	
	Av.	S.D.	Shock side	Non-shock side
I	1202.8	433.0	175.9	238.2
II	990.2	358.7	286.3	302.2

The average for Group I is 1202.8 sec., S.D. 433. The best score was 572 sec., the worst, 2091 sec. The average for Group II was 990.2 sec., S.D. 358.6; the best score was 360, and the poorest, 1831. The significance of the difference between the records of the two groups of *Ss* is 2.88, which is large enough to be considered reliable. Punishment for errors by electrical shock increased the time of the performance by 21.5%.

The above results agree with those of Dodson who found that rats acquired more time to learn a two-part discrimination box when pain was used as a motive than when hunger was the spur.[2] They stand, however, in disagreement with the findings of Bunch, in which electrical shock applied as punishment for errors decreased the learning time by 30%.[3]

Certain comments made by the learners suggest that learning with punishment for errors resembles the case of learning with caution or threat as a warning. The *Ss*, while avoiding the shocks, would ofttimes verbalize as follows: "Must not touch that side; no, not that; look out for shocks; or danger there; this side is safe; and it doesn't shock here." Incipient movements, accompanying such responses, would bring the hand to a stop just in time to avoid a shock. Hence, as a result of inhibitions, it took more time to make one round on the star, and there were fewer errors.

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[2] J. D. Dodson, Relative values of reward and punishment in habit formation, *Psychobiol.*, 1, 1927, 231-276.

[3] M. E. Bunch, The effect of electric shock as punishment of errors in human maze learning, *J. Comp. Psychol.*, 8, 1928, 343-359.

It is generally understood that both caution and warning exercise an inhibitory effect on speed as well as errors. This, however, is a point for later investigation.

The reliability coefficient of 0.913 ± .03 was calculated from the paired odd- and even-numbered time scores of Group I; similarly, 0.787 ± .04 for Group II. The method of averages in the calculation tends to make the coefficients higher for the reason that individual irregularities are averaged. Paired odd and even circuits by individuals obviously would yield coefficients lower than those given above.

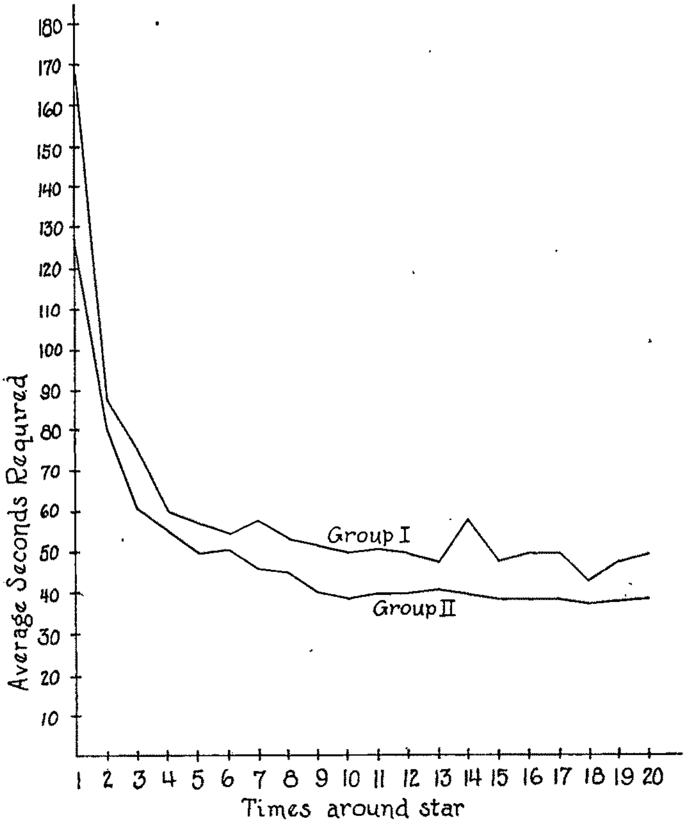

FIG. 2. SHOWING THE AVERAGE TIME REQUIRED BY EACH GROUP OF *Ss* FOR EVERY ONE OF THE 20 SUCCESSIVE TRACINGS OF THE STAR

Group I learned under punishment; Group II without punishment.

Average time in seconds is plotted against successive trials in Fig. 2. It can be seen at a glance that the average time of Group I is greater than

Group II for every trial. The graphs differ markedly in two respects, leaving out of consideration the first trial. Graph I descends with less steepness and straightens out with less flatness. The difference between the two mean scores in trial 1 is 40.4 sec. The average difference for trials 2-6 is 7.5 sec.; of trials 7-20, 10.9 sec. The punishment seemed to affect learning most during the early and late practice periods; and least during the intermediate stages, during which time rapid progress takes place.

Such an influence on learning cannot be accounted for in terms of emotional disturbance during the first few circuits of the star, since most of the *S*s reported that their apprehension gradually wore off and practically disappeared toward the end of the sitting. Two *S*s, however, were emotional throughout the performance, and a third noticed emotion only through the later circuits. In general, there was more emotion during the first half of the practice and less during the last. If emotion had served to slow down speed, the most marked effect would have taken place while the excitement was most stirring; but the reverse is true with the exception of trial 1, that is, the most effective influence of punishment on learning occurred during the stages in which there was least excitement.

The analysis of time scores in mastering the sides of the star, brings up the three problems; namely, difficulty of the sides, the place of serial order in learning, and transference of training. The same questions arise with both groups, as may be seen by the graphs of Fig. 3 in which seconds are plotted against successive sides of the star. The order in degree of difficulty for both groups is the same. The fan-like spread of the time-scores suggests temporal order, and position of the sides in the series as external factors which make for difference in amount of influence exerted by the shocks. Whether or not these are real factors or spurious ones remains to be proved. The tracing began from a point on the track directly in front on the side nearest *S*, and continued counter-clockwise around the star. Since most of the *S*s were right handed it is possible that the direction of movement had something to do with the difficulty of the various sides as well as transfer. In order to establish the apparent fact of primacy in acquiring visual-motor skill it will be necessary to obtain further data with 'serial position' and 'transfer' controlled.

The results of the present study thus far agree with the findings of Dodson, viz., that learning with errors punished, took more time than learning errors without punishment. Contrary to this, both Johanson[4] and

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[4] A. M. Johanson, The influence of incentive and punishment on reaction-time, *Arch. Psychol.*, 8, 1922, (no. 54), 1-53.

Vaughn[5] concluded that reaction-time decreased with electric shock administered as punishment for errors. The last conclusion might seem a contradiction to the others, but they all agree from the standpoint that the infliction of pain during learning always brings about responses aimed at avoiding punishment. Sometimes the action is slower, sometimes faster, all depending on whether or not pain is thereby avoided.

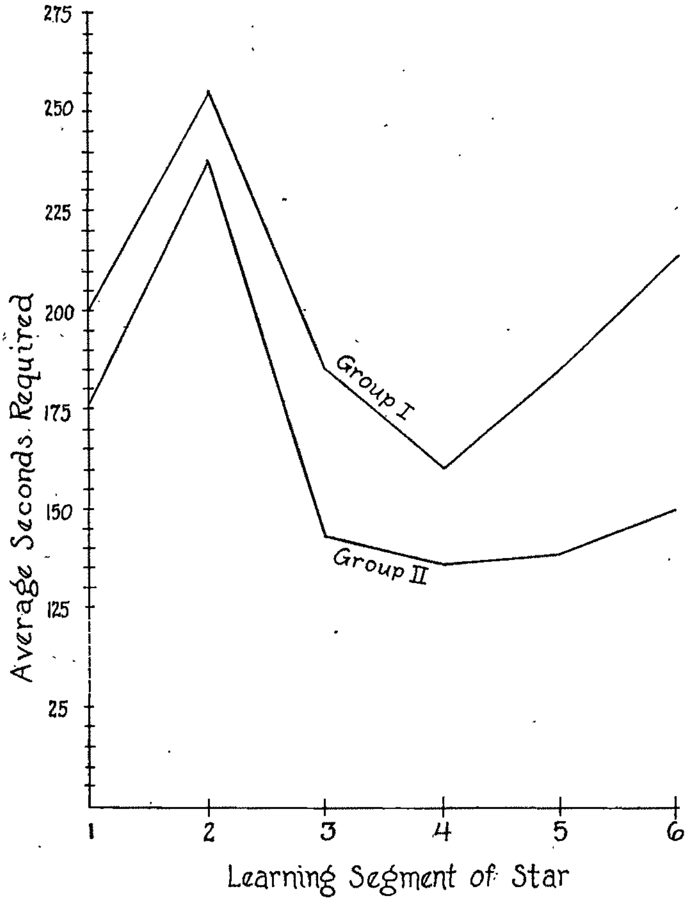

FIG. 3. SHOWING THE AVERAGE TIME REQUIRED BY EACH GROUP OF Ss TO LEARN EVERY STAR-SEGMENT
Group I learned under punishment; Group II without punishment.

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[5] James Vaughn, Positive vs. negative instruction, *Publ. Nat. Bur. Casualty & Security Underwriters*, 2, 1928, 1-172.

(2) Accuracy. Errors were mostly of the visual-motor type resulting from the disruption of previously established coördinations. As *S* looked into the mirror he began the test by following the pathway over the sections in order from 1–6 (cf. Fig. 1); *i.e.* counterclockwise from a to a. The transverse movements across the track were due to the illusion of the hand seeming to move forward, whereas it was actually moving backward, the converse also being true. While passing over sections B_1 and D, moving counterclockwise, the hand seemed to be approaching the body although it was actually receding; along C and G_1 it looked to be going toward the right and nearer himself, but the motion was toward the right and farther away. Misperception of motion in two directions occurred on every ray, the illusion in one direction held for one side, while the illusion of another direction held for the other side.

TABLE II

STAR-TRACING ERRORS BY GROUPS I AND II

(Average errors for each trial)

Group	Side	Trial						Total
		1	2	3	4	5	6	
I	Non-shock	36.1	75.8	36.2	31.5	34.8	23.8	238.2
	Shock	25.2	35.6	17.6	17.17	32.6	47.0	175.7
II	Non-shock	52.1	84.8	43.0	53.0	42.4	26.9	302.2
	Shock	44.3	68.5	34.3	28.6	48.0	62.6	286.3

Punishment while learning progressed, reduced the number of errors. The average number of errors made per round by Group I was 414.13, with an S.D. of 288.1. Similarly, the average of Group II was 590, S.D., 234.3. A difference between the two mean scores of 2.75 times the sigma difference indicates that Group I are reliably more accurate than Group II. Those given electric shock on account of mistakes, learned with 30% less errors than the *S*s did who were not shocked. The punished learners made fewer mistakes on all segments except the first, cf. Fig. 4.

The segments of the star shown in Fig. 1 were connected as described above to inflict a shock for errors from the inside edges of one side and the outside edges of the other side of the pathway; a punishment edge being on one side with a non-punishment edge opposite in every segment. Contacts on the non-punishment side are designated non-punishment errors; on the punishment side, shock, or punishment errors. These errors for both groups of *S*s are represented in Table II.

In Fig. 4 are represented the average errors made on each circuit. Group I is consistently more accurate than Group II with the exception of trail 1.

The *S*s of Group I showed a tendency to be more accurate on the shock sides than Group II; by making an average of 175.5 errors per trial, as compared to 286.3; the latter group did not receive any shocks from the shock sides. Also, Group I manifested greater accuracy on the non-shock sides, with an average of 238.2 mistakes per round as against 302.2, in spite of the fact that the former Group did not get any shocks from the non-shock sides. Evidently, the *S*s of Group I did not, while avoiding the

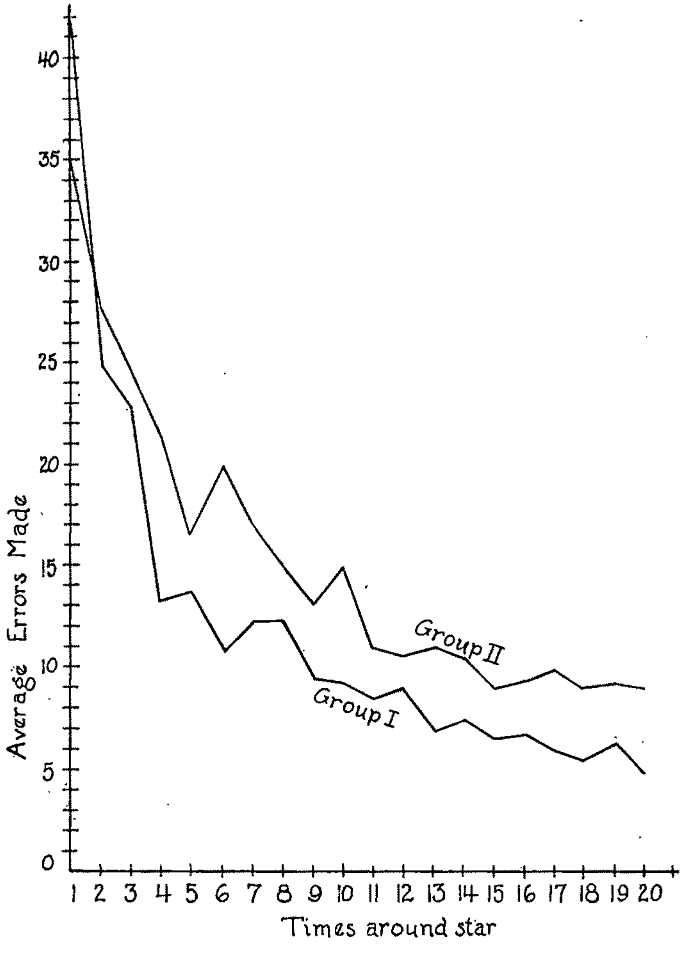

FIG. 4. SHOWING THE AVERAGE NUMBER OF ERRORS MADE BY EACH GROUP OF *S*S IN EVERY ONE OF THE 20 SUCCESSIVE TRACINGS OF THE STAR
Group I learned under punishment; Group II without punishment.

shocks, hug the non-shock sides. Accuracy resulting from the shocks was transferred to non-shock performances. The sigma difference of the two means of errors made on the non-shock sides, expressed in difference units

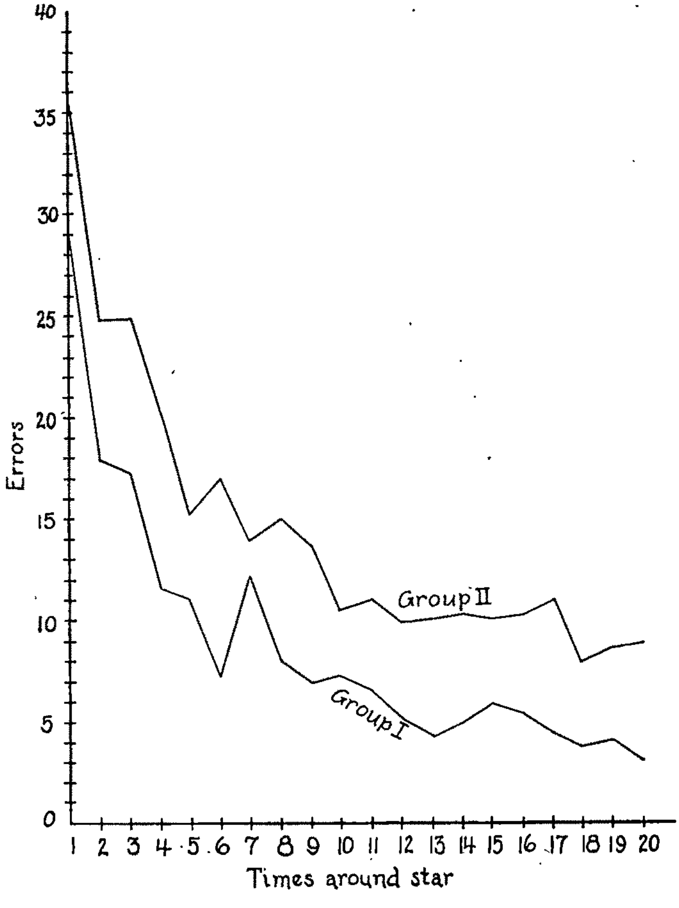

FIG. 5. SHOWING THE AVERAGE NUMBER OF NON-PUNISHMENT ERRORS OF EACH GROUP IN EVERY ONE OF THE 20 SUCCESSIVE TRACINGS OF THE STAR

Group I, curve of non-punishment errors by group learning under punishment; Group II, curve of non-punishment errors by control group, *i.e.* group learning without punishment.

is 1.68. Number of mistakes on each round is plotted against successive rounds in Fig. 5. Even though neither group were punished on the non-shock side, Group I was more accurate on account of the shocks given for errors on the shock sides. The shocks evidently brought about generalized motor response of averting contacts on both sides of the track.

In the case of errors the reliability coefficients were calculated from the averaged even-odd pairs of scores for the 20 trials. On the non-shock sides it is 0.896 ± .029 for Group I, and 0.857 ± .032 for Group II; similarly, on the shock sides they are 0.736 ± .069 and 0.766 ± .052.

Errors of the non-shock part by the Experimental Group are to be accounted for partly by transfer. The shock spurred the *S*s on to an early generalization of avoiding painful annoyances. They found apparent forward movements actually to be backward actions, and conversely; and also that seeming right- and left-hand directions were as they appeared. Some were unable to report on any learning cues; they stated that they learned to follow the groove in an unanalyzed way. The accuracy on one side of the star pathway resulting from punishment for errors on the other side, points to transfer as a plausible partial explanation.

Also, intentional inhibition plays a part in building up new sensorimotor coördinations. Mistakes leading to pain soon urge the *S* to anticipate the consequences of touching the shock sides. Mere starts follow in various directions until the successful direction is hit upon, thus mistakes may be entirely eliminated—naturally it takes more than average time. Thus learning would seem to be going forward on an incipient motor level.

Muscular tensions is a third possible factor. They were reported as being present in different degrees by all the *S*s of Group I. They seemed to be noticed principally in the fingers, arms, legs, face and neck. Some punished *S*s showed jerky responses during the first few rounds of the star.

CONCLUSIONS

(1) Mild electrical shock as punishment for errors in the case of mirror star-tracing, increased the time required for learning.

(2) On a given number of circuits in the mirrored star, punishment serves to decrease the number of errors.

(3) The effect of punishment is more pronounced on the later than on the earlier stages of star-tracing.

(4) Electrical shock applied as punishment for errors on one side of a course reduces the errors on the other side, although no shock is given from that side.

MODULATION OF THE OPTIC NERVE-CURRENT AS A BASIS FOR COLOR-VISION

By GLENN A. FRY, Washington University

Troland originated the idea of the modulation of the current of impulses in a single optic nerve-fiber as a basis for color-vision.[1] He proposed that the impulses passing along a given fiber could "be spaced in different characteristic ways to represent the various colors, luminosity still depending upon the total number of pulses per unit time."[2] One kind of modulation could consist in a periodic variation in the frequency of impulses as indicated in Fig. 1. The waves in the solid line represent impulses succeeding each other. These impulses must be conceived to be fundamentally the same in accordance with the all-or-none principle of nerve conduction. The frequency is high at A, low at B, high again at A′, and low again at B′. The dotted line represents this periodic variation in frequency.

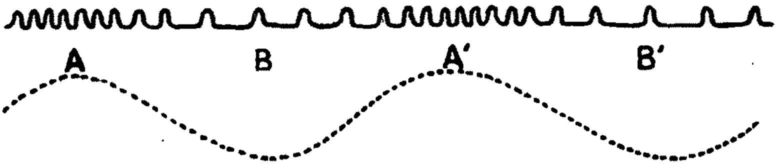

FIG. 1. MODULATION OF FREQUENCY

According to this scheme there are four independent variables: (1) the average frequency of impulses; (2) the frequency; (3) the form; and (4) the amplitude of the modulation waves. Intensity might depend upon the average frequency of impulses, and hue might depend upon either the form or the frequency of the modulation waves or both.[3] An unmodulated current of impulses might correspond to a sensation of white, and saturation of hue might depend upon the amplitude of the modulation waves. Blackness might depend upon the degree of inactivity.

In the case of a flickering color, flicker as well as hue must depend upon some variation in the sequence of impulses. It might be supposed that for a non-flickering color the periodic variation in frequency which subserves hue would be smoothly graded, and that for a flickering color the sequence of impulses would be irregular; however, this irregularity would not need to interfere with the slow modulation rhythms subserving hue.

The modulation theory relates strictly to the transmission of color-impressions from the retina to the cortex, and no assumption need be made as to what happens after the cortex is reached.

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* Accepted for publication May 5, 1933.

[1] The present paper is published from the Department of Ophthalmology, Oscar Johnson Institute, Washington University, St. Louis.

[2] L. T. Troland, The enigma of color vision, *Amer. J. Physiol. Optics*, 2, 1921, 23-48, esp. 43.

[3] Hue might depend upon frequency only indirectly as varying the frequency would change the form of the waves; or the cortical processes subserving color-vision might be affected separately by both frequency and form.

If color-vision depends upon the modulation of the optic nerve current, the problem arises as to how steady stimulation with colored light modulates the optic nerve current, for most of the color-sensations which come to us in everyday life are produced in this way. This is a problem of fundamental importance, but no data are at hand which throw any light upon it. Troland himself pointed out that when we raise the question as to the retinal mechanism by means of which colored light modulates the optic nerve current we find ourselves at a standstill.[4]

Although the principle of modulation can be applied to a single nerve-fiber, it is quite possible that a single end-organ is never stimulated alone, and quite often a large area of the retina is stimulated and a large group of fibers simultaneously brought into activity. Can these fibers function independently of each other as isolated units, or must the modulation rhythms in the various fibers be synchronized? Since neighboring retino-cortical paths interact with each other,[5] the modulation rhythm in one path might interfere with the rhythms in neighboring paths unless the rhythms in the various paths were synchronized. There is no final proof, however, that interaction would interfere.

The modulation theory requires the assumption that modulation patterns can be transferred across synapses without being interfered with in the process. An experiment of Adrian and Matthews[6] bears upon this problem. They exposed the retina of a conger eel to an intermittent white light and found intermittent phases of activity in the optic nerve fibers. The rate of these phases corresponded to the rate of stimulation as long as the rate of stimulation did not exceed 13 per sec., and this correspondence occurred in spite of the fact that synapses intervened between the retinal end-organs and the optic nerve-fibers. It is plausible to assume that during each phase of activity each fiber responded a number of times in rapid succession, since a nerve-fiber is capable of responding 200 or more times per sec. In other words, we have here an experimental demonstration that intermittent stimulation of the retina may set up modulated currents of impulses in the optic nerve fibers.

If intermittent stimulation of the retina can set up a modulated current of impulses in the case of man as well as in the case of the conger eel, the color-sensations produced with intermittent white light might be attributed to this modulation. With this in mind let us consider the experimental facts described in the remainder of this paper.

The disk illustrated in Fig. 2 is made of cardboard. It is 10 in. in diam. and the black band is 1 in. wide. B is an open slit. This disk was rotated before a bright surface whose position and size with respect to the disk are indicated by the dotted line. This bright surface consisted of a ground glass illuminated from behind with two 250-watt lamps. The front of the disk was illuminated with a 250-watt lamp placed 12 ft. away and at an angle of 60°. The situation was such that the brightness of B, the angular size of B, and the speed of rotation could be varied independently of each other, while the brightness of A and A′ and the brightness of C were held constant. When the disk is rotated in front of the bright surface, that part of the bright surface corresponding to the path of the moving slit intermittently stimulates the retina and produces a flickering color which changes hue whenever either the brightness of B, the angular size of B, or the speed of rotation is varied. The observations were made at a distance of 12 ft.

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[4] Troland, *op. cit.*, 43.

[5] For a summary of the evidence that the retino-cortical paths interact with each other, the reader is referred to F. B. Boswell, Visual irradiation, *Harvard Psychol. Stud.*, 2, 1906, 75-107. R. S. Creed (The physiological integration of sensory processes within the grey matter of the central nervous system: A critical review, *Brain*, 54, 1931, 29-53) has given a critical and historical survey of the evidence. In a series of investigations with the retina of the conger eel E. D. Adrian and R. Matthews (The action of light on the eye, *J. Physiol.*, 63, 1927, 378-414; 64, 1927-28, 279-301; 65, 1928, 273-298) have furnished abundant evidence that the retino-cortical paths interact with each other at the synapses of the retina.

[6] Adrian and Matthews, *op. cit.*

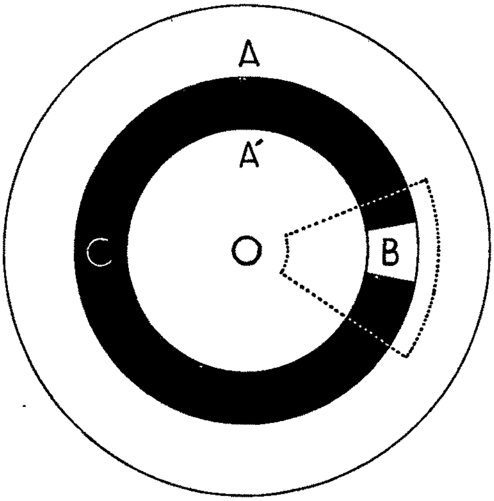

FIG. 2. STIMULUS DISK
B is an open sector, and the dotted lines show a sector of bright illumination behind the disk

The data in Table I show the effect of increasing the frequency of stimulation in several instances in which the duration of each stimulation is held constant. The brightness of B (402.7 c. per sq. ft.), the brightness of A and A′ (0.506 c. per sq. ft.), and the brightness of C (0.014 c. per sq. ft.) are all kept constant.

As the frequency is increased, the color changes, in the case of the 0.46σ stimulus, from blue to green and finally to white. In the case of the 1.39σ stimulus the color changes from purple to blue but then turns white without changing further in hue. In the case of the 2.78σ stimulus, the purple changes to white without changing in hue at all. In the case of the 6.94σ stimulus, the color changes from purple to purple-pink, to pink, and finally to white. In other words, purple and the 2.78σ stimulus seem to be transition points; with a shorter stimulus the color changes from purple toward green; with a longer stimulus the color changes in the opposite direction, *i.e.* from purple toward pink; but with the 2.78σ stimulus the color does not change in either direction.

Varying the frequency of stimulation changes the *frequency* of the modulation waves (at least it so happens in the case of the conger eel) and perhaps also the *form* of the waves. If color-vision depended on modulation, then the change of hue accompanying the change in frequency of stimulation could be attributed to either of these factors.

TABLE I

SHOWING THE EFFECT OF INCREASING THE FREQUENCY OF STIMULATION

Frequency of Stimulation	Color for Different Durations of Stimulation			
	0.46σ	1.39σ	2.78σ	6.94σ
4 per sec.	Blue	Purple	Purple	Purple
6 per sec.	Blue	Violet	Purple	Pink-purple
8 per sec.	Greenish blue	Violet	Light purple	Pink(blue borders)
10 per sec.	Greenish white	Light blue	White	White
12 per sec.	Greenish white	White	White	White
15 per sec.	White	White	White	White

The data in Table II show the effect of varying the angular size of B and the brightness of B, when the speed of rotation (8 per sec.), the brightness of A and A′ (0.506 c. per sq. ft.), and the brightness of C (0.014 c. per sq. ft.) are all kept constant.

TABLE II

SHOWING THE EFFECT OF VARYING ANGULAR SIZE AND BRIGHTNESS

Size of Slit	Duration of Stimulation	Color for Different Brightnesses of B			
		402.7 c. per sq. ft.	219.3 c. per sq. ft.	56.4 c. per sq. ft.	6.4 c. per sq. ft.
1°	.35σ	Blue	Bluish green	Dark gray	Dark gray
5°	1.74	Purple	Blue	Greenish blue	Dark gray
10°	3.74	Purplish white	Bluish white	Blue	Dark gray
15°	5.21	Pink-purple	Purplish white	Blue	Dark gray
20°	6.94	Pink (blue borders)	Purplish white	Bluish white	Dark gray
30°	10.41	Pink (blue borders)	Purplish white	Purplish white	Bluish green
60°	20.82	White	Pinkish white (blue borders)	White	Greenish blue
90°	31.23	White	Pinkish white (blue borders)	White	Greenish blue
120°	41.64	White	White	White	White

For a given duration of stimulation increasing the intensity causes the color to change progressively in the direction from green toward pink. Below a certain level, however, decreasing the brightness of B causes the color to change to gray.

For a given intensity of stimulation increasing the duration causes the color to change progressively in the direction from green toward pink. In other words increasing the duration of stimulation is equivalent to increasing the intensity. Beyond a certain limit, however, increasing the duration causes the color to change to white. This loss of hue is due, perhaps, not to the increase in duration of stimulation, but to the decrease of the interval between stimulations, for the size of C decreases as the size of B increases.

If it assumed that the *frequency* of the modulation waves is kept constant by keeping the frequency of stimulation constant, the change of color accompanying a change in duration or intensity of stimulation must be attributed to a change in the *form* of the waves, *i.e.* if the facts are to be explained on the modulation theory.

So far we have dealt with the rôles played by the speed of rotation, the angular size of B, and the brightness of B. Now what is the rôle played by the brightness of A and A′? In Table III the results obtained when A and A′ are brighter than C are compared with the results obtained when A and A′ are no brighter than C, the brightness of B (402.7 c. per sq. ft.), the brightness of C (0.014 c. per sq. ft.), and the speed of rotation (8 per sec.) being held constant. As can be seen in the table, pink can be produced without the aid of bright surroundings, whereas purple, blue, and green cannot. How do bright surroundings aid in the production of these colors? It might be supposed that bright surroundings suppress the retinal activity during the intervals between stimulation and thus help to modulate it.

TABLE III

Showing the Effect of Varying the Brightness of the Surroundings

Size of Slit	Apparent Color of the Band	
	Brightness of A and A′ 0.506 c. per sq. ft. Brightness of the Black Band 0.014 c. per sq. ft.	Brightness of A and A′ 0.014 c. per sq. ft. Brightness of the Black Band 0.014 c. per sq. ft.
1°	Blue	White
2°	Violet	White
5°	Purple	White
10°	Purple	White
15°	Pink-purple	White
20°	Pink (blue borders)	Pink (blue borders)

Summary. Troland's theory that the transmission of color impressions from the retina to the cortex depends upon the modulation of the optic nerve-current shows itself to be compatible with modern theories of nerve-conduction. An experimental basis for the theory is provided by the production of color-sensations with intermittent white light, inasmuch as it may be assumed that intermittent stimulation modulates the optic nerve-current. The dependency of these color-sensations upon frequency, duration and intensity of stimulation, and upon the brightness of surroundings, can be adequately accounted for on the modulation theory.

THE EFFECT OF SERIAL POSITION IN THE CONTINUOUS MEMORIZATION OF NUMBERS

By MILDRED B. MITCHELL, George School, Pa.

Robinson and Brown investigated the effect of serial position upon memorization when there was an interval of 10 sec. between the various trials in learning a series of numbers and nonsense syllables.[1] The present report deals with lists of 10 three-place numbers learned on McGeogh's memory drum by the *anticipation* method with *no* extra time interval between the various trials. The last number thus became the cue for the first and there was no break in the 2-sec. exposures until the list had been given correctly once from the first through the tenth number.[2] This last requirement of giving the numbers from 1 through 10, instead of stopping *S* whenever he responded correctly to any 10 consecutive numbers, may have tended to give results more like those of Robinson and Brown than would otherwise have been the case.

Out of over 23,000 correct responses given by 11 *S*s during the learning of 552 lists, there were more of these given for the first number in the list than for any other number in the list. The second number ranked second in quantity of correct responses, the third number ranked third, the last number ranked fourth, while the fourth number ranked a close fifth. Table I gives the total number of correct responses for each position in the series, the percentage of the total correct responses given in each position, and the ranked order for the 10 positions. As may be seen from this table, the other 5 positions vary only slightly; in fact, there is a range of less than 2% between number 8 which ranks last and number 5 which ranks sixth.

These results are in essential agreement with those of Robinson and Brown. Both primacy and finality facilitate learning, but the primacy effect is greater and extends farther than the finality effect.

The effect of the position of a number upon its ease of being learned is also shown by studying the correct responses in learning a particular list. For instance, list 33 was learned by all 11 *S*s. The number of correct responses for each number in the list was tabulated. The number of trials needed to learn the list to one perfect repetition ranged from 6 to 22. Since the *S*s did not attempt to give the numbers after they had made one perfect repetition, each number was tabulated as if correct from the perfect repetition through 22 trials. A few errors, no doubt, would have occurred if further trials up to 22 had been given to all the *S*s. Had the experiment been set up for the purpose of studying this problem, a sufficient number of trials would have been given to have eliminated the necessity of this inaccuracy. Since the same procedure has, however, been applied to all the numbers in the list, they

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[1] E. S. Robinson and M. A. Brown, Effect of serial position upon memorization, this JOURNAL, 37, 1926, 538-552.

[2] A more detailed account of the method and procedure is given in the study of which this was a part. Errors in the memorization of numbers, this JOURNAL, 45, 1933, 1-16.

may be considered comparable. Trial 1, the first presentation, was always zero. The first number in the list was given correctly by all 11 *Ss* after the fifth trial with 3 exceptions when it was missed by only 1 *S*. The learning curve for this number (Fig. 1), therefore, shows a rapid rise and is negatively accelerated.

TABLE I

Number Correct Responses by Position in List
(11 Ss learned 552 lists)

	Serial Position									
	1	2	3	4	5	6	7	8	9	10
No.	3447	3253	2932	2348	2023	1833	1944	1570	1717	2371
%	14.7	13.9	12.1	10.0	8.6	7.8	8.3	6.7	7.3	10.1
Rank	1	2	3	5	6	8	7	10	9	4

The sixth number, which ranks lowest for this particular list (it has 127 correct responses as compared with 130 for number 8), has a learning curve which is positively accelerated at the beginning (Fig. 1). The last number has a learning curve similar to that of the first number, while the eighth number has a learning curve more like that of the sixth number. Hence the more favorable positions have rather sharply decelerated curves, while there is some evidence for positive acceleration in the curves for the unfavorably situated numbers. This also is consistent with the results obtained by Robinson and Brown.

Conclusion. In conclusion, it may be stated that omitting a time interval between the trials in memorizing lists of numbers under the conditions of this experiment does not materially change the effect of the serial position. The first few and the last numbers still maintain an advantageous position with a negatively accelerated learning curve, while the others tend to be learned with greater difficulty and to have a positively accelerated learning curve.

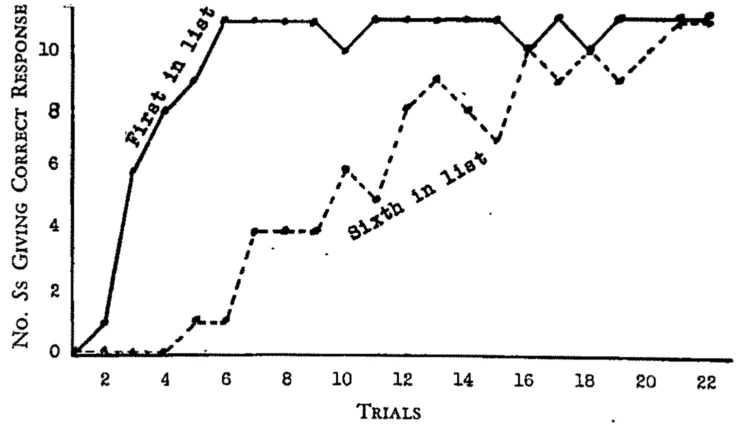

Fig. 1. Learning Curves for Positions 1 and 6 in a List Learned by 11 *Ss*

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

VII. The Affective Value of Combinations of Odors

By Willard Spence and J. P. Guilford

In a previous study,[1] the senior author proposed the possibility of predicting the affective value of a combination of stimuli from the affective values of its two components. Quantitative laws were suggested to apply to combinations of colors. The following theoretical conclusion was reached concerning the summability of affect: "There are some indications that the summation of feeling is an additive one and not a product, and that the combined value is not an algebraic sum but a weighted mean of P and U or of some simple function of P and U."[2] By 'P' was meant the more pleasing member of the combination, and by 'U' the less pleasing member. Quoting further, "It is suggested that there is a limit to the prediction of the 'affective whole' from its 'components,' and that the emergent or 'Gestalt-effect' may be measured by the index of non-determination, K^2."

It is the purpose of this study to test the validity of these conclusions by using combinations of odors. There are several reasons why combinations of odors might be better material with which to test these assertions than are combinations of colors. The latter must necessarily be separated spatially; the former can be more intimately mingled and fused. This fact might, on the one hand, produce a more unitary perceptual experience and therefore give a more complete amalgamation of affective responses. One cannot be sure that two colors, although juxtaposed in space, will be taken by his *O*s as a unitary perceptual experience. On the other hand, because the odors become more unitary in their combinations, new and emergent perceptual properties might give rise to unpredictable affective reactions. Those who have had much to do with the creation of perfumes know that this may be true. Our mode of presentation of the odor stimuli did not permit complete mixtures of stimuli such as we find in perfumes, however. We presented one odor to each nostril, so that any resulting mixture had to be a cortical one as in the usual psychological or physiological study of mixtures of odors. To allow the vaporous stimuli to mingle before striking the sensitive surfaces of the olfactory organs leaves us in doubt as to just what kind of a stimulus we are applying, *i.e.* as to just how much chemical action might have occurred before the vapor reaches its goal. Our combinations of odor were perhaps not so different from combinations of color after all. There was a spatial separation of the two components, except that olfactory localization is much inferior to visual localization, and in addition to this there was the opportunity for cortical blending. This type of blending was of course not possible in the case of the combinations of color.

The odors that were used in the combinations were selected after some preliminary

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* Accepted for publication March 16, 1932.

[1] J. P. Guilford, The prediction of affective values, this JOURNAL, 43, 1931, 469-478.

[2] *Op. cit.*, 478.

trials. It was desired to select single odors that extended over the whole affective scale at approximately equal intervals. Eight *Os*, including 5 men and 3 women, were used throughout the experiments. Two were graduate students in psychology, 1 an instructor, and the remaining 5 were undergraduates. For the type of observations demanded, we could see no difference between the practiced and the unpracticed *Os*. The *Os* first gave judgments, using a scale of values, upon 24 single odors. After two ratings by each *O*, we pooled the 16 judgments upon each odor and thus found its approximate affective value on our scale. From the 24 odors we then selected two groups, one of 6 and one of 7 odors, which extended evenly over the whole range of affective values found for the 24 odors. Each one of the 6 odors was combined with each one of the 7 in order to make the 42 combinations which were used in this study.

The experimental procedure with the single odors was as follows: A double olfactometer was set up in the opening of a large screen which separated *O* from *E*. Two bent glass tubes were inserted in the olfactometer. *O* adjusted the ends of the tubes in his two nostrils. *E* uncorked a bottle containing an odorous substance and inserted the other ends of the glass tubes into it. At a signal from *E, O* executed a long slow inhalation, and held his breath until a judgment was reached. This was found to be necessary, for in the very first experiments it was found that *O*'s judgment might change during an exhalation because the quality of the odor itself seemed to change. *O* had the following instructions before him in typed form:

> After you are adjusted to the olfactometer, let *E* know by giving a signal. When *E* says 'Ready' he is preparing the stimulus. When he says 'Now' you are to inhale with one long slow inhalation. Keep your inhalations constant from one experiment to another in speed and in length, and hold your breath until your judgment is made. You are to judge the pleasantness or unpleasantness of the odor on the 9-point scale given below. If you cannot make a judgment, you may ask *E* to repeat the experiment. You will be allowed not more than two trials. Scale of values: (9) greatest possible pleasantness, (8) extremely pleasant, (7) moderately pleasant, (6) mildly pleasant, (5) indifferent, neither P nor U, (4) mildly unpleasant, (3) moderately unpleasant, (2) extremely unpleasant, (1) greatest possible unpleasantness."

This form of scale was adopted for two reasons. Conklin has found that with unpracticed *Os* a 9-point double scale is best.[3] We did not make the unpleasant steps negative and the pleasant ones positive because Young has shown how this breaks the continuum into two parts and produces a bimodal distribution of judgments.[4] The more accurate methods, of paired comparisons and of ranking, are out of the question in the judgment of odors for very obvious reasons. The use of the rating scale method in this connection has given rather good results for a number of investigators in the past.[5] We have not taken the trouble to test the reliability of the measurements which were used in this study. Inspection of the same *O*'s repeated judgments upon the same single odor or combination of odors shows extremely few deviations of more than one point on the 9-point scale. Furthermore, the values assigned by different *Os* to the same odor show surprisingly few deviations greater than one point. By pooling the judgments of 8 *Os*, individual peculiarities of preference are cancelled out and something approaching an 'objective' measure of affective value of the stimuli is reached. In order to obtain the best measure of 'objective' affective value, one should follow the technique of Beebe-Center;[6] *i.e.* find the extent to which each *O*'s judgments are correlated with the objective values and weight them accordingly. Our pools of judgments may be regarded as unweighted pooled observations of the objective affective values.

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[3] E. S. Conklin, The scale of values method for studies in genetic psychology, *Univ. of Ore. Stud.*, 2, 1923, (no. 1), 1-36.

[4] P. T. Young, Studies in affective psychology, this JOURNAL, 42, 1930, 17-27.

[5] See esp. J. H. Kenneth, A few odor preferences and their constancy, *J. Exper. Psychol.*, 11, 1928, 56-61.

The procedure in judging the combinations is best described by quoting the instructions that were given to the *Os*.

> The experiments will be conducted just as before. When *E* says 'Now' you are to inhale deeply and slowly and at a uniform rate, and to make a judgment before you exhale. Try to give your attention to the whole odor-combination. Try to take it as a single total sensory experience. If you should fail to do this, be sure to tell *E* what kind of a combination you experienced and what features of it determined your judgment. Add any other comments that you think are significant.

The 13 odors that were chosen for combination will be found listed in Table I. Their mean affective values are given as are the mean affective values of the 42 combinations. In the final results we have shifted the zero point of the scale to the indifference point so it may be seen at a glance whether a particular affective value is P or U.

The order of presentation of the combinations was counterbalanced so that no odor was given twice in succession, and so that the same odor when it was repeated in the same hour would be presented to the two nostrils alternately. Each *O* judged every combination twice, on two different days. The order of presentation on the second day was just the reverse of that on the first day, so that each pair of odors could be reversed in arrangement in the two nostrils. After all the judgments of combinations had been completed, the 13 single odors, again mingled with the entire list of 24, were judged two more times by every *O*. There were, therefore, 32 judgments of each single odor and 16 of each combination. In both cases the glass tubes of the olfactometer were changed after every judgment. After every experimental hour they were deodorized and sterilized by boiling for 3 min. in water and drying under air currents. *O* always rested for about 2 min. after making a judgment. A large electric fan kept the air of the room continually stirred up and kept up a circulation of fresh air entering the room.

Table I shows that there is a close agreement between the values of the single odors and those of the combinations. The 6 odors at the top and the 7 at the side are arranged in descending order, and it will be noted that the trend of the whole table is in the same direction. This is perhaps not very surprising. But can we formulate any mathematical equations by which one can predict the value of a combination from the values of its two components? And what form will these functions take? In the case of color combinations, previously cited, the best laws of combination were probably linear, although due to the small number of measurements and to their peculiar distribution there was some uncertainty about this point.[7] In the case now under consideration, the component stimuli were especially chosen for the occasion, with affective values ranging widely over the whole scale and evenly distributed. The number of combinations is also relatively large and we should be able to come to a more definite conclusion concerning the law of summation of affect.

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[6] J. G. Beebe-Center, General affective value, *Psychol. Rev.*, 36, 1929, 472-480.

[7] Guilford, *op. cit.*, 472-475.

We first examined the values of combinations with each of the 13 single odors in turn. For each odor at the top of the table there are 7 values of combinations. These may be taken as functions of the 7 values of the single odors at the left hand side of the table. Likewise, for every odor at the left there are 6 values of combinations with the 6 odors at the top of the table. When these 13 sets of paired values are plotted, they are all obviously linear, and when we remember that they extend over a wide range of the scale we may assume that they would be linear throughout. The slopes of the lines, however, are not exactly the same. They range from 0.21 when carbon bisulphide is the standard or constant member of the combinations to 0.90 when musk is the constant one. They are all less than 1.0, which indicates that the total range of values for the combinations with any constant odor is never as great as the range of the variables that are combined with it. Since carbon bisulphide was the most unpleasant odor of the lot and gave the smallest slope, and since musk was among the pleasant group, it occurred to us that the slope of the line might be proportional to the value of the constant member of the combinations. A correlation between the two things, slope of line ("b" in the table) and the affective value of the single odors was only 0.30. This insignificant relationship was almost entirely due to the very small slope in the case of carbon bisulphide alone, and we shall point out later that this is probably due to the fact that the carbon bisulphide tended to dominate all the combinations in which it appeared. The effects of dominance and fusion will be discussed later.

TABLE I

Affective Values of the 13 Odors and of the 42 Combinations

(b = slope of line; a = y-intercept, *i.e.* indifference value; and r = coefficient of correlation)

		pepper-mint	cinna-mon	musk	ben-zene	creo-sote	stale cheese	b	a	r
	*	2.31	1.19	.69	−.56	−1.53	−2.19			
orange	1.84	2.19	2.00	1.43	.00	−.06	.44	.56	1.01	.93
anise	1.78	1.75	1.75	1.37	.12	−.38	.56	.47	.87	.86
almond	.97	1.81	1.12	.62	−1.06	−.94	−.63	.69	.16	.93
bergamot	.59	1.00	.93	.37	−.19	−1.19	−1.07	.58	.00	1.00
benzoin	−.22	1.00	.31	.25	−1.38	−2.00	−1.88	.79	−.60	.93
amber	−1.06	1.25	.25	−.88	−.94	−1.07	−1.82	.63	−.53	.96
carbon bisulphide	−3.34	−2.50	−2.94	−3.37	−3.13	−3.16	−3.69	.21	−3.13	.85
b		.78	.88	.90	.56	.52	.79			
a		.87	.42	−.10	−.98	−1.32	−1.22			
r		.91	.97	.99	.92	.75	.98			

* In this line and in this column are given the affective values of the single odors.

Another item of interest in Table I lies in what we have called the 'indifference values' of the single odors. These are given in the column and the line marked "a" in the table. They are merely the y-intercepts of the 13 linear functions. We have called them 'indifference values' because they are the values of the combinations for given standard odors when the other odor lies at the indifference point. We had no single odors with that exact value, of course, but from the linear functions we can predict what the effect of such odors would be. One striking thing about these indifference values is that with one exception, and that is in the case of benzoin, they are all nearer the zero point on the scale of values than the standard odor itself. That is, the combination of any odor, whether P or U, with a second indifferent one is always less P or U, as the case may be, than that odor is alone. This may be due to the manner of presentation of the stimuli. It may be more pleasant or unpleasant to have the same P or U odor in both nostrils than to have it in only one with an indifferent one in the other nostril. This fact may also be a cause of the slopes all being less than 1.0. Or, both of these facts may be due to one of our former conclusions, that the combined affect is not an algebraic sum but a weighted mean of its two components.

One fact that might be of great practical interest, is that these indifference values are almost perfectly correlated with the scale values of the standard odors. The coefficient is 0.96. Thus, by merely knowing the affective value of any given odor, one could predict very accurately its indifference value or the value of any indifferent odor combined with it. Now if one could also predict in a similar way the slope of the 'function of combination' for the same odor, one would have all the necessary information for the prediction of the combined affect from its components. By 'function of combination' is meant the linear equation for any given standard odor, with the value of the combination as the dependent variable and that of the second odor combined with the standard as the independent variable. Unfortunately, there is no way of predicting the slope of the line from the value of the standard odor, as we have shown in a preceding paragraph.

The last row and column of Table I give the coefficients of correlation between the variables in the different functions of combination. There is little of interest in them, except that they are all high enough to justify our acceptance of the linear relationships. Three of them are conspicuous for being below 0.90; these are for creosote, anise, and for carbon bisulphide. No reason is suggested for this, and in view of the very small number of measurements, 6 or 7 for each function, it is hardly worth one's while to look for any reason.

We began by raising the question as to whether the law of combination found for color stimuli would also hold for odor stimuli. This law of combination distinguishes between the more pleasing component, P, and the less pleasing component, U. It was admitted that the distinction might be somewhat artificial, but it seemed to be the only one that could be made. If we apply the same assumption to our data with odors, we find the law fully verified. In the previous paragraphs we have found that when one member of a number of combinations is held constant, the value of any combination is a linear function of the variable members. Therefore we need only consider first powers of P and U in using least square treatments. The necessary constants were computed by the method of least squares with the three variables, C (the affective value of the combination), P, and U. The following equation resulted: $C = .54\,P + .69\,U - .21$. The coefficient of multiple correlation between the predictions from this equation and the observed affective values is 0.967. The standard error of the estimate is 0.39. Both these last two values are uncorrected for the number of variables and for the number of observations. The number of variables is so small and the number of observations is so large that the correction would be very small.[8]

This result can be regarded as a complete vindication of the assumption of this type of combinational law for the summation of affects. This is all the more significant as a law of affect, in view of the fact that it holds for stimuli in two sense departments, visual and olfactory, and also the fact that the types of combination, perceptually or physiologically regarded, are not quite the same in the two cases. Some concern was expressed at the beginning of this report about the fact that combinations of odors are phenomenally different from combinations of colors. It was suggested that the combinations of odors are more unitary, due to the fact that the two components are less separated spatially. This may account for the result that the multiple correlation coefficient is slightly higher here in the case of odors than it was in the case of colors.[9] If R^2 measured the percentage of total determination of the combined affect by its components, that determination amounts to about 94% in the case of odors. The remaining 6% might be due to errors of measurement, but we believe that we have found another factor to account for at least a part of it. That factor is the lack of fusion of the odors, or the dominance of one odor by another.

In the previous article it was suggested that this remaining percentage, (K^2), could be used to indicate the 'Gestalt effect' or the emergent properties arising from the combination. In the case of colors, with women *O*s especially, this remainder was reasonably large. Certainly, if the law of combination was correct, there were other unknown factors at work, and these factors might be attributable to the totality of the experience and not to the components themselves. In the case of odors, however, very little remains to be accounted for. This would mean that if there is a 'Gestalt effect' it must have little potency. But the factor of dominance, which we think is the most important remaining factor, is by its very nature a Gestalt effect. There could be no dominance if there were not a relationship between two odors. It might be logically argued, of course, that the property of dominance exists in the dominant odor and the property of recessiveness exists in the odor that is dominated. Carbon bisulphide, for example, tended to dominate everything with which it was paired. Cinnamon tended to dominate nothing else and was dominated by many of those with which it was paired. On the other hand, a clear order of combination is not easy to make out. It does not always follow that an odor *A* that dominates odor *B* which in turn dominates odor *C*, will also dominate odor *C*. We make these statements with some hesitation, because our *O*s were untrained in the observation of odors, and because the number of reports of actual dominance are limited.

In the instructions for judging the combinations, *O* was told always to render a judgment as to whether the two odors fused well, and if they did not, to state which one of the pair was dominant. Out of a total of 672 judgments, only 182 were reported to be cases of dominance; the remaining 490 pairs of odors were fusions. The number of cases of dominance is too small to give any adequate idea of the relative dominance of the various odors. We were able to select 17 specific combinations of odors, however, in which it was reasonably apparent that one odor of the pair tended to dominate over the other one. Each pair had been judged only 16 times. In the 17 cases of which we speak, the one odor dominated over the other one from 4 to 10 times out of a possible 16. This gave us only a rough criterion for distinguishing between the dominant and the recessive member of a combination so that we might find an empirical weighting for the two in the prediction of the value of the combination.

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[8] M. Ezekiel, *Methods of Correlation Analysis,* 1930, 121.

[9] Guilford, *op. cit.,* 474.

Applying least square procedures again, this time using the dominant member (D) and the recessive member (R) as the two independent variables, we found a regression equation for the prediction of C, the value of the combination. The equation is as follows: $C = .85\ D + .21\ R - .40$. The coefficient of multiple correlation, uncorrected, is 0.947, and the standard error of estimate 0.51. When the dominant and recessive members of combinations are weighted as in the above formula, two thirds of the predictions will fall within 0.51 of the observed values. It is significant that the dominant member is weighted about four times as much as the recessive one. If this factor of dominance were taken into account in addition to the distinction between the P and the U components, predictions of combined affects from odors might be made almost perfectly, within the margin of errors of measurement. And if the degree of dominance were considered, the predictions would probably be even more accurate. For example, carbon bisulphide, which dominated almost every combination in which it appeared, was almost the sole determiner of the values of those combinations.

SUMMARY AND CONCLUSIONS

This study was undertaken in order to test a law of affective summation previously proposed for the affective values of combinations of color. Two groups of single odors, 13 in all, were selected from a list of 24 odors because they ranged evenly over a wide extent of the pleasant-unpleasant continuum. The affective values of the 13 single odors and of 42 combinations of them were determined by means of rating scale judgments. The following conclusions were reached after a functional analysis of the results.

(1) For any given odor, the affective value of the combination of a second odor with it is a linear function of the affective value of the second odor. This holds true without regard to the degree of fusion of the two odors.

(2) The affective value of a combination of odors can be predicted rather accurately from the affective values of its two components, the more pleasing and the less pleasing component.

(3) Again, as in the case of combinations of color, the affective reaction to a combination of odors is apparently not an algebraic sum of the affects belonging to the single odors, but it is rather a weighted mean of the two.

(4) When one odor of a pair is dominant, it carries much more weight in determining the affective value of the combination than does the recessive odor. The relative weight of the dominant odor probably depends upon the degree of dominance.

APPARATUS

THE HATHAWAY GALVANIC APPARATUS AS AN INSTRUMENT OF RESEARCH

By T. W. Forbes, Columbia University

Since the advent of Hathaway's "psychogalvanometer" a number of investigators have made use of it for research. Among these are Hathaway,[1] Porter,[2] Porter and Copeland,[3] Ruckmick,[4] Smith,[5] Crosland,[6] Dysinger,[7] and Misbach.[8]

As a means of demonstrating the galvanic phenomenon the apparatus has proved its worth, since the reaction is quickly, easily, and directly exhibited. For research purposes, however, the fact that a galvanic response is indicated is not sufficient. Where statistical comparison is made of average deflections called out by certain types of stimuli, it is especially important that the deflections be accurately proportional to actual galvanic responses. If the apparatus introduces any distortion, this distortion must be known and corrected for before statistical treatment of deflection scores will yield valid data concerning galvanic reactions. Davis has questioned whether the proportionality of deflection to reflex remains constant in the Hathaway apparatus.[9] Our own experience led us to question whether the current through the subject used by the Hathaway apparatus could be sufficiently well standardized. It seemed, for these reasons, that it would be worth while to determine the actual physical characteristics of the apparatus.

Electrical and physical characteristics. In determining these characteristics we substituted in place of the human *S* the simplest possible combination of physical resistances and capacities which could validly be used. This combination consisted of a variable resistance and a variable capacitance connected in parallel, as shown in Fig. 1. This figure also shows the Hathaway galvanic circuit in simplified form in order to indicate the exact relationship of *S*'s circuit to that of the apparatus. It has previously been shown by Philippson,[10] Cole,[11] McClendon,[12] and Strohl,[13] that this combination is too simple to represent the electrical characteristics of human skin accurately. It does represent, however, a rough approximation.

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[1] S. W. Hathaway, A comparative study of psychogalvanic and association time measures: A new psychogalvanic apparatus, *J. Appl. Psychol.*, 13, 1929, 632-646.

[2] J. P. Porter, The psychogalvanic compared with other measures of emotional reactions, *Psychol. Bull.*, 26, 1929, 156-157.

[3] J. P. Porter and H. A. Copeland, Association and psychogalvanic responses during early adolescence, *Proc. IX Int. Cong. Psychol.*, 1929, 349-350.

[4] C. A. Ruckmick, Emotions in terms of the galvanometric technique, *Brit. J. Psychol.*, 21, 1930, 149-159.

[5] F. O. Smith, Variations in the galvanic response, *Psychol. Monog.*, 41, 1931, (no. 187), 142-152.

[6] H. R. Crosland, Objective measurements of emotion, *Univ. Oregon Pub.*, 1, 1931, (no. 3), 153-196.

[7] D. W. Dysinger, A comparative study of affective responses by means of the impressive and expressive methods, *Psychol Monog.*, 41, 1931, (no. 187), 14-31.

[8] L. E. Misbach, Effect of pitch of tone-stimuli upon body resistance and cardiovascular phenomena, *J. Exper. Psychol.*, 15, 1932, 167-183.

[9] R. C. Davis, Factors affecting the galvanic reflex, *Arch. Psychol.*, 18, 1930, (no. 115), 12-13.

[10] M. Philippson, Les lois de la resistance électriques des tissues vivants, *Bull. (Sc.) Acad. roy. belgique*, 7, 1921, 387-403.

Having set up our circuit as shown in Fig. 1, we adjusted the Hathaway apparatus according to the mimeographed directions accompanying the apparatus, for each of a series of values of resistance (R) and capacitance (C). According to the directions this setting should standardize the current through the external circuit which includes the resistance and capacity of the body of the *S*. In order to see whether such standardization actually did take place we read the actual current passing in this external circuit on a rectifying milliammeter with a scale range of 0–1 ma. The physical resistance used was a General Radio Co. 111,110 ohm decade resistance box. The resistance windings in this box are of Ayrton-Perry and tape type, designed to be practically non-inductive and to have a very small distributed

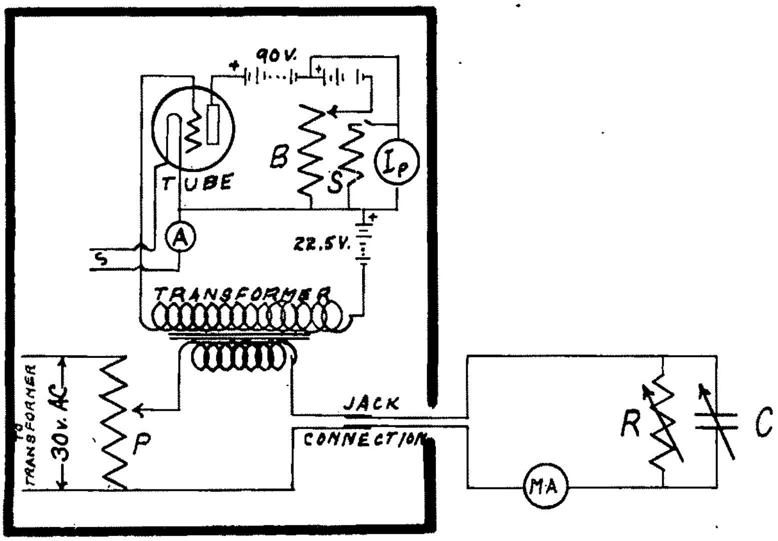

FIG. 1. SHOWING SIMPLIFIED DIAGRAM OF HATHAWAY CIRCUIT WITH R AND C IN PLACE OF S

Heavy line = Hathaway cabinet; A = filament ammeter; B = meter zero adjustment rheostat; C = variable capacitance; Ip = Hathaway milliammeter; MA = milliammeter; P = potentiometer; R = variable resistance; S = shunt.

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[11] K. S. Cole, Electric impedance of suspensions of spheres, *J. Gen. Physiol.*, 12, 1928, 29-36; Electric phase angle of cell membranes, *ibid.*, 15, 1932, 641-649.

[12] J. F. McClendon, The permeability and the thickness of the plasma membrane as determined by electric currents of high and low frequency, *Protoplasma*, 3, 1927, 71-81.

[13] A. Strohl, La conductibilité des organismes vivants en courant alternatif: II. Schémas équivalents aux conducteurs organiques, *J. Radiol. et d'Électrol.*, 15, 1931, 537-552.

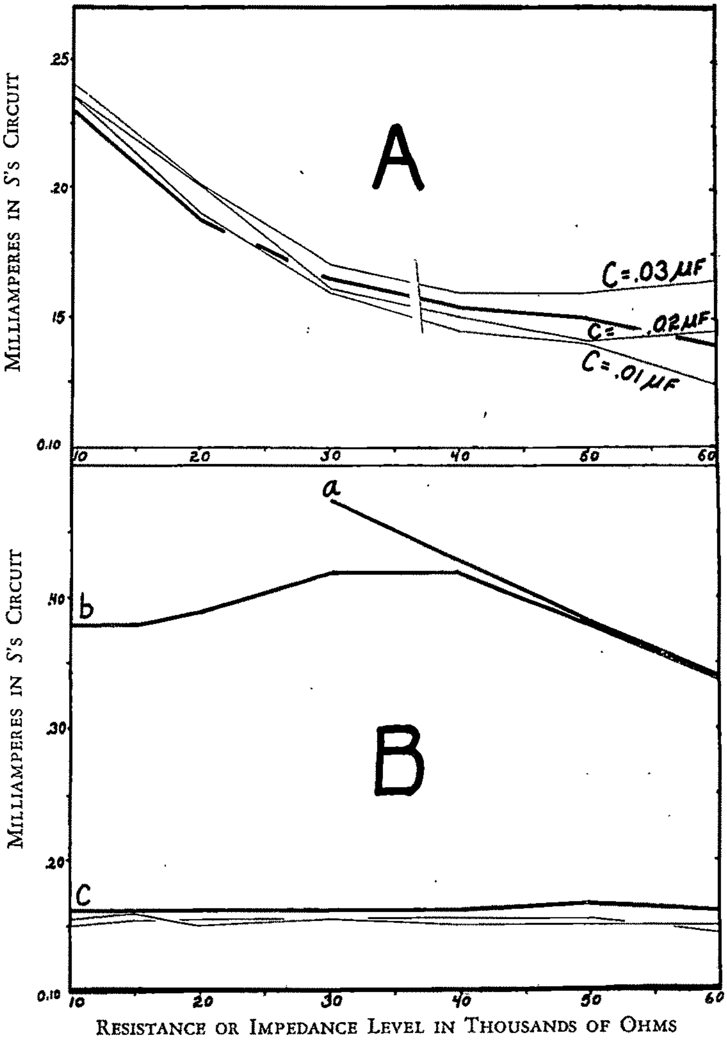

Fig. 2. Showing Current through S for Various Levels of Body Resistance and Capacity

A = Apparatus No. 1; B = Apparatus No. 2; Heavy lines = current at approximated levels of body impedance; light lines = at various resistance levels and a given capacitance level. a = 1.0 amperes filament current; Ig = 0; no standardization possible. b = 2.05 amps. filament current; standard current in grid circuit = .05 ma.; indicating meter (Ip) could not be set to zero. c = 2.05 amps. filament, Ip (Shunted) = .05 ma. as standard.

capacity. The capacitance used was a General Radio Co. 0.001–0.1 microfarad decade capacitance.

In order that our results might be of general application, we have determined the characteristics both of an instrument assembled by Hathaway and of a Marietta Apparatus Co. outfit. These will be designated as Apparatus No. 1 and Apparatus No. 2. As far as we know, no other manufacturers have marketed this apparatus.[14]

The current flowing in *S*'s circuit is plotted against the level of resistance in Fig. 2. The heavy line in each half of this figure represents the value of current through *S*'s circuit when resistance alone was substituted for *S*, and therefore represents roughly the effect of impedance change.[15] The light lines show how this current varied with both resistance and capacitance in *S*'s circuit. The upper curve in Fig. 2A, for instance, shows that with 0.03 microfarad in circuit and a 10,000 ohm resistance in parallel with it, the current through the external circuit (through *S*) was 0.24 milliamperes, when the apparatus was set as directed in the accompanying instructions. The same curve in Fig. 2A shows that, with the same capacitance and 20,000 ohms in parallel with it, 0.20 milliamperes flowed through the circuit at the standard setting. It will be noted that the general shape of the curves is similar for the pure resistance and for the parallel combination of resistance and capacitance. Crossing of the curves probably indicates slight variations of A.C. main line current not taken care of by the ballast tubes of the apparatus. It is quite apparent from these curves that as resistance or impedance in *S*'s circuit varied from 60,000 to 10,000 ohms the current through *S*'s circuit varied roughly from 0.13 to 0.24 milliamperes for Apparatus No. 1. *In other words, the setting of the meter of this variety of apparatus at 0.06 milliamperes does not satisfactorily standardize the current in the external circuit.*

Fig. 2B shows similar curves for Apparatus No. 2. When the standardizing milliammeter on Apparatus No. 2 was thrown into the grid circuit we found it necessary to adjust the filament current to approximately 2.0 amperes instead of 1.0 ampere as stated in the directions, if we were to have the milliammeter record at all (curves a and b of Fig. 2B). Incidentally the same value of filament current (approximately 2 amperes) was necessary for sensitivity.

Curve b in Fig. 2B indicates that use of the milliammeter in the grid circuit for standardization purposes was generally unsatisfactory. Curve c shows that by using the shunted standardizing milliammeter in the plate circuit set to 0.05 milliamperes as a standardization index or by using the unshunted meter set to zero with the 'bucking out' control (B in the apparatus directions), a satisfactory constancy of current in *S*'s circuit was obtained. The latter method is recommended by Hathaway for maintaining the standardization after an initial adjustment has been obtained. Apparatus No. 2 was therefore superior in this respect to Apparatus No. 1.

Proportionality of deflection. In some studies it is desirable to compare the amplitude of the galvanic skin reaction of various *Ss* to the same stimuli or of a given *S* to various stimuli. Now, if the apparatus produces any distortion in the

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[14] The C. H. Stoelting Co. sold several instruments assembled by Hathaway but does not now make them.

[15] Since resistance equals impedance when there is no capacitative or inductive reactance.

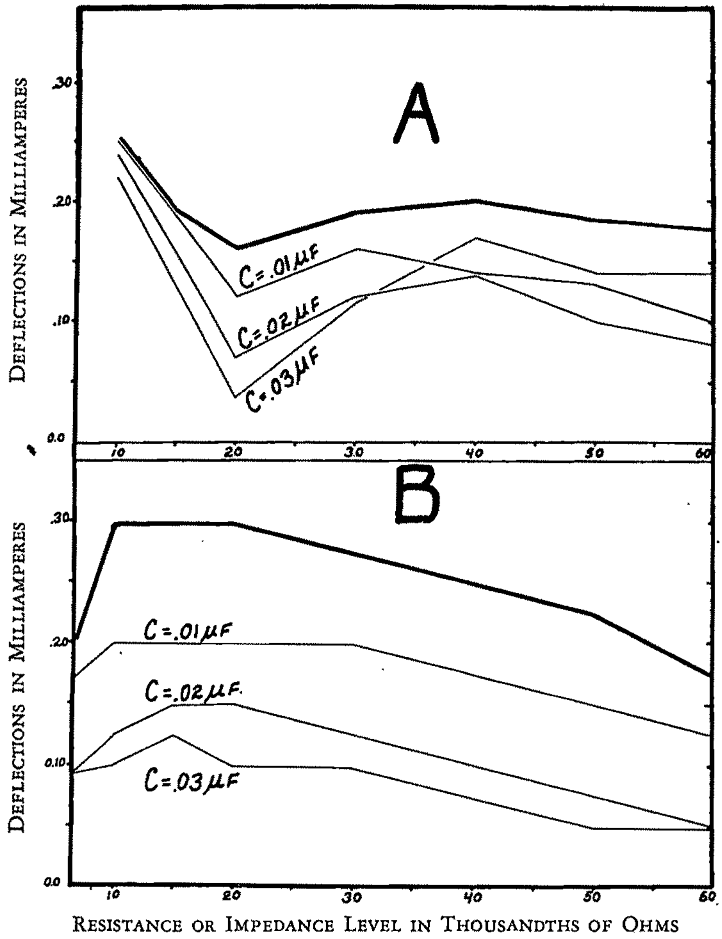

FIG. 3. SHOWING MAGNITUDE OF DEFLECTION PRODUCED BY 2000 OHMS CHANGE AT VARIOUS LEVELS OF BODY RESISTANCE AND CAPACITANCE

A = Apparatus No. 1; B = Apparatus No. 2. Heavy lines = for impedance approximation; Light lines = for resistance at given capacitance values.

amount of deflection, such distortion should be known and corrected. We were, therefore, interested in the relationship between recorded deflection and actual change in the impedance of *S*. (Impedance is in alternating current measurements, the counterpart of resistance in direct current measurements.)

In order to determine the relationship of amount of deflection to amount of impedance change, we again used the circuit shown in Fig. 1, standardizing both Apparatus No. 1 and No. 2 in the optimum manner for each type. Since McClendon and Hemingway[16] have shown that both resistance and capacity of the skin vary on stimulation and since each of these produces a deflection on the Hathaway apparatus, it was desirable to vary each and also to vary their resultant, *i.e.* impedance.

Fig. 3 shows the deflections in milliamperes which were produced by a reduction of 2000 ohms resistance for Apparatus No. 1 and No. 2. The heavy lines represent determinations made by varying resistance alone, and therefore represent an approximation of the effect of impedance variations (see note 2 above). The heavy line in Fig. 3A indicates that with 10,000 ohms in the circuit, a reduction of this resistance to 8,000 ohms gave a deflection of 0.25 milliamperes on the indicating meter (Ip) of Apparatus No. 1. Each of the light lines represents the amount of deflection for the same resistance change (minus 2000 ohms) at each level of resistance when the capacitance was kept at a given value; *i.e.* 0.01, 0.02, 0.03 microfarads, respectively. Again it will be noted that the introduction of capacitance does not change the general form of the curve but does change its general level somewhat.

From these curves it will be seen that a given value of impedance or resistance change (minus 2000 ohms) of the parallel combination used in place of *S* gave very different amplitudes of deflection at different values of total resistance level. This means that with Apparatus No. 1 and an *S* whose body resistance is 20,000 ohms an actual reduction of 2,000 ohms in his resistance would result in smaller deflection than the same resistance change in an *S* whose level of body resistance is 10,000 ohms or one whose resistance is 30,000 ohms.

Fig. 3B shows similar curves for the amount of deflection produced by a reduction of 2000 ohms in Apparatus No. 2. A comparison of A and B of Fig. 3 shows that the magnitude of deflection from the two pieces of apparatus varies in a dissimilar manner. For both Apparatus No. 1 and No. 2 the deflection produced by 2000 ohms change varied in amplitude from 0.10 to 0.30 milliamperes as total resistance varied from 60,000 to 10,000 ohms.

It occurred to us that the deflection might be proportional to the percentage resistance change in *S* rather than proportional to actual resistance change. Fig. 4 demonstrates that this is emphatically not the case. In this figure the impedance curves of Fig. 3 have been replotted. Impedance change has been expressed as percentage of total impedance; and deflection has been put in terms of percentage of maximum deflection (that obtained at 10,000 ohms) for each piece of apparatus.

If the deflection were proportional to percentage of impedance change a given percentage change would always add the same amount of deflection. This would appear as a straight line sloping toward the upper left of the figure. Such a line appears only for Apparatus No. 1 between 10,000 and 20,000 ohms level.

The apparatus, therefore, produces a deflection which varies in proportionality to the absolute change or to the percentage change of impedance involved in the galvanic reflex. Now, if this variation of amplitude is to be corrected for, we must know the actual body impedance at the time of reaction. The Hathaway apparatus gives no such measure. We could make the correction on the basis of a *preliminary independent* determination of skin impedance *if* the level of *S*'s skin impedance remained the same during the course of the experiment.

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[16] J. F. McClendon and Alan Hemingway, Variations in the polarization capacity and resistance of the skin, *J. Gen. Physiol.*, 13, 1930, 621-626.

Skin impedance, however, falls during the course of an hour as is shown by Fig. 5. The curves show the course of skin impedance or resistance of 3 *S*s during the course of an hour. Determinations of impedance were made by alternately placing *S* and the decade resistance across the electrode connections of the Hathaway apparatus. The substituted resistance which gave the same standard current on the Hathaway milliammeter (Ip) as that with *S* in series, was used as his body *impedance. Resistance* determinations were made on an A.C. bridge balanced by means of resistance and capacity in parallel using a 0 to 1 rectifying milliammeter as a null indicator. All curves for these *S*s show a drop of skin impedance or resistance during the course of an hour; the drops being from 10,000 to 50,000 ohms in extent. Obviously, then, we cannot apply a correction to the Hathaway readings unless we make a *series* of impedance determinations during the course of the experimental period.

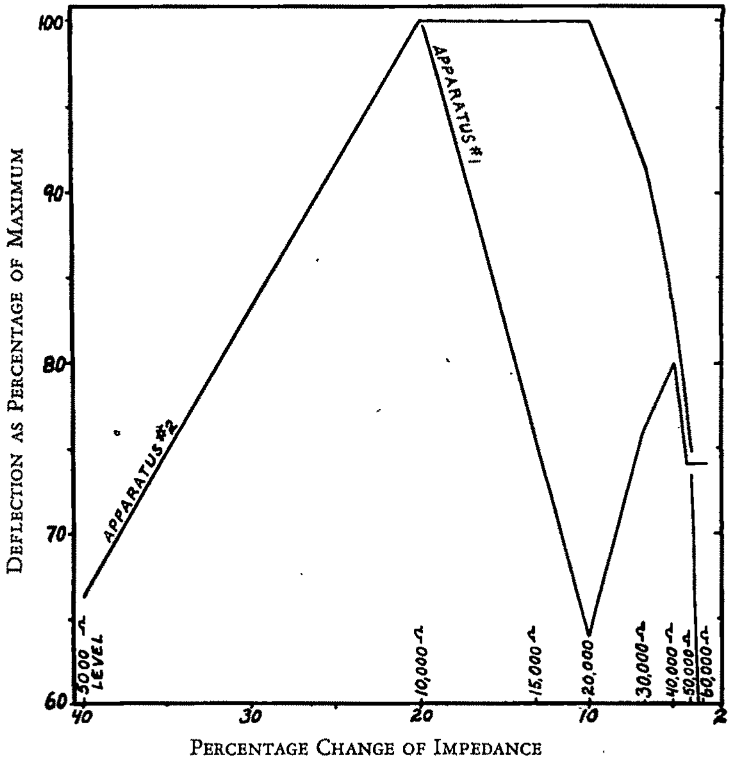

FIG. 4. SHOWING RELATION OF DEFLECTION TO PERCENTAGE CHANGE OF IMPEDANCE

Deflections shown by the heavy lines of Fig. 3A and B are re-plotted in terms of percentages.

In order to make such a series of determinations, a microammeter might be used in series with *S* and Apparatus No. 1 calibrated by use of a physical impedance somewhat in the manner we have used. Current through *S* would give a rough index of body impedance and therefore of apparatus sensitivity. Since current through *S* remains constant for Apparatus No. 2 when standardized, this method could not be employed here. The manufacturers could perhaps revise the circuit to give a measure of resistance in some such fashion as Davis has done with a D.C. vacuum-tube circuit.[17] If such determinations are not made and Hathaway scores are used uncorrected, ordinary statistical treatment of amplitudes will be invalid. Fig. 6 indicates how seriously the fall of body resistance may affect the amplitude of deflections. The resistance curve (a) for *S* 3 of Fig. 5 has been used, to construct Fig. 6, since it is intermediate in level and steepness. The amount of distortion is the vertical distance of each curve from its respective horizontal (dotted) at any given time.

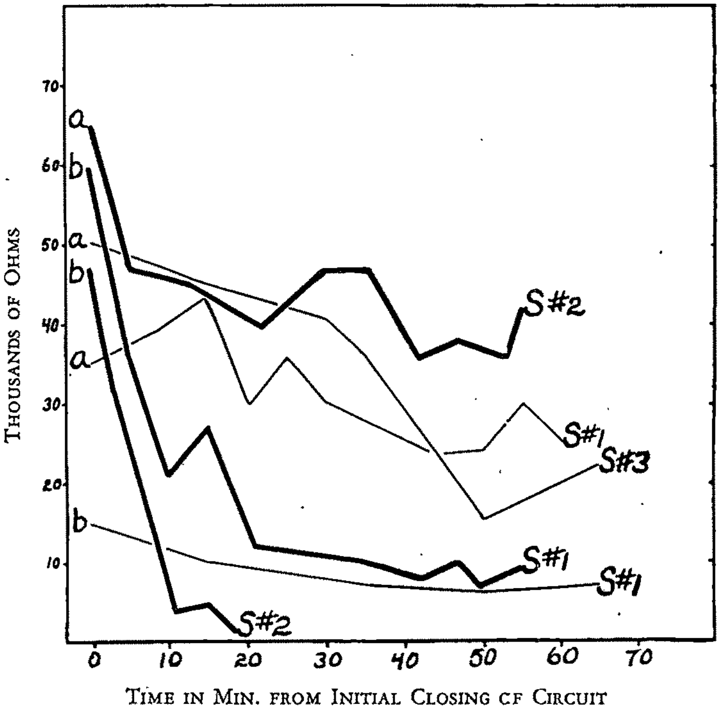

FIG. 5. SHOWING VARIATION OF SKIN IMPEDANCE AND RESISTANCE OF 3 *S*s DURING ONE HOUR

Heavy lines = approximate impedance value; Light lines = resistance by bridge with 0.12 to 0.14 ma. through S. a = using small; b = using large Hathaway electrodes.

In Fig. 6 actual deflection is plotted against actual resistance change (from apparatus curves of Fig. 3). Plotting percentage change only exaggerates the distortion. The curves show rather dramatically that the same galvanic reaction would produce very different deflections for this *S* during the course of an hour. It is clear that an increased deflection may mean a greater galvanic skin response (G.S.R.), but it may indicate merely a lower level of body impedance or even (for Apparatus No. 1 especially) an increased level (cf. Fig. 3).

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[17] *Op. cit.*

The labor of calibration and computation involved in applying a correction may make it more convenient to eliminate the Hathaway apparatus and use deflections and impedance determinations from a properly designed A.C. or D.C. bridge. Darrow,[18] for instance, has designed a very convenient, sensitive and well standardized D.C. bridge for psychological research measurements. Many investigators in the physiological field have used the A.C. bridge (cf. Landis).[19]

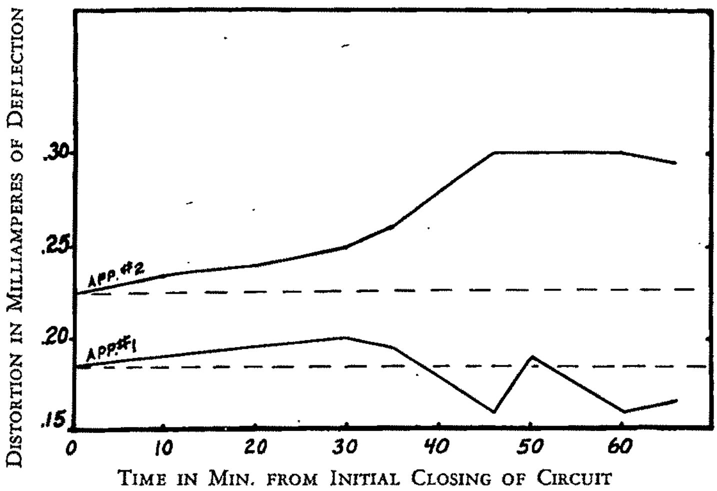

FIG. 6. SHOWING DISTORTION OF GALVANIC RESPONSE DUE TO CONTINUOUS DROP OF BODY IMPEDANCE

Computed for *S*3, Fig. 5, on the basis of Fig. 3A and B. Distortion is represented by vertical distance of each full line from its corresponding dotted line.

In general, therefore, the Hathaway galvanic reflex apparatus is of value for demonstrating the G.S.R. with alternating current but, as at present produced, should not be used for research requiring comparison of deflections except in some all-or-none fashion.

SUMMARY

Recent studies using the Hathaway apparatus and the apparent growing popularity of the circuit, together with detailed statistical treatment of deflections therefrom, as in Crosland's study, have led us to determine the actual characteristics of the circuit.

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[18] C. W. Darrow, Uniform current for continuous standard unit resistance records, *J. Gen. Psychol.*, 6, 1932, 471-478.

[19] C. Landis, Electrical phenomena of the skin (galvanic skin reflex), *Psychol. Bull.*, 29, 1932, 693-752.

(1) The characteristics were determined by putting in place of the subject a parallel combination of non-inductive and non-capacitative resistance and a decade capacitance (Fig. 1). Current through the subject's circuit was measured by means of a rectifying milliammeter, and deflections were noted for varying resistances and capacitances (R and C, Fig. 1).

(2) One Hathaway instrument made by each of two manufacturers was checked. To the best of our knowledge no other varieties have been made available.

(3) After setting the apparatus according to directions for standardizing of current through *S*, the actual current through *S*'s circuit was read. For Apparatus No. 1 the current through *S*'s circuit varied from 0.13 to 0.24 milliamperes as total resistance varied from 60,000 to 10,000 ohms. Constancy of current was therefore not obtained. For Apparatus No. 2, following the manufacturer's directions did not give satisfactory operation or standardization of current. A constant current in *S*'s circuit was obtained, however, by using a 2.0 ampere reading on the filament meter instead of 1.0 as directed, and by using the milliammeter in the plate circuit only.

(4) Both makes of apparatus showed a variation of deflection of from 0.10 to 0.20 milliamperes when a given impedance change (minus 2000 ohms) was made at different levels of total impedance from 60,000 to 10,000 ohms. The size of deflection was proportional neither to actual resistance change nor to the percentage of resistance change in *S*'s circuit.

(5) Three *S*s showed a drop of total resistance of from 15,000 to 50,000 ohms when attached to the apparatus or to an A.C. Wheatstone bridge during the course of one hour.

(6) It is therefore impossible to tell whether an observed increase of deflection represents an increased galvanic skin reflex (G.S.R.), or a change in the level of body resistance, unless a measure of total skin resistance level is also made at the time of the G.S.R. No facilities for such measures are at present supplied with the instrument. The manufacturers might be able to revise it to include such a measure somewhat as Davis has done with a D.C. circuit.

(7) The Hathaway apparatus is suitable for demonstration purposes, but, as now put out, should not be used where magnitude of deflection is desired as an index of magnitude of G.S.R.[20] Circuits are available for research use as designed by Darrow and Davis for D.C. and by various physiological investigators for A.C.

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[20] The manuscript of this article was read by Dr. S. W. Hathaway. He agrees to the advisability of pointing out the character of the apparatus and calls attention to the fact that at the end of his study, he advocated the use of *frequency* of response without regard to quantitative value (*op. cit.*, 645).

IMPROVEMENT AND STANDARDIZATION OF THE PURSUITMETER TECHNIQUE

By ALVHH R. LAUER, Iowa State College

The pursuit techniques of Miles,[1] Koerth,[2] and Weiss and Renshaw,[3] have had a rapid spread in interest. Such devices, originally designed for measuring sensori-motor learning, have been found applicable to studies of genetic behavior as well as to problems in the clinical and abnormal fields. With further advances in construction of synchronous motors the possibilities of improved types of instruments have become much greater.

While studying the relative influence of various factors of integrated response mechanisms the writer found need for an extension of the pursuitmeter equipment. Certain other tests were modified, assembled, and attached to the pursuitmeter as described in a previous study.[4] The wiring diagram is shown in the same paper.

The original instrument used by Weiss and Renshaw employed a spring handle stylus.[5] This had some disadvantages and a rigid stylus was tried. It proved equally unsatisfactory since *S* could ride the electrode in such a way as to slow down the motor and coast along with it. The kinaesthetic sensitivity of some *S*s seemed to increase their learning in a spurious manner. Others would barely touch the electrode and thus change the nature of the problem.

To offset these tendencies the writer devised the electrode and stylus shown in Fig. 1. The core of the electrode (K) is insulated from the cup (J). The latter two are wired in parallel with separate counters on recording circuits. When the stylus point (B) breaks contact it may either move off to one side or it may slide down over the oiled electrode and register an error by touching the cup (J). The elbow joint (*3*) prevents riding. The stylus is rounded to constitute the section of a sphere. It is kept lubricated by a fine oil. The shield (F) standardizes the distance of the hand from the electrode. A tripod support (D) carries the added weight of the cup and other accessory apparatus.

This is only one aspect of the changes introduced. A floating type of electrode (*5*) is attached by the nut (A) to the arm (*1*). The brass bracket (*5*) is located so that the hole (I) is directly over the electrode (K). The small part of the stylus (C) is held in the hole and must be kept off the side or an error is registered. If the stylus rides the side the motor stops. This constitutes a second type of serial

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* Developed during the tenure of a National Research Council Fellowship held by the author at the Ohio State University 1929-30 under the direction of Dr. A. P. Weiss. Credit is due Mr. Leon Goldstein for assistance in carrying out the study.

[1] W. R. Miles, A pursuit pendulum, *Psychol. Rev.*, 27, 1920, 361-376; The pursuit meter: An apparatus for measuring the adequacy of neuromuscular coördination, *J. Exper. Psychol.*, 4, 1921, 77.

[2] W. Koerth, A pursuit apparatus: Eye-hand coördination, *Psychol. Monog.*, 31, 1922, 288-292.

[3] A. P. Weiss, and S. Renshaw, Apparatus for measuring changes in bodily posture, this JOURNAL, 37, 1926, 261-267.

[4] A. P. Weiss, and A. R. Lauer, The psychological principles in automotive driving, *Ohio State Univ. Contrib. Psychol.*, 1930, (no. 11), 1-57.

[5] *Op. cit.*, 262 f.

response primarily involving eye-hand coördination. The contact-stylus technique involves more kinaesthetic and tactual cues. This is shown by the fact that induced visual defects do not noticeably affect the contact or ordinary pursuit performance, while they do affect the floating type.[6]

A third type of serial response mechanism is measured by adapting the Scott 3-hole test to the pursuitmeter. It moves in an irregular pattern in such a way as to vary the stereotyped performance of the stationary target. In experiments so far conducted a rate of 132 contacts a minute was established by use of a metronome and buzzer. The technique has great possibilities for investigating the 'personal tempo,' or optimal working speed of the individual in relation to learning.

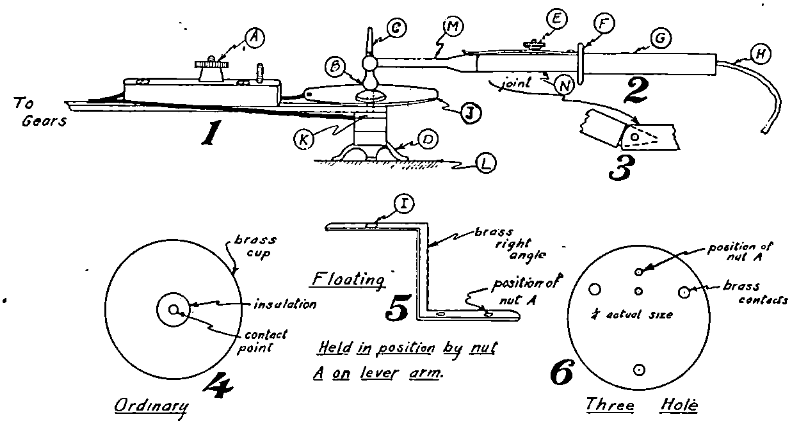

FIG. 1. SHOWING DETAILS IMPROVED

DESCRIPTION OF THE DIFFERENT MECHANISMS

(A) Ordinary. The diameter of the cup is $2\frac{15}{32}$ in. gradually curved to a saucer shape with the circumference $\frac{3}{16}$ in. higher than the center. The hole in center is $\frac{3}{8}$ in. with insulated plug $\frac{1}{2}$ in. in diam. at top, held tight by a $\frac{1}{8}$ in. pin (K) with lock nut under bracket support (D). This makes contact with the connection attached to (*1*), but is insulated from the lever.

The pin serves as the electrode. The 3 feet of bracket (D) describe an equilateral triangle $1\frac{3}{8}$ in. on a side. This runs on a fiber plate (L) of sufficient size to insure firm support at any extreme of the cycle-pattern. The distance from the plate surface to the extreme high point of the electrode is $1\frac{9}{32}$ in. The cup (J) is wired by a separate connection to another counter in parallel with that from the electrode. The ball (B) of the stylus is used as a means of contact, completing the circuit through the return connection (H). Thus both lost contacts and slips to the cup (J) are recorded.

(B) Floating. Bracket (*5*), which attaches at (A), has two holes $\frac{4}{32}$ in. in diam. At (1) a hole $\frac{1}{2}$ in. in diam. is used as a guide for the small end of the stylus (C). The latter is $\frac{7}{64}$ in. in diam. A contact of (C) and the bracket at (I) operates

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[6] Weiss and Lauer, *op. cit.,* 96.

a relay which stops the motor in addition to registering an error. This eliminates riding. The bracket is made of silver plated brass 1¾ in. wide by 3/64 in. thick. The horizontal section at (I) is 1¾ in. long; the one at (A) 1 29/32 in. long; while the vertical section is 1 11/16 in., and is quickly detachable.

(C) Three-hole. The fiber disk (*6*) is ⅛ in. thick and 8 in. in diam. The two small holes attach it to the arm (*1*). The targets are silvered brass ½ in. in diam. and describing an equilateral triangle 5¼ in. on a side.

(D) Stylus. The stylus is 6⅜ in. over all from the tip of (B) to the attachment of (H). The dual stylus contacts (B) and (C) are attached by a screw tapped through the stylus handle. Contact (B) is 9/32 in. in diam. at the largest point and extends 11/32 in. Sections (M), (N), and (G) are 2 3/32, 1 19/32, and 2½ in. respectively. The small diam. at (M) is 3/16 in., while that at (N) is 5/16 in. (G) is covered by a soft rubber sleeve which fits up snugly to the fiber shield at (F). This standardizes the distance of the hand from the electrode. The stylus (*2*) is hinged at (*3*) by an elbow joint. The tension required at (B) to break the joint is approximately 70 grm. It can be regulated by the screw and spring at (E). Not more than 200 grm. pressure can be placed upon it.

A tapping device was also attached to the instrument, and by manipulating switches speed of movement records can be taken very easily. The timing device attached to the pursuitmeter is quite accurate within the limits of error of the type of synchronous motor used. This is accomplished by star-wheels. The technique was the same as that used by Ream,[7] except that 10 sec. tapping periods were given.

All contacts on the instrument are either silver or are heavily plated with silver. Silver oxide is a conductor, and therefore poor connections due to corrosion are practically eliminated.

The reliability of the tests as measured by correlating the mean of the first and fourth trials with the mean of the second and third trials was found to be as follows, after correction by the Spearman-Brown formula: ordinary pursuit, R = +0.939; three-hole pursuit, R = +0.895; floating pursuit, R = +0.906.

The correlation between the three types of pursuit movement is not high. An *r* of +0.435 was obtained between the ordinary and the floating type of performance. Between the 3-hole and the ordinary, *r* is —0.149, while between the 3-hole and floating, an *r* of —0.148 was obtained. The high score, in terms of contacts made on the ordinary and floating performance, is low score. The actual correlations are therefore slightly positive.

The three types of pursuit movement, together with the speed of movement tests, make an ideal battery for measuring sensori-motor and serial learning. In general the performance is as follows: first trial with the preferential hand, second trial with the non-preferential hand, third trial with the non-preferential hand, and final trial with the preferential hand. This order was adopted to offset fatigue and practice effects. By starting with the preferential hand *S* does better and is more strongly motivated. Each cycle is about 2 min. in length.

The pursuitmeter proper was built according to the specifications of Weiss and Renshaw.[8] A 1/100 h.p. constant speed motor was used instead of the ordinary type.

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[7] M. J. Ream, The tapping test: A measure of motility, *Psychol. Monog.*, 31, 1922, (no. 140), 290-319

[8] Weiss and Renshaw, *op. cit.*

It is rated at 1800 r.p.m. instead of 1750. On 60-cycle alternating current the variation is slight unless overloaded. These techniques have been used for three years in our laboratories and they are relatively trouble-proof.

Validity of pursuit measurements has been covered by various writers cited in the literature. The reliability is very satisfactory for most experimental purposes. Because of slight differences in construction it is suggested that each instrument be standardized after it has been in operation for a short time by giving it to a representative group of *Ss*.

A THYRATRON-TUBE CHRONAXIMETER

By Frederick C. Thorne, Columbia University

The present apparatus has been developed in an attempt to produce a rectilinear electrical wave of a variable short duration for the purpose of testing the excitability of nerve and muscle preparations in chronaxy measurements. In most of the previous experiments, Lapicque's condenser discharge method has been used in preference to the Weiss ballistic rheotome or the Lucas pendulum because of its greater accuracy and ease of manipulation. A rectilinear impulse is obtained from the rheotome and the pendulum, as compared with the exponential curve derived from condenser charge or discharge. Although Lapicque has published results indicating that the three methods are essentially equivalent,[1] there remain several considerations in favor of the use of a square wave. Since the duration of the condenser charge or discharge is a function of the resistance across which it flows, it would seem that much precaution should be taken to insure a constant value for the resistance of the stimulated tissue from experiment to experiment.

A circuit giving a rectilinear wave has been made possible by the recent development in the General Electric laboratories of the Thyratron vacuum tube. The FG-17, described as a mercury-vapor, grid controlled rectifier, is recommended here because of the small voltage drop from filament to plate and also because once the grid has lost its control, it exerts no further appreciable influence on the plate circuit until control is returned by breaking the plate circuit.

In Fig. 1, at the starting point, there is a negative grid bias on Tubes I and II, this bias preventing any flow of current in the plate circuit of either tube. When switch 1 is closed, the grid bias is effectively removed from Tube I instantaneously, while the condenser C_1 delays its removal from Tube II for a short period of time dependent upon the value of C_1. The plate resistance of the tubes is very low, and it will be seen that there is a relatively high resistance in the plate circuit of Tube I, while there is none in the plate circuit of Tube II. When Tube II is in operation, it will short-out Tube I, since most of the plate current from the common B-battery will follow the path of least resistance through Tube II. Tube I therefore operates only in the interval between the closing of the switch 1, and the flick-on of Tube II which has been delayed by the condenser C_1. In any individual set-up, Tube II may not entirely short-out Tube I; this should be remedied by bucking out the remaining current with a small battery placed in the plate circuit of Tube I.

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[1] L. Lapicque, *L'excitabilite en fonction du temps*, 1926.

The stimulating current is led off the potentiometer R_6, through which current flows only when Tube I is in operation. Switch 3 is opened only when it is desired to measure the voltage being led off from the potentiometer R_6, Tube II being inoperative in this case, therefore allowing Tube I to remain on long enough to permit a reading of the voltmeter V. A voltmeter of high internal resistance is specified in this circuit in order that its inclusion will not seriously alter operations. Switch 2 is customarily kept closed except when it is desired to return the apparatus to the starting place by cutting off the plate circuit and restoring grid control. The value of the condenser C_2 is not exact; however, it should be as large as possible since this condenser is included to counteract the influence of any stray inductance that may be present in the circuit. In operation, it is advisable to leave all resistances except R_6 in a fixed position, since any change in these will cause the condenser to charge at a different rate. However, if the settings

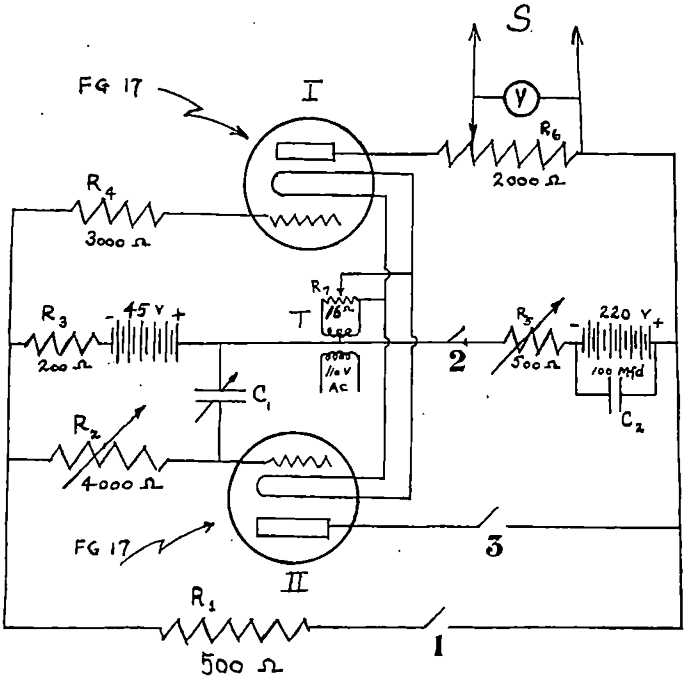

FIG. 1. CIRCUIT DIAGRAM FOR THYRATRON-TUBE CHRONAXIMETER

R_1, R_3, R_4 = fixed resistances of the non-inductive type (Lavite Resistors recommended); R_5, R_6, R_7 = variable tubular rheostats of ordinary type; R_2 = a calibrated resistance box of the decade variety; C_1 = a calibrated decade condenser (No. 219 G manufactured by the General Radio Co.; C_2 = a fixed electrolytic condenser (Dubilier Electric Co.); V = a Jewell D.C. voltmeter, pattern No. 2, 0 to 150 volt range (specified because of its high resistance; and T = the filament transformer (must deliver at least 10.8 amperes at 2.5 volts).

of C_1 should not provide fine enough time intervals, the value of R_2 may be lessened, making possible an indefinitely small delay.

The filaments should preferably be operated from an A.C. transformer capable of delivering 10.8 amperes at 2.5 volts. This voltage should be accurately maintained using a filament voltmeter connected directly across the filament terminals of one of the tube sockets. The return leads from the plate and grid circuits should be connected as indicated to the midtap of the filament transformer.

The length of the stimulating impulse may be calibrated theoretically or at any single stimulation. It will be seen that by connecting a simple ballistic galvanometer in the plate circuit of Tube I, the quotient of the voltage on the load and the reading of the swing of the galvanometer will give the time of the impulse, assuming the wave to be rectangular.

AN APPARATUS FOR RECORDING FETAL MOVEMENT

By L. W. Sontag and Robert F. Wallace, Antioch College

In preparation for the investigation of the activity of the human fetus during the later half of its intrauterine life, we were confronted with the fact that no entirely satisfactory apparatus has been devised for recording the fetal movements. Since we wished to study the variation of movement from day to day, its relationship to fatigue, excitement, periods of the day, etc., it was necessary to record continuously for relatively long periods. There should be a minimum of inconvenience in the preparation for the taking of each record, and infrequent need for changing the record-sheets. Since there are movements of the maternal abdomen, particularly respiratory movements, which are in no way associated with fetal movement, it was essential that the records differentiate these from the fetal movements. Finally, it is necessary to record simultaneously motion of all parts of the maternal abdomen if the record is quantitatively to estimate the movements of the fetus.

Of the experimenters who have worked with fetal movement, two describe apparatus for recording it. Hicks used an apparatus which he described as "an instrument like a cardiograph" which he "tied lightly on the abdomen of a person lying on the back."[1] The cardiographic record on kymograph paper showed the respiration-wave upon which was superimposed the fetal movement. From the records which Hicks published, it is difficult for us to differentiate the fetal movements from others. He observed that movements of the mother's limbs will give the same record as fetal movement and must be watched. He also points out that tympany may cause similar movements of a coil of distended intestine. Ray used a 2-in. tambour, placed diaphragm-side-down on the mother's abdomen and fastened in place with adhesive tape.[2] In this method the tambours rise and fall with the

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* From the Samuel S. Fels Research Institute, Antioch College, Yellow Springs, Ohio.

[1] J. B. Hicks, On recording the fetal movements by means of a gastrograph, *Trans. Obstet. Soc. London,* 22, 1880, 134.

[2] W. S. Ray, A preliminary report on a study of fetal conditioning, *Child Dev.,* 3, 1932, 175-177.

breathing movements of the abdomen and Ray states that the record showed only fetal movements. He used three tambours to record the movement in three parts of the abdomen, whereas Hicks used but one. In our opinion, however, the area of the three 2-in. tambours was not large enough to record a dependable part of the fetal movement. Recording in both of these studies was by kymograph.

The apparatus devised by the authors, with the aid of advice and suggestions of the late A. P. Weiss, is constructed as follows. It consists of four rubber sacs, each forming one quadrant of an ellipse, and together covering the entire abdomen. The four sacs are enclosed in a cloth bag to keep them in proper spatial relation to one another. Over the cloth bag containing the four sacs is placed a previously prepared thin plaster-of-Paris cast. A binder of unbleached muslin is passed around the patient's back and the ends laced together over the cast, thus holding the cast and air sacs firmly against the abdomen. (See Fig. 1.)

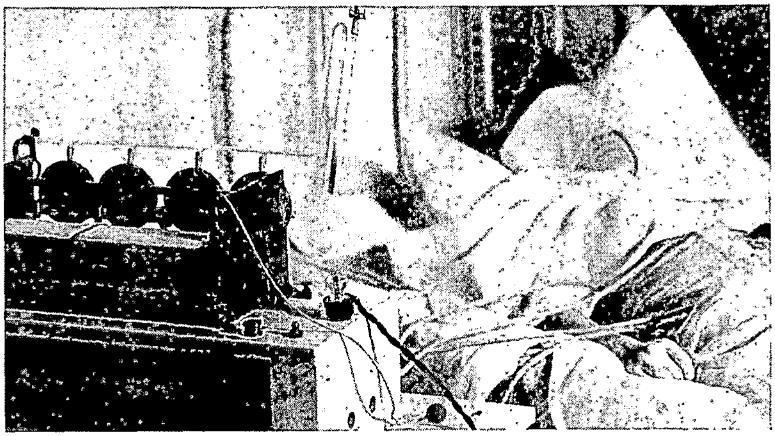

FIG. 1. APPARATUS FOR RECORDING FETAL MOVEMENT

The recording part of the apparatus (Fig. 2) consists of (1) four 3-in. tambours with diaphragms so mounted that there is no radial strain, (2) four pens each actuated by a short rod connected to the center plate of each diaphragm and passing out through the brass tambour-housing, (3) a 3-in. motor-driven brass drum which drives the paper through the machine, and (4) a brass table over which the paper is pushed and upon which the pens rest.

When the apparatus is in use, the four sacs, which have been connected with the four tambours, are inflated with air at a pressure of 60-120 mm. of water. Movements of any of the four quadrants of the patient's abdomen produce displacement of the respective tambour diaphragms and consequently excursions of the pens. As the brass drum revolves, it carries past the pens a strip of paper 14 in. in width fed from a roll suspended beneath the drum. The speed of the drum and the tension of the tambour diaphragms are both variable. The apparatus also includes valves and manometer necessary for obtaining the desired pressure in the system. Abdominal breathing of the mother is recorded by the even swinging of the four pens in unison. A fetal movement is indicated when the pens shift their relative positions in such a way that one or more are going in one direction at the same time that others are going in the opposite direction.

The apparatus has been in use for a total of about 60 hours during the past three years. It is being used for the study of fetal conditioning as well as in an attempt to ascertain the relation between fetal movement and maternal fatigue, emotional states, hunger, and ductless gland-secretion. Its advantages are briefly as follows: (1) it uses ordinary paper and barographic pens rather than the smoked paper of the kymograph and so is capable of furnishing a continuous record for several hours; (2) it is propelled by an electric motor rather than a spring driven motor; (3) it has a recently developed tambour in which the tension of the membrane is obtained by a spring instead of radial tension in the membrane itself; (4) variation of the pressure against these diaphragms may be obtained by changing and regulating the compression springs; (5) the recorder has four tambours and is therefore capable of recording, independently and simultaneously, movement of four variables; and (6) with minor changes, it may be used for recording such other factors as heart beat, respiration, and tremors.

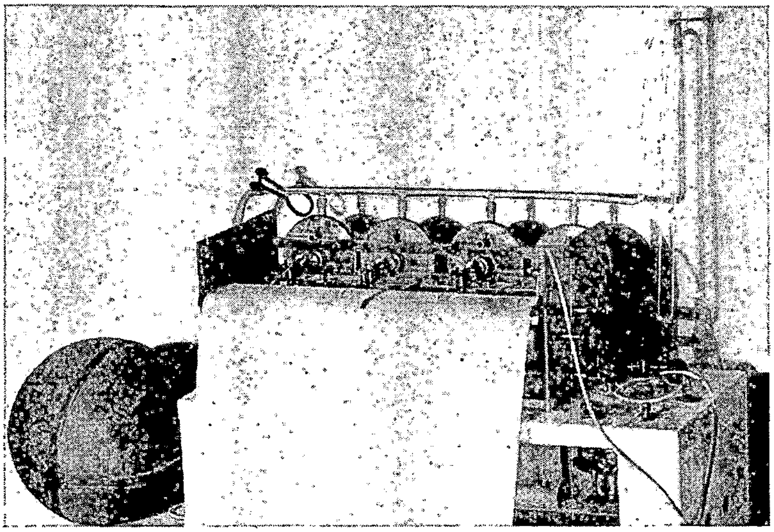

FIG. 2. RECORDING APPARATUS

A RESET DIAL FOR CHRONOSCOPES

By L. F. BECK, Brown University

Several dial-type chronoscopes, such as the Hipp, the Cenco impulse counter, and under certain conditions the Dunlap, carry no convenient reset mechanism for the sweep-second hand. This limitation forces one to note the initial as well as the final readings of the hand during a series of measurements. Such an operation is both cumbersome and time-consuming. This objectionable feature of the dial-type chronoscope can be overcome by supporting an adjustable, secondary, graduated dial on the face of the instrument. Prior to a measurement the dial is turned so that its graduated zero line is directly over the tip of the hand of the chronoscope. After the reaction the number of units is read directly from the dial; whereupon the dial again is turned until its zero line is coincident with the hand of the chronoscope.

The following detailed description is confined to such a reset dial adapted to the Cenco impulse counter. This instrument is selected as an illustrative model, since it can be used either as a timer or a counter, and therefore is applicable to many kinds of reaction-experiments.[1] Specifications for the dial and its support are given in the assembly sketch, Fig. 1. The support is fashioned from $\frac{1}{16}$ in. sheet brass. The diameter of its aperture is $2\frac{25}{64}$ in., $\frac{1}{64}$ in. larger than the bottom rim of the dial. The support is attached by spreading the two $\frac{1}{4}$ in. springs until they encircle the cylindrical metal cover of the counter. The support then is pushed down until the felt (or rubber) pad rests upon the top surface of the counter.[2] The dial is cut from $2\frac{3}{4}$ in. brass rod. Its outer top rim carries a no. 5 knurl. The bottom rim is $\frac{5}{32}$ in. deep, which is $\frac{3}{32}$ in. more than the thickness of the support calls for. It is considered expedient to preserve this dimension in order that, if one wishes,

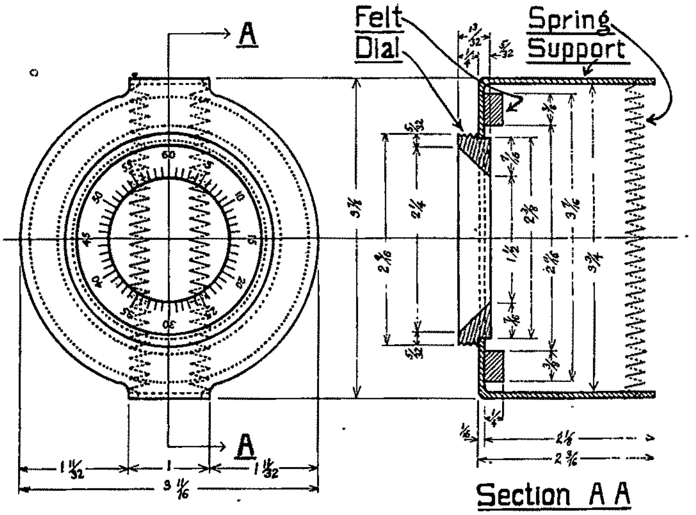

FIG. 1. ASSEMBLY SKETCH OF RESET DIAL FOR THE CENCO COUNTER

the dial can be mounted on a panel, and the counter concealed below it in a cabinet. The graduations of the dial correspond to those on the face of the counter. The cutting of the graduations on the inside bevel can be done with a Brown and Sharpe milling machine equipped with a spiral head. Both the dial and the support are chromium-plated, and in this respect they match the metal cover of the counter.

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[1] For a discussion of some of the numerous uses and a description of this counter, cf. W. R. Miles, Correlation of reaction and coördination speed with age in adults, this JOURNAL, 43, 1931, 377.

[2] It has been found that the dial support can be centered more accurately above the face of the counter if it is provided with four legs instead of two as depicted in the assembly sketch in Fig. 1.

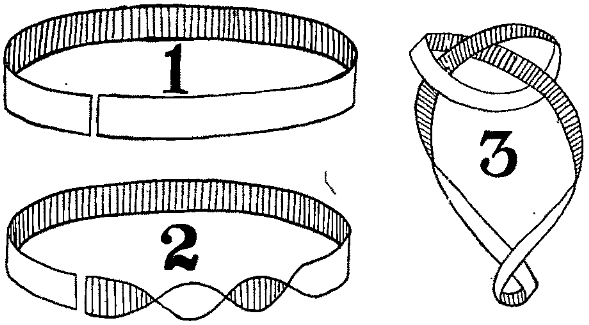
The dial does not obscure the graduations on the face of the counter. In fact, readings can be taken from the graduations on the dial or on the counter with equal facility. The dial, however, embodies the advantageous reset principle.

APPARATUS NOTES

An Experiment in Reasoning

The following problem, which has an unknown history, may be used as material for an experiment or exercise in reasoning.

Cut a small strip of paper, about 2 cm. wide and 25 cm. long; bend the strip so that the two short ends come into contact (Fig. 1); turn one end three times 180° about the longitudinal axis of the strip (Fig. 2); paste the two ends together; cut the strip to half its width by cutting along the middle of its entire length. A strip will result containing a well-formed knot (Fig. 3)—one that customarily results from inserting a free end through a prepared loop. The problem is to anticipate the result and to explain it after it has been obtained.



Figs. 1-3. Showing the Strip, the Twists, and the Knot

Similar phenomena were demonstrated at spiritistic meetings in Germany 50 years ago and were given as evidence of the supernatural. Shortly after, Simony wrote a book upon the formation of knots in an enclosed circular band;[1] and more recently Höfler and Witasek included the problem as an exercise in their manual of psychological experiments.[2] Nothing further is known about the history of the problem. Its discoverer is obscured by its spiritistic background.

University of Southern California — Norma V. Scheidemann

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[1] Oskar Simony, *In ein ringförmig geschlossenes Band einen Knoten zu machen,* 1881.

[2] Alois Höfler and Stephen Witasek, *Hundert psychologische Schulversuche,* 1903 (Exper. No. 38), 17.

NOTES AND DISCUSSIONS

A Critique of Crosland's "Measurement of Emotion"

The development of the "ready made psychogalvanic apparatus," such as that of Hathaway,[1] makes possible the use of the technique by any experimenter, regardless of his technical knowledge of the apparatus or of this field of research. This is an undoubted advantage for general classroom work, but in research it has led to fictitious results and fallacious interpretations.

Certain errors in technique and interpretation in the most elaborate study which has utilized the Hathaway galvanic apparatus will serve as an example of this scientific error. The following critical remarks apply equally to other studies in which similar errors appear. In a preceding paper, Forbes has presented the electrical characteristics of the Hathaway apparatus.[2]

Crosland's study[3] presents a detailed statistical treatment of results which are in great part invalidated by certain fundamental errors in his use of the apparatus. For instance, a great deal of space and statistical analysis is devoted to evaluation of "absolute" versus "relative" readings. These two types of readings are described in his monograph as "observing merely the whole deflection and recording the farthest point reached by the needle," and as "observing and noting down the differences between the beginning point and the termination-point of the deflection."[4] An understanding of the apparatus would have eliminated consideration of "absolute" readings or else would have resulted in a different interpretation of the statistics obtained with them.

Crosland was greatly surprised to find the absolute readings more reliable statistically, but later discarded them on the basis of low validity. He suggested that the difference between the two measures represented "persisting reaction."

Interpretation on the basis of a "persisting reaction" seems a bit ambiguous. If this means an incomplete recovery of the skin in returning to a normal impedance level due to a too short time interval between stimuli, the technique is open to question as will be shown below. On the other hand, if a gradual change of basic resistance level is meant, then the phenomenon is similar to the well known "rest curve" of direct current galvanic skin measures which is ordinarily corrected for by resetting the resistance units of the bridge or other measuring circuit. The Hathaway apparatus is provided with a potentiometer adjustment for this purpose and reference to Hathaway's work shows that his "absolute readings" started from approximately zero in each case.

(1) In Crosland's work this adjustment was evidently not used properly, *i.e.* the milliammeter needle quite evidently did not start from zero, for Crosland says,

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[1] S. R. Hathaway, A comparative study of psychogalvanic and association time measures, *J. Appl. Psychol.*, 13, 1929, 632-646.

[2] T. W. Forbes, The Hathaway galvanic apparatus as an instrument of research, this Journal, 45, 1933, 502-511.

[3] H. R. Crosland, Objective measurements of emotion, *Univ. Oregon Pub.*, 1, 1931, (no. 3), 153-178, 179-196.

[4] Crosland, *op. cit.*, 157.

"the beginning points of the deflections were the most variable."[5] Table I shows 8 scores from Table XIII of the monograph which have been picked to illustrate the fact that the adjustment was not properly made. Columns 1 and 2 give respectively the "absolute" and "relative" readings from Crosland's table. Column 3 is the average starting point of the needle which we obtained by subtracting the average "relative" values (column 2) from the average "absolute" (column 1). Column 3, therefore, represents roughly the adjustment of the meter. The adjustment levels computed, vary from approximately 0.10 to 0.80. Since the scale on the Hathaway meter runs from 0.00 to 1.00 ma., it is evident that these average adjustment levels represents a wide difference from *S* to *S*. Either the meter adjusting potentiometer was set differently for different *S*s, or else the initial drop of resistance level was allowed to go uncompensated. Forbes demonstrated a continuous drop of resistance in impedance level of 3 *S*s who were sitting quietly with as little in the way of stimulation as could be easily obtained.[6] There were wide individual differences even under these conditions. There is no evidence, therefore, that a similar drop in level can validly be referred to as a "persisting reaction" to association stimuli.

TABLE I

SAMPLE READINGS TO SHOW VARYING STARTING POINTS (METER ADJUSTMENTS) FROM CROSLAND'S TABLE XIII

	Subject No.	Non-significant			Significant			Post-significant		
		1 Absolute	2 Relative	3 Start	1 Absolute	2 Relative	3 Start	1 Absolute	2 Relative	3 Start
visual series	39	.106	.038	.068	.083	.050	.033	.079	.044	.035
	42	.860	.068	.792	.868	.099	.769	.860	.066	.794
	46	.574	.072	.502	.722	.185	.537	.645	.047	.598
	50	.130	.028	.102	.120	.052	.068	.122	.031	.091
oral series	10	.778	.075	.703	.759	.091	.568	.762	.085	.677
	12	.197	.070	.127	.261	.093	.168	.260	.074	.186
	14	.866	.075	.791	.893	.110	.783	.876	.093	.783
	38	.672	.072	.600	.714	.098	.516	.702	.097	.605

In any case, the effect would be to add a constant error to each individual's score. This constant, as will be seen from our Table I, is in most cases a large percentage of the *S*'s "absolute" score and for each individual is fairly constant (see Table I, Non-Significant, Significant, and Post-Significant), hence a high reliability coefficient based on an error of manipulation. The use of elaborate statistics on faulty data is, of course, rather pointless.

(2) A second criticism lies in the rate of presentation which introduced an uncontrolled variable. The automatic phonograph presentation allowed a maximum of 7.7 sec. interval between stimuli. Figures given by Davis,[7] and records in our laboratory, indicate that the total cycle of a galvanic reflex often requires longer than 8 sec. Evidence that such cases occurred in his study appears in Crosland's

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[5] *Ibid.*, 159.

[6] Forbes, *op. cit.*, 508ff.

[7] R. C. Davis, Factors affecting the galvanic reflex, *Arch. Psychol.*, 18, 1930, (no. 115), 1-64.

monograph, since "the P.G.R. reaction was usually in progress just at the instant when the next succeeding stimulus was being presented."[8] Therefore the effect on readings of the overlap is unknown and uncontrolled, and comparisons of magnitude of deflections are accordingly invalidated.

(3) In the third place, several factors tend to invalidate the statistical findings. Crosland's significant stimulus series were divided in such a manner that 15 of the 20 significant words fell in the last half of the list, thereby displacing words of the non-significant list. Therefore there were three times as many significant reactions in the last half of each stimulus series as in the first half. There were, on the other hand, slightly *less* than 50% of the total number of non- and post-significant reactions. Any factor which produced a progressive increase in the magnitude of deflection during the presentation of a list would, therefore, have increased the average of the significant reactions to a greater degree than the average of the non- and post-significant ones.

Such a factor would be expected to have operated. Davis has called attention to the fact that the deflection is known to increase with the current used, and Forbes has shown that a gradual drop of *S*'s body impedance may occur when the *S* is attached to the Hathaway apparatus. Forbes also showed that, in one form of the Hathaway apparatus, the current through *S* increased as body resistance decreased. Crosland obtained deflections to relevant stimuli which were significantly larger *statistically* than those from irrelevant and post-relevant. Since no check was made of the extent to which increased deflection was due to decreased body resistance and resulting increase of current through the body, we do not know how much of this difference represents characteristic reaction to the two types of stimuli. Until we do, the fact that the difference was statistically significant cannot be taken at face value in drawing conclusions.

(4) Furthermore, the *Hathaway apparatus itself* produces a distortion which makes comparisons of deflection magnitudes generally undesirable, for, as Forbes has shown, the proportionality of deflection to galvanic reaction not only differs, but it differs in a non-linear fashion. If, then, the body resistance of a given *S* changes during the sitting, our determinations indicate that the apparatus itself introduces a gradually changing (increasing for the most part) magnitude of deflection. This distortion by the apparatus makes the meaning of the greater deflections to relevant stimuli obtained by Crosland for any given subject still more ambiguous.

The same characteristics of the apparatus will also affect *individual differences* of deflection. If, for instance, one *S*'s body resistance is generally higher due to starting at an initial value of about 50,000 ohms while another's starts with an initial value of 30,000 ohms (see body impedance curves), then the distortion by the apparatus will affect quite differently the average scores for the two *S*s. It will, in general, produce a greater magnitude of deflection for the second *S* than for the first.

Hathaway apparently attempted to avoid comparisons between individuals on this account in correlating average scores for each word with reaction-times. Crosland criticizes Hathaway's technique on statistical grounds but in correlating the average deflections and average reaction-times of each person on the significant stimuli, Crosland neglects to take into account the factor we have just mentioned, *i.e.* the individual differences in amplitude due to different body impedance levels. He also neglects the fact that such differences would increase the reliability coefficient for his "relative" scores in a spurious fashion.

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[8] Crosland, *op. cit.*, 159.

Crosland mentions that larger differences of deflection occurred between different *S*s than between different stimuli, indicating that such widely different body impedance levels probably did occur.[9] His finding of practically no correlation of P.G.R. with average reaction-time for relevant stimuli may, therefore, be valid or it may indicate that the *S*s showing a given reaction-time score were equally distributed over the range of apparatus sensitivity as determined by body resistance.

The small negative relationship with the Thurstone Personality Scores is based on a similar correlation of average deflection for significant stimuli. The same criticism as above, again applies, and the coefficient may indicate a relationship to body resistance level rather than to galvanic reactivity.

In general then, the above factors, *i.e.* the unknown extent of overlap of reactions due to speed of presentation, the effect of different levels of body resistances, of *changing* body resistances, and of apparatus distortion, would tend to prevent the occurrence of a normal distribution of galvanic scores. Nevertheless, differences between means based on theoretically normal distributions are used in comparing visual, oral and phonographic presentations.

Moreover, the *actual* size of average differences ranged from 0.004 to 0.008 ma., the larger figure being but slightly greater than the probable accuracy of reading of the meter. In our experience this error is likely to be at least 0.005 ma. or about ¼ of one division on the meter. We therefore agree with Crosland that "they (the differences) are nowhere removed entirely from the possibility of being chance differences,"[10] and are surprised at their inclusion in the summary of results where it is stated that "small but evident differences accrued as between the three series."[11] We submit that the disturbing factors must be controlled, or else corrected for specifically by statistical means before such small differences can carry any weight. As Forbes pointed out in the previous paper, the Hathaway apparatus, *as at present supplied on the market,*[12] does not permit the application of such a correction.[13]

SUMMARY

In recapitulation we have made the following criticisms of Crosland's study which should be understood as applying to any of the other galvanic reflex studies in which similar Hathaway procedure was used.

(1) Based on poor understanding or manipulation of Hathaway apparatus.

(a) Interpretation of the "absolute" deflection as containing a "persisting reaction" seems both ambiguous and questionable in the light of the progressive drop of impedance level long known in direct current studies and reported for alternating current measurements by Forbes.

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[9] *Ibid.,* 187. [10] *Ibid.,* 169. [11] *Ibid.,* 176.

[12] Crosland says, "The galvanometer employed was the market variety put out by Hathaway" (*op. cit.,* 154).

[13] Forbes, *op. cit.,* 505ff.

(b) As pointed out by Forbes, when using the Hathaway apparatus, comparison of magnitudes of deflection is invalid, since there is a varying proportionality of deflection with changing body impedance, unless special provision is made for calibration and correction. No such correction is reported by Crosland.

(2) Based on previous skin reflex work and general considerations.

(a) The high reliability of "absolute" deflection readings was apparently due largely to faulty manipulation of apparatus.

(b) The use of a list of significant stimulus words unbalanced as to position in the series would tend to accentuate differences due to increasing magnitude of deflection as general resistance level falls.

(c) The rate of automatic phonographic presentation was so fast as to introduce an overlapping of reactions to a varying and unknown extent, thus further invalidating comparison of magnitudes.

(d) Actual differences between group average deflections ("relative") of from 0.004 to 0.008 ma. were used for drawing conclusions. The probable accuracy of reading is about 0.005 ma. For this reason and in the light of criticisms above, we question the significance of such small actual differences no matter what their relationship to the magnitude of a probable error based on a theoretical normal distribution.

(e) In correlating total deflections with total reaction time scores on Significant, Non-Significant, and Post-Significant, for each individual, the effect of differences of magnitude occurring with individual differences in body resistance has been overlooked.

(f) The tendency to a spuriously high reliability for "relative" galvanic scores which would result from these individual differences, has not been considered by Crosland.

(g) On the basis of the above considerations, the alternative that "emotional and attentional reactions came through the galvanometer undifferentiated . . . or that . . . time records are influenced by attentional and intellectual perturbations but the psychogalvanic reactions are not so influenced,"[14] must be viewed as without experimental foundation.

(h) The results of the "psychogalvanic" sections of Crosland's study may be briefly and correctly stated as follows: Word association-reactions are accompanied by galvanic reactions when the Hathaway alternating current circuit is used.

Columbia University

T. W. Forbes
Carney Landis

Intelligence and Handedness

A number of attempts have been made to relate intelligence and handedness. Binet and Vaschide, for example, obtained a significant correlation between dullness and ambidexterity;[1] and their results were corroborated by Smedley, who observed that dull children tend to be equally strong in both hands.[2] Schuyten likewise found a definite relationship,[3] and Carman has written that intelligent children exceed dull children "by an average of 3 kg. with the right, and 1 kg. with the left hand."[4] Barr has observed that "a test of grip is more serviceable than a test of language to diagnose mental status."[5] MacDonald, however, found no evidence to support these conclusions.[6] Yet Wallin, who has studied many epileptics and feeble-minded subjects, believes that "degenerates tend to be ambidextrous, and that dextrality and intelligence go together."[7] The controversial nature of the supposition of correlation is indicated in these studies on the problem.

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[14] Crosland, *op. cit.*, 182.

[1] A. Binet and N. Vaschide, Experiences de force musculaire et fond chez les jeunes garçons, *Année Psychol.*, 4, 1897, 15-63.

[2] F. Smedley, *Report of the Commissioner of Education*, 1902, i, 1095-1115.

Since every entering student at Bucknell University is given an intelligence test and, among other physical tests, a strength of grip test, advantage was taken of the opportunity to correlate for these older subjects the ratings in these two tests. The records of men students in two successive freshman classes (339 in number) were taken for study. The average age at the time of testing was 18.56 years. No other study of the problem has dealt with a group at this age-level.

The intelligence rating was determined by the American Council Psychological Examination, 1931 and 1932 editions, by converting the raw scores into percentile values as given in the official reports on the test results for the two years concerned. Strength of grip was measured by the Smedley hand dynamometer, according to the technique described by Whipple.[8] Wallin, it should here be noted, suggests the possibility of inaccuracy in the Smedley instrument;[9] hence a check was made to ascertain the reliability of the strength of grip measurements. Fifty cases were taken at random and tested one month after the physical examination; the coefficient of correlation between the first and the second scores was 0.87 ± .17. Consequently it was concluded that the technique gave reasonably accurate results for the purposes of this study.

The data were treated from three aspects. (1) A correlation was drawn between strength of grip in the right hand and percentile ranking in the psychological test. The coefficient between these variables computed by the Pearson method was only 0.02 ± .03—not a significant relation.

(2) The dynamometric score for the left hand was then compared with the intelligence score. Here also the coefficient was low, being —0.15 ± .04. There appeared to be no evidence to conclude that the strongest are the brightest, as Schuyten inferred from a study of children. This point may be illustrated by stating that the median dynamometric score in the right hand was, for those in the 90 percentile on the psychological test, 56.5 kg.; and for those in the 10 percentile, 58.1 kg. The superiority of 1.6 kg. of those in the low percentile is too slight to be significant. Similar results were obtained by comparing the median scores in each level of intelligence. Hence these findings support the conclusions of MacDonald rather than those of Barr and others.

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[3] M. C. Schuyten, Les variations de la force musculaire et le developpement intellectuel des élèves (Reported by A. Binet), *Année Psychol.*, 9, 1902, 448-449, from *Paedologisch Jaarboek*, 1902.

[4] G. M. WHIPPLE, *Manual of Mental and Physical Tests: Simpler Processes*, I, 1914, 106.

[5] M. W. Barr, *Mental Defectives*, 1910, 162.

[6] A. MacDonald, quoted by Whipple, *op. cit.*, 106.

[7] J. E. W. Wallin, *Clinical and Abnormal Psychology*, 1927, 499.

[8] Whipple, *op. cit.*, 101-2.

[9] Wallin, *op. cit.*, 499.

(3) The data were, in the third place, arranged to compare the index of dextrality with the intelligence ranking. The term "index of dextrality" is used by Whipple to indicate "the superiority of one hand (whether right or left) over the other,"[10] and is expressed as a ratio between strength in one hand over the other. This comparison is pertinent because "superiority of one hand over the other is evident when the child enters school, but becomes increasingly evident as maturity approaches."[11] If, then, handedness is related to intelligence, those in the higher percentiles should have a greater diversity than those at the central tendency, and those in the lower percentiles should be more or less ambidextrous.

The findings again showed no significant correlation, the coefficient being only 0.07 ± .03. There were, for example, 10 *Ss* in the lowest intelligence level and 12 in the highest who were equally strong in both hands. This illustration is typical of the results.

At the end of the first semester in 1931-1932 a study was made to ascertain whether the instructors' grades were related to the strength of grip in either hand and to the index of dextrality. For this purpose 50 cases were taken at random, and the check merely supported the findings of the more extensive study mentioned above.

It appears, therefore, that there is no significant relation between the two variables —strength of grip and intelligence—among college freshmen. The investigators who have found a correlation have dealt with dull children, delinquents, or mental defectives. It may be that subjects of these types are neurologically predisposed to ambidexterity, but if that be the case and if a relation exists between strength or ambidexterity and intelligence, a sampling of a group of young normal adults should show, at least, some suggestion of it.

Bucknell University PHILIP L. HARRIMAN

SOME OBSERVATIONS ON AFTER-IMAGES OF BODILY MOVEMENT

The observations here given are fortuitous rather than designed, but are, I think, suggestive enough to be worth recording. They concern after-images of bodily movement following several days at sea and are reported from notes made as soon as feasible after the events described.

Sometime ago I had occasion to go on a cruise off the Maine coast. The boat was a thirty-foot sloop, staunch and deep, yet, because of its small size, not free from rolling and bobbing in a seaway. For three and one-half days we were on board, without once setting foot on land. Possibly one-half the time was spent 'outside' (beyond sheltered water), where considerable ground-swell was always present and where, from late forenoon to early evening, it was usually choppy or rough; the rest of the time was spent 'inside,' mostly at anchor, where swell was absent but where the breeze was generally sufficient to roughen the water and produce a clearly perceptible movement of the boat. Accordingly, for the approximately eighty-four hours of the trip our bodies were in almost constant motion.

We returned on the late afternoon of the fourth day. Upon stepping ashore I noticed a lack of steadiness in balance, which lasted for upwards of an hour, but no reactions which could properly be called after-images. About three hours later, however, when the excitement and activity consequent to returning had abated, a striking experience occurred. I was standing in a relatively quiet room, feet slightly spread, quite relaxed and unoccupied, and conscious of general fatigue, when suddenly the floor beneath me began to rise and fall, at the same time rolling and tipping in all directions. This continued for perhaps a minute. The movements were rhythmical, but not uniformly so, and were unmistakably like the rhythmical but irregular movements of the sea. The rise and fall varied from hardly perceptible excursions of a half-inch or so to six or seven inches, and the rolling and tipping varied from less than one to several degrees. The movement was felt rather than seen, and upon closure of my eyes became much more vivid; in fact, there were moments when, with eyes closed, I was sure I was being thrown off balance, although the raising of the lids failed to reveal any appreciable body sway. Consequently with the eyes open the experience was perceived as an illusion, but with the eyes closed it seemed to be real. The cues were distinct changes of pressure in the soles of my feet, and a general kinaesthesis, chiefly from my legs, similar to the kinaesthesis of balancing movements. The experience appeared to die out of its own accord, though I was aware of something like a change of set or attitude, a change of orientation, and I believe this had much to do with the disappearance. There were no recurrences until bedtime.

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[10] Whipple, *op. cit.*, 100.

[11] *Ibid.*, 107.

At that time, upon getting into bed and relaxing, I began to experience the movement with even greater vividness.[1] The bed commenced to rise and fall, roll and tip, and I had definite experiences of changes in cutaneous pressure, as though I were sliding longitudinally a centimeter or so back and forth and rolling slightly from side to side. Had I not known better, in that dark room I could easily have convinced myself that I was still on the boat. There was, again, a consciousness of bodily fatigue. The experience varied in intensity and extent, sometimes momentarily disappearing, to recur, usually fainter, in a manner not unlike negative after-images in vision. I do not know how long this experience lasted, probably not more than a few minutes, for it was lost in the drowsy descent of sleep.

The next morning there was nothing, though I had memory of a dream of being abed on board the boat, which was tossed about considerably by a strong wind and gave me restless anxiety lest it drag anchor and pile up on the shore. During the day there were only one or two occasions when, in a moment of pause, I thought I was aware of slight movement of a fleeting and indefinite nature. Whether considerable occupation with overt matters inhibited the response, or whether it had simply faded I have no way of knowing. Upon retiring that night, however, it reoccurred, but much less vividly than the night before. The next day, and thereafter, there was nothing.

The experience was nearly always pleasurable and lulling; one had, when abed, visions of being gently swayed to sleep.

The following highly tentative generalizations suggest themselves: (1) The movement imagery is like the original movement sensations in quality and pattern. (2) The components are tactual and kinaesthetic. (3) Vision is not contributive;

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[1] Dallenbach reports a similar experience after a long automobile ride; cf. K. M. Dallenbach, Recurrent images, this JOURNAL, 35, 1924, 155.

it seems to inhibit. (4) The phenomenon seems to occur only in moments of relative relaxation and pause, and perhaps is conditioned by fatigue. (5) The experience obtrudes; it is apparently not aroused at will. (6) The experience is almost invariably pleasant.

Western Reserve University HERBERT GURNEE

RESPONSES OF ANIMALS TO THE AIRPLANE

In a recent and extensive airplane trip, I noticed (when the 'plane was flying low enough to permit observation) that the different animals behaved very differently to the stimulus of the plane. Geese, chickens, and other domestic fowl almost invariably fled for shelter as the plane approached. Other and wild birds nearly always took wing and flew away at right angles roughly to our line of flight. The quadrupeds, on the other hand, appeared relatively indifferent. Most of the cattle and sheep were undisturbed. When the plane passed directly over their heads, the cows and horses would sometimes stand rigidly looking up. When we were flying very low, a few cows (not more than one or two in any pasture) would run about excitedly but the alarm did not become general. So far as my observations are concerned, goats and sheep seem to be the most indifferent to the airplane, plow-horses next, and cattle next. Domestic fowl are thrown into a wild panic.

The airplane is essentially a new stimulus. Neither these animals nor their ancestors are likely to have had any direct experiences with it. Reactions to it cannot be explained as conditioned reflexes or as mechanically operative racial habits. The plane does, however, as seen from the ground, bear some resemblance to a large bird. If we assume that animals perceive it as a gigantic hawk, then the differences in their behavior become somewhat more intelligible. Hawks are not dangerous to horses and cattle. These large quadrupeds have no reason to fear birds of any kind. To geese and chickens on the other hand, the hawk is one of the most deadly enemies. If they interpret the airplane as a giant hawk, their frantic efforts to find shelter are not unreasonable.

West Springfield, Mass. MABEL F. MARTIN

THE TWENTY-EIGHTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-eighth annual meeting of the Southern Society for Philosophy and Psychology was held in Wardman Park Hotel, Washington, D.C., April 14 and 15, 1933.

The Friday morning session, with Knight Dunlap serving as chairman, was devoted entirely to experimental psychology. J. E. Morsh, (Gallaudet College), in the first paper, reported comparative data on motor performances from two groups of *Ss* equated as to sex and age, one group being from the Columbia Institution for the Deaf and the other one consisting of persons who were normal in hearing. The performances involved were speed and accuracy of movement, inhibition of movement, hand-eye coördination, speed of eye-movement, and balance. The tests, involving both hands, "were repeated over a period of days to ascertain the effect of practice." Results indicate "the importance of motivation in tests of this kind," and wide variation among the *Ss* in degree of handedness and motor learning. It is hoped that Dr. Morsh will indicate in detail how the differences in motivation operate.

R. H. Peckham (Johns Hopkins University) described a new precision stereoscope for investigating problems in binocular vision, particularly retinal rivalry. The instrument is essentially a four-mirror Wheatstone stereoscope. There are proper lenses for adapting it to the accommodation of the crystalline lens, and there is a vertical background for a projection plane. The background is mounted upon a carriage which bears two telescopes trained on the reflected images of *S*'s eyes. To make permanent objective records, moving picture cameras are substituted for the telescopes. This permits of a high degree of accuracy, and several checks are available. The records of the extent, frequency, and duration of eye-movements or torsions in retinal rivalry, made simultaneously with *S*'s report of his observations, afford "convincing evidence against the drainage theories devised to explain, among other things, temporary functional anopsia."

Frank A. Geldard (University of Virginia) described a case of total color-blindness in a man 23 years old. The data consisted of measurements of acuity, reactions to color and to brightness tests, associated refractive defects, nystagmic movements, and bodily adjustments. An ophthalmological examination revealed findings which checked with those of former studies of total color-blindness.

Sarah Calista Dunlap (Johns Hopkins University) described work in progress on the voluntary modification of the knee-jerk. Improved apparatus is being constructed for experiments on reversing the knee-jerk, on transferring it to the non-stimulated leg. The degree of success thus far attained raises questions as to the methodology of 'conditioning' reflexes and as to the criteria of the 'conditioned' reflex. Great fluctuations were noted, both in the records of any one individual and among those of different individuals.

John E. Winter (University of West Virginia) reported a study of 37 women theft-suspects in which cardio-pneumographs and association tests were compared as to efficiency. It was found that the former records are more 'trustworthy' than association records, indicating the guilty better than the latter method; but such psychograms showed distinct symptoms of guilt in occasional innocent persons. Both methods combined gave more reliable results than either method separately. The guilty persons were determined by confessions, but the experimenter admitted that on the test records alone, without confession, they could probably not have been detected. How does he know that those who confessed were the only guilty ones, even though most of them were probably innocent? At least one of those who confessed hoped thereby to save further embarrassment.

Lyle H. Lanier (Vanderbilt University) reported preliminary studies of the relationship between speed of reaction and certain basic physiological processes. Thirty-five undergraduate men were given speed tests in both simple and discriminative reactions, in two forms of tapping, in marking lines, naming colors, reading names of the same colors, and cancellation. The five slowest and the five most speedy men (as selected on the basis of mean percentile rank scores in these tests) were then measured as to blood pressure and basal metabolic rate, and incidentally also in rate of respiration, pulse rate, and pulse pressure. Though the differences between the two groups in the physiological tests were comparatively slight, the 'quicker' group showed higher basal metabolism rate, higher blood pressure (both systolic and diastolic) and faster pulse rate than the 'slower' group. The latter group had, however, a higher mean respiratory rate. There was no group difference in pulse rate. The technique and the results were regarded by the author as only tentative.

Joseph Peterson (George Peabody College for Teachers) reported analytic studies of the generalizing processes by senior college and graduate students on the Disk Transfer problem. This problem has been used as a learning test in the study of racial differences (see *Ment. Meas. Monog.*, 1929, no. 5, 31 f., and 61 ff.). *S* was required to transfer a set of disks stacked in Circle A (in an order reverse to that of size) to Circle C (in the same order), using Circle B as a 'relay station.' Only one disk must be moved at a time, and no disk is to be put upon one smaller than itself. Errors would make more moves necessary but would not at first be noticed by *S*. Instead of keeping time, which seemed in preliminary work to confuse some of the *Ss*, *E* tallied marks for the several moves, and indicated with appropriate signs the occurrences of certain important errors, as well as *S*'s remarks, his method and special difficulties. For each *S* a demonstration of the solution with three disks was made. *S* was then asked to solve again the 3-disk problem to the point of two trials in succession without any excess moves; next he was required to solve the 4-disk problem; and finally the 5-disk problem. He was informed after each trial as to the number of moves made and the number of necessary moves. After this it was pointed out to him that only one move was sufficient if only one disk was used, that three moves were necessary with two disks, and it was recalled that the 3-disk problem required 7 moves, the 4-disk problem 15 moves, and the 5-disk problem 31 moves. Then he was asked how many moves would be necessary for a 6-disk problem, for a 7-disk problem, an 8-disk problem, and finally an *n*-disk problem. He was asked to go to an adjoining room and work this last one out by developing a formula, taking what time he needed. Results showed marked variations in method of attack, in degree of random (as opposed to rational) procedure, and in moves required at different levels of difficulty. Of 14 *Ss* only 5 supplied the formula and were able to give really helpful general statements supposed to aid a novice beginning the problem. Many *Ss* failed even to keep clearly in mind at critical stages what was to be done next, and showed insight only temporarily here and there. The degree of random procedure was rather surprising.

Two sessions were run simultaneously in the afternoon. One dealt solely with philosophy, and was presided over by Anna Forbes Liddell, the president of the Society; the other was devoted to papers on experimental psychology, Joseph Peterson acting as chairman. The philosophical papers offered were "Pickwickian senses," by Charles A. S. Dwight (Keuka College); "Platonic love," by Adam Alles (St. John's College); "Leibniz' Theory of Knowledge," by Lewis M. Hammond (University of Virginia); "The determinable and its determinates," by John Kuiper (University of Kentucky); "Philosophy and religion," by Lucy Shepard Crawford (Sweet Briar College); "Some historical steps toward parallelism," by Albert G. A. Balz (University of Virginia); "The sophistic attitude in art appreciation," by Marjorie S. Harris (Randolph-Macon Woman's College); and "Frames of reference in philosophy," by W. P. Warren (Furman University).

In the section on experimental psychology, J. M. Porter, Jr. (University of Virginia) led out with a report on studies on 60 epileptics, carried out to determine the total apparent bodily resistance to the passage of a weak electric current, and the galvanic skin reaction (the so-called psychogalvanic reflex), both during 'normal conditions' and during the 'seizure.' Differences were found in the *Ss* between the normal and the seizure conditions only in bodily resistance to the current, the resistance during seizure dropping to a level well below that of normal adults in general. The total apparent bodily resistance during 'normal' states "was considerably higher than that found in normal adults." Porter held that "the total bodily resistance and the appearance of the galvanic skin reactions are to be regarded as indicative of sympathetic innervation during the seizure in the light of results obtained by other investigators working with preparations in which innervation by the different branches of the nervous system was controlled." Questions arose in the discussion as to whether some of the subjects were not really hysterical epileptics.

Wendell L. Gray (Johns Hopkins University) gave a preliminary report on studies of the effect of forced activity (fatigue) on learning in white rats and on the selection and consumption of food materials. The animals were forced to run various distances in rotating drums and electric shock was employed as motivation. Apparent differences between 'control' and 'experimental' animals were tentatively reported both as to learning rate and as to quantitative and qualitative food consumption.

Wallace B. Varner (Washington and Lee University) reported an experiment on the effects of alcohol on the maze habits of albino rats, designed to test the view of certain pharmacologists "that alcohol affects the nervous system directly." If the hypothesis is true "one would expect that alcohol would give, temporarily at least, some of the same effects found by Lashley in his brain lesion work." Ten albino rats, beginning age about 85 days, were used. Two mazes were used (a simple and a complex one) similar to those devised by Lashley, and the motivation employed was that of getting to food after a deprivation period of 24 hours. The alcohol was not administered until after the maze habits were firmly established. At this stage the animals were subjected to alcohol fumes in a galvanized tank, first for a period of 5 min. each day, and then for progressively longer periods until the animals "were obviously under the influence of the drug." The animals were daily tested in the maze in the order simple–complex–simple–complex after being removed from the fumes. Results showed that the simple maze habits were unaffected by exposures which "reduced the efficiency of performance in the complex maze by greatly increasing the number of errors, lengthening the running time and by producing neuro-muscular incoördination." Progressive resistance to the drug with continued exposure on successive days made the animals in time immune to degrees of exposure originally effective. The conclusions, that alcohol affects the nervous system directly and that "apparently, narcosis does temporarily what a cerebral lesion does more permanently," certainly needs particularization and detailed analysis. There is nothing unusual or unexpected in the results themselves. But the effect of the alcohol is not "direct" in the sense that removal of parts of the brain is. Miles has shown that alcohol tends especially to paralyze the action of coördination of the hind legs of the rat. Were the present results due to such behavior, or to more general incoördination?

Wayne Dennis (University of Virginia) reported results of a survey of data on the order of difficulty of maze blind-alleys, including results from his own experiments. Data from the distribution of more than 2,000 errors collected only from responses in repetitive simple alternation mazes show that in this type of maze, "regardless of the size, conditions of experiment, etc., alley difficulty (total errors) decrease with serial order for at least the first four alleys. Beyond alley 4 or 5, no further decrease with difficulty occurs." It was found by comparison of

4-alley with 10-alley mazes that the former as to relative difficulty are like the first four alleys in the latter, not like the last four alleys. He concluded that the factor determining the order of difficulty of maze alleys is proximity to the entrance and not to the goal. It might be added that only in mental mazes, where spatial factors are eliminated, does a true backward order of elimination from last to first alley show itself, and that the backward elimination is based on a progressively increasing 'learning coefficient' (proportion of expected runs past to runs into blind alleys) from entrance to goal as based on the law of probability (cf. *J. Exper. Psychol.*, 3, 1920, 277 ff.). There are reasons for doubting that "alley difficulty" correlates perfectly with total errors made in the several blind alleys.

Frank A. Pattie, Jr. (Rice Institute) reported a study of social behavior tendencies in 42 chicks which had been hatched and reared in isolation. Normal chicks of equal number were used as controls. Every chick was individually placed in a test-pen 30 min. daily for 6 days, beginning at the age of 4 days. In one wall of the test-pen were two similar windows. Outside one of these windows were two stimulus-chicks of the same age as the tested chick, and outside the other were two white mice. Provision was made for an automatic recording of the time spent by the tested chick before each of these windows. The normal chicks on an average spent 61% of their time "trying to reach the chicks" and 20% "trying to reach the mice." The corresponding percentages of the chicks reared in isolation were 35 and 32. These differences in themselves may not be significant, but a study of the changes in preferences through the successive days shows a gradual shift from nearly twice (41:23) as much time devoted to the chicks the first day to over 44% (27:39) more time given to the mice than to the chicks the last day. A possible interpretation of this shift was tentatively given by Pattie, with due caution. He suggested that it might indicate an innate social tendency as operating on the fourth day of age, but gradually waning later. Certain data were pointed out as not favoring this interpretation. It is to be hoped that more work of this sort will follow, and the reviewer suggests the desirability also of a check experiment in which the mice are removed from behind the one window, so that the stimuli will be chicks versus no live objects. In view of all the linguistic controversies we have had concerning 'instincts,' carefully controlled experimentation is very desirable.

Roy M. Dorcus (Johns Hopkins University) reported data contrary to results found by Griffith, that "rats subjected to continuous rotation for a long period of time were permanently disoriented and tended to run in a circular path depending upon the direction of rotation." Since retardation and acceleration "are the effective stimuli in provoking nystagmus," Dorcus sought to determine whether the intermittent stimuli of acceleration or retardation would produce any disorientation. His results were negative. There is obviously a need of reducing the differences thus reported from the two laboratories by further experimentation, with most careful statements of details as to procedures and results.

Franklin Fearing and O. H. Mowrer (Northwestern University) reported an experiment on two groups of 10 adult pigeons in each group, designed to determine whether the well-known reduction in vestibular nystagmus by repeated elicitation is due to specific injury to the vestibular receptors, or whether, according to another theory, the reduction is simply a matter of 'learning' or adaptation to a new sort of environment. The pigeons of both groups were given "a standard rotation test for vestibular nystagmus," but the birds of the experimental group were deeply anesthetized with appropriate injections of sodium ethyl-methyl-butyl-barbiturate (prepared by Abbott and sold under the trade-name of "Nembural") on the 1st, 4th, 7th, 10th, and 13th days following this test "and while anesthetized they were subjected to an intensive program of repeated bodily rotation. On the 16th day after the original test they were retested for nystagmus in a normal condition. Results showed no detectable reduction in their 'normal' nystagmus, while the control group, not anesthetized, showed an average reduction of nystagmus of "over 50%." Suitable control tests showed that the difference "was not due to artifacts of any kind." The authors conclude that the reduction of nystagmus is due to 'learning' or to some analogous process in the central nervous system. The conclusion was questioned in the discussion of the paper, as it seemed to involve an interpretation based on a particular view. It might well be postponed until after some careful supplementary studies from a neurological approach as well as a histological post mortem study of the vestibular apparatus of the rotated birds, shall have been made.

After the annual banquet, the president of the Society, Anna Forbes Liddell (Florida State College for Women), representing the philosophers, appropriately interpreted Cusanus in modern terms to the Society, largely psychologists, in a paper on "Instructed Ignorance, The Philosophy of Nicholas of Cusa." The address will be published in full.

In the Saturday morning session, with Dr. Liddell as chairman, the program consisted largely of philosophical papers, some of which cannot be profitably represented in this short report. B. C. Holtzclaw (University of Richmond) gave the first paper on "Ideas of value in ancient philosophy." P. F. Finner (Florida State College for Women), in a critique of recent literature on 'suggestion,' held that suggestion is not merely a social process but may occur with inanimate stimuli; that it is not to be limited only to direct and immediate responses to stimuli; that it is to be distinguished from the more permanent effects of moods, purposes, and wishes; and that the explanation of suggestion must be found either in an arousal or a change in some neuro-muscular set "that affects responses other than the set itself," or in the arousal of responses through conditioned stimuli. He distinguished two processes properly to be designated by the term 'suggestion.' It is hoped that these may be published for careful examination by psychologists, but meanwhile the discussion in general seemed unfavorable to the use of 'suggestion' in psychology as a technical term, because of its indefiniteness and its implication of forces in conditions that would require more analytic treatment for the determination of the real stimuli.

Floyd C. Dockeray (Ohio State University) discussed emotion as "disorganized response," contending that emotion may be regarded as the opposite of attentive or organized behavior; that it is really a disorganization or maladjustment of the individual's total reaction to a situation. "There are segments of the total behavior which are highly organized, such as certain glandular or visceral reactions; but the total picture is that of disorganization." He held that the emergency reactions described by Cannon "are undoubtedly found to occur in organized behavior as well," and promised supporting data from investigations now in progress. The presentation seemed to give the impression, as is evident in this criticism of Cannon's descriptions, of *a priori* assumptions of the points to be investigated. But we shall await the promised data. Dockeray's incidental admission that he had probably approached the problem at first from a too "mentalistic" position brought criticism of the implied dualism not only in this term but also in the implications of terms like "behaviorism," "purpose," etc. Direct explanations of activities in terms of known factors, or of testable assumptions of the operations of such factors, seemed to be preferred by those who discussed the paper. Reactions which phylogenetically may have preserved animals possessed of certain mechanisms with emergency value may of course seem to be 'disorganized' from the standpoint of what the individual was doing before the emergency arose. The response of a newly decapitated hen could hardly be cited as an ideal case of emotional reaction.

John E. Bentley (American University) discussed relations between intelligence and intellect.

H. M. Johnson (American University), discussing epistemological realism with respect to the question as to whether sensory content is public, took the position that even though such realism presupposes that sensible qualities (sensibilia, sentienda) exist independently of their perceiver, and therefore are not changed by reason of being perceived, this does not imply that the sensory content of one individual can be detected as such by another. He criticized the realists who deny the privacy of sensory content, as falsely identifying *sensible* qualities with *sensed* qualities. He held that an indeterminate number of sensibilia may occupy the same 'locus' of public space at the same time, so that even though two individuals may at any given instant be pointing to some spot in which they see what they agree in calling *red,* they may be pointing also to other sensibilia than red. The only way either one may indicate to the other which of the compressed sensibilia he is perceiving is to exhibit his collection of *sensed* qualities. However, as Bertrand Russell and others have pointed out, the system of reference of the one person's percept is so correlated with that of the other person's percept that, although the terms of the relationship, namely the *sensa,* remain private, the interrelations are public.

H. C. Sanborn (Vanderbilt University) presented a paper on the views of Hugo Dingler as expressed in his 1928 publication, *Das Experiment.* The paper aroused much discussion.

The Saturday afternoon program, with Buford Johnson as chairman, was devoted largely to child psychology. Maurice C. Langhorne (Emory University) described an experiment on 78 children, of ages ranging from 7–17 yr., in the training to operate the Renshaw-Weiss pursuitmeter. Sexes were about equally represented. His work was designed to evaluate the truth of two alternative hypotheses set up by him on the influence of the age of the learner on the acquisition of skill. The hypotheses were stated as follows: "If the skilled act or habit is formed by a reorganization of old order habits, then it may be argued that older *S*s, with a fuller complement of habits, should acquire skill more rapidly. If, on the contrary, the process of habit formation does not involve the mere revision of neuromuscular systems already present, but is an acquisition *de novo* of sensory-motor controls, then the rate of learning should correlate inversely with life age." Each child ran 36 cycles, 4 min. each, during 18 consecutive days. Results show that both the limit and the rate of improvement by practice increased directly (but not in parallel with each other) with age, with the maximum "acceleration in improvement" occurring at about 13 or 14 yr. of age. Boys were superior to girls. We can hardly agree with the author that the first of his two hypotheses is confirmed by these results. It is probable that neither of the two hypotheses is correct, that there is no such sharp line to be drawn between "organization of old order habits" and "acquisition *de novo* of sensory-motor controls." However, certain practical inferences as to the relatively favorable periods of age to begin the training in certain types of skill acquisition *may* be made. Such inferences will of course have no relation to the main problem of the experiment.

Grace O. McGeoch (Stephens College) reported that an analysis of the experimental literature on the whole-part problem, including an experiment of her own, reveals no inherent superiority in either method *per se,* their relative efficiencies in any experiment being a function of a large number of uncontrolled irrelevant factors. A further study in 172 9-, 10-, and 11-yr. old children, in which the *S*s had been allowed to find out their own preferred methods, showed that 94 of them used the 'verse-by-verse method,' 69 the 'line-by-line method,' and 9 the 'whole' method. No reliable differences were found between the first two of these methods. On the bases of these results she called in question the "customary textbook treatment of ascribing superiority to the whole method:" There is, unfortunately, still room for further controversy on this old theoretical problem, inasmuch as its solution experimentally involves control of so many (probably uncontrollable) factors as to make solution practically impossible.

John A. McGeoch (University of Missouri) discussed and presented data on the problem as to whether interpolated learning is more effective in producing a decrement in the reproduction of given memorized content when introduced immediately after the original learning than it is when introduced at the end of the intervening retention period just before the relearning of the content. In case the former is true the results would, according to one view held since the work of Müller and Pilzecker, be regarded as favoring the perseveration theory. It is a question of whether interference (the so-called retroactive inhibition) is greater when the interpolated learning is introduced soon after the original learning or later, just before the relearning begins. The experiments were carried out to test this question. In one of them, on the retention of lists of words, the delayed interpolated learning was introduced just before relearning, after periods of 30 and 60 min., and 24, 48, and 144 hr. In the other experiment, on stylus-maze learning, the delayed interpolated learning came just prior to relearning after a period of 24 hr. In each of these experiments, which were on college students, both the immediate and the delayed interpolated learning of different materials brought about a decrement of the relearning of the original material; but there was either no reliable difference between the immediate and the delayed interpolation or the latter showed the greater degree of interference, in contradiction to the perseveration hypothesis. It is possible that any supposed difference between retention by perseveration and otherwise needs investigation in the light of our present better knowledge of the neural processes involved. Possibly the hypothesis under investigation has no real meaning. At any rate, the problem of the effect on reproduction of different degrees of delay in the interpolated learning has certain values irrespective of its bearing on perseveration.

J. E. Bathurst (Birmingham-Southern College) reported results of an experiment on sympathy and resistance (negativism) in 36 boys and 37 girls of ages between 1.5 and 5.5 yr. He had measured 'sympathy' by telling a story designed to elicit sympathetic responses, and 'resistance' by encouraging procedures involving "actual contact" with the children when sympathetic responses were not elicited. The child's environment, his possession of pets, age, and sex were considered in relation to the sympathy and resistance scores assigned. While girls showed slightly more resistance than boys, the author did not remark that both *E* and the 'hero' of his story, told to evoke sympathy, were males, as developed in the discussion; but girls and boys did not differ reliably in sympathy responses. The possession of pets was found to increase sympathy significantly, although other environmental factors did not affect either sympathy or resistance. Sympathy in both sexes increased with age. Unfortunately the score points were assigned arbitrarily and were not based on any objective standardization experiments.

W. C. Beasley (Johns Hopkins University) made a preliminary statement of work in progress on problems regarding the vision of newborn infants. "Numerous traditional views have been found to be grossly in error." While a survey on the ocular functions of 250 newborn infants at the Woman's Clinic of the Johns Hopkins Hospital revealed important individual differences, evidences of "fairly acute" visual discrimination was found in all of these infants by the tenth day, and earlier in most cases. Pupillary reflexes develop in 5–8 days; and horizontal, vertical, and circular pursuit responses improve markedly during the first 10 days. Squints, or incoördinate binocular fixation during pursuit, are infrequent and not typical, while coördinate fixation can be elicited in a considerable distance range during this period. A special technique for investigating visual acuity and color vision has been developed. It is to be hoped that with improvement in such techniques, early studies of the relations of visual, auditory, and kinesthetic localizations and interrelations may be followed closely in their relations to the growth of activity.

Beverly von Haller Gilmer (University of Virginia) reported an analytic study of spontaneous responses in newborn infants through the tenth day, dealing both with data from cinematographic records and with observational data. Responses like crying, stretching, sneezing, mouthing, and other such isolable movements are found to be patterned into body-wide organization, supporting the view "that mass activity is not the only total bodily response of infants."

In the final paper of the meeting, Martha Vance Ellesor (University of Virginia) described an experiment to test "the hypothesis that visual stimuli arouse crying only through a process of conditioning." Two children—ages are unfortunately not given—were individually stimulated with the following: a devil's mask, a flashlight, a brown fur, and a movie. Each stimulus was repeated "on successive days until the child lost interest in that particular object." On the first presentation crying was the response elicited by every stimulus but the flashlight. On repetition of the several stimuli crying and negative responses gradually gave way to responses of "investigation and examination" followed later by smiling and laughter, before interest was lost in the objects. The conclusion, that not all responses to visual stimuli are acquired by conditioning, is not surprising. A greater variety of stimuli tried on a considerable number of children with known developmental backgrounds and studied from an early age with changes of stimuli is desirable, as well as an objective method of recording responses so that individual interpretation may not replace detailed exhibits of actual behavior. The work is obviously regarded by the author as only preliminary.

At the business meeting, following the Saturday morning program, Roy M. Dorcus of Johns Hopkins University was elected president of the Society for the next year, and Knight Dunlap and Frank A. Geldard, were elected members of the Council. The Committee on the Teaching of Psychology, now consisting of 20 members, with P. F. Finner, Florida State College for Women, Tallahassee, Florida, as chairman, submitted a tentative report which gives the institutions within the Society's field that are qualified (a) to teach only elementary courses in psychology, and (b) to teach and train in both elementary and advanced psychological work. The report also lists, with an asterisk, all instructors qualified for teaching only elementary courses in psychology. These data were submitted with requests for supplementation and correction by members who notice deficiencies or errors, so that the final report may be as accurate as possible. This committee also distributed a mimeographed list of 10 American psychological journals and of 430 books printed in English on psychological and related topics. By some slip of the 'censor,' it appears, one book in a foreign language was also included—Fechner's *Elemente der Psychophysik.* The list represents books taken from three different sources: those marked "D" being on "the list prepared by Dr. Dockery;" those marked "A" being on "the list of the American Library Association;" and those marked "S" being books "that the majority of members of the Southern Society for Philosophy and Psychology who checked the list approved." The list is apparently also in a beginning stage of development. It seems important that it should be carefully gone over by, and suggestions obtained from, psychologists widely distributed over the country. It is intended, apparently, although there is no statement to that effect, as a sort of 'minimum reading requirements' suggestion for smaller institutions. It will be beneficial if it does not, in other institutions during times like these, check somewhat the efforts to secure recent valuable books and magazines from foreign countries. Since the War the teaching of German, for instance, has suffered serious handicaps, and even yet it comes back into the high schools with much difficulty.

It was voted to accept the invitation of Birmingham-Southern College and to hold the next meeting of the Society in Birmingham, Alabama, March 30th and 31st, 1934.

Peabody College JOSEPH PETERSON

THE PHILADELPHIA MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The twenty-ninth annual meeting of experimental psychologists was held at the University of Pennsylvania on April 12-13, 1933, under the chairmanship of Professor S. W. Fernberger. The meetings were attended by eighteen members from thirteen laboratories. After a short business session, reports of the work in progress were received from the following universities and colleges: American, Brown, Clark, Columbia, Cornell, Harvard, Illinois, North Carolina, Peabody, Pennsylvania, Princeton, Vassar, and Yale.

The next annual meeting will be held at Harvard University on Tuesday and Wednesday, April 3-4, 1934, under the chairmanship of Professor E. G. Boring.

K.M.D.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Animal Drive and the Learning Process: An Essay toward Radical Empiricism. Vol. I. By EDWIN B. HOLT. Supplementary Essay on The Material World. By Harold Chapman Brown. New York, Henry Holt & Co., 1931. Pp. xi, 307.

'Radical empiricism' (from William James) is described as "a way of thinking which aims to escape . . . from the absurdities of subjectivism and any form of psycho-physical parallelism." The author points out that there are two current explanations of motivation or of answers to the question, "What is it that moves an animal to action?" The one points to chemical energy derived from food and from various sorts of stimuli, while the other ascribes the action to feeling, emotion, images, desires, or even to the older faculties, using a mere name for the explanation. This latter explanation he calls 'word magic.' Conation, will, fiat, instinct, purpose, insight, or even *Gestalt,* are examples in human psychology. Holt seeks to avoid both this word magic and controversies, and to make explanations "in terms wholly of physical and physiological processes" (p. 7).

Growth is not a mere "unfolding from within," but a developing life process constantly assisted from such environmental factors as warmth, moisture, specific nutritive substances, and in some cases light. The constancy of pattern in development, attributed by many writers to "innate conditions," depends on constancy of the external environment, and "the phenomena of structure and function are, in living organisms, never independent of each other" (p. 12). The work of C. M. Child is considerably drawn upon. Holt contrasts the older conception of 'growth,' as an unfolding from within, with 'learning,' as a process brought about by effects of the environment, only to insist that the processes are not so distinct; that the *'structure'* of the organism, the organic pattern, is in considerable part acquired through the influences of external stimuli which produce excitations that are transmitted through the chemically formed but organismically unformed tissue of the embryo. In spite of his insistence upon the rather extreme effect of external factors in the development of organic structures, he holds that "all anabolic processes are doubtless carried on and regulated by the *enzymes* contained in the fertilized egg" and are probably "the sole basis, in any true sense, of heredity" (p. 17). These enzymes *select* from the food material available, "and they determine *how* this shall be synthesized into higher organic compounds (anabolism) to make the *substance* of the organism. But this raw substance acquires ('learns') organic pattern by an inverse process," katabolism. Both of these processes go on together.

Holt regards it as established that the "primitive neuroblasts develop into neurones by growing out along axial gradients," contrary to the preformist's view of inherited sensori-motor connections. These questions are now matters of observation; the growing neuroblasts, in making their way through the embryonic tissue, look much like the growing roots of plants which turn from light toward moisture, select certain chemical substances and avoid others. Both the axones and the dendrites of growing neuroblasts follow electrical gradients, the dendrites growing *toward* (Kappers' law) active neurones or nerve bundles (kathode pole), and the axones *away from* them (Bok's law). According to Bok, when a bundle of unmedulated nerve-fibers grows out and passes neuroblasts, these latter become activated by it and send forth axones in a direction perpendicular to the activating bundle, apparently taking the same direction as the current which irradiates sidewise from the growing bundle. Even the cell body may migrate in the direction of the dendrites. These laws of neurobiotaxis, according to Holt, who in this regard follows Kappers, are, with the small additional consideration as to the direction in which the excitations flow, identical with "the psychological law of association" (p. 21).

Holt goes on to show "that *no* independent new principle enters in to interrupt this continuity of development including 'psychological association' even in the adult stages of life, and that this same developmental process in its later phases is mentation and consciousness" (p. 21). An attempt is then made in Chapter 5—by noting the valve-like or one-way action of the synapse, the probable tendency of every neurone of the motor side of the central nervous system to spread its receptive areas (dendrites) toward the terminal arborizations over which the impulses that are to be conditioned are arriving and thus to extend the receptive areas of the dendrites—to connect these known facts with the processes involved in Pavlov's conditioned reflex, and to account on this basis for the entire mental growth ontologically. Holt thinks "that the only *original* 'unconditioned' impulses are such as make their way through the central nervous system and out to the muscles *at random;* corresponding to the picture of general random movements in the foetus" (p. 27). His opinion is that "this growth of dendrites under the stimulus of nerve impulses is the sole basis of learning" (p. 27). In general, then, any organism is actually propelled, but not guided, by the food energy stored in its tissues, especially its muscular tissues; but it is guided by external stimuli of different kinds operating through the sense organs, by means of dendritic conditionings in the central nervous system as suggested in this chapter.

Several weeks prior to birth mammals show a markedly random type of behavior; it is so uncoördinated that a touch on the eyeball, for instance, will produce a response of almost any organ as readily as of any other. It is generally thought that such synaptic connections between neurones as are necessary for coördinated movements have not at that stage of development been made. In the human infant such random movements are rather frequent even after birth, due to various sorts of inner and outer stimulation. These first utterly random and somewhat jerky movements "become biologically useful, significant, or 'purposive' only very slowly," some of them (especially when new situations are met) persisting even throughout life (p. 31). But even in the late foetal period more definitely coördinated and regular movements, presumably due to the development of synaptic connections, make their appearance. Whether or not these increases in coördination with the advance in age are due to nerve-fiber growth along physiological gradients, or to effects of practice in the reduction of resistances, the statement that "every passage of a nervous impulse across the junctional tissue between two neurones (at the synapse) lowers the resistance of that tissue to the passage of all subsequent nervous impulses" (p. 34) is now subject to criticism on the basis of considerable experimental data.

The effect, then, of conditioning in the central motor dendrites, Holt, following Bok, holds is to produce 'circular reflexes' through the operation of the proprioceptive impulses aroused by the contraction of any responding muscles. Any incoming excitation "will find outlet along the tract just used by the random impulses, that is, will go back to, and will further contract, the very muscle from which it came" (p. 38). In the course of time all the muscles will be involved, "and in all of them such a reflex-circle will then be established. The result is that in every muscle a contraction tends to reënforce and to perpetuate itself" (p. 38). But any response may also produce other external stimuli. For example, if a random impulse reaches the flexor muscle of a finger the response of the finger will cause it to press on the palm. The afferent impulses from this contact will, when this "has happened a few times (as it is bound to happen)," effect a reflex-circle so that a pressure stimulus on either finger or palm will bring about the 'grasping reflex.' This reflex is thus established in the foetal period. Infants "born deaf do not learn to speak even though their vocal organs be absolutely normal" (p. 39). While this statement, from Preyer, is offered as evidence of the general circular-reflex principle, borrowed from Sherrington, the facts are obviously interpretable just as well in other ways which also do not imply "inherited engrams" for specific speech forms. Using language is something more than a mere reflex response of the vocal organs, and to bring such responses and other complexly coördinated acts under the simple principle of a conditioned circular-reflex is certainly a bold hypothesis, one, however, that should not be hard to test out experimentally on young children and animals. Why, for example, in normal children, should "a sound received at the ears cause the vocal organs reflexly to reproduce that sound as closely as their anatomical structure permits?" This is not at all clear in Holt's explanation of the acquirement of speech. Such a basic assumption cannot safely go uninvestigated.

Certainly, without selective reactions by other persons in the social environment, the child would never thus respond. In Holt's explanation he adopts the word "adient" response (suggested by H. C. Warren) as one giving the organism "more of the stimulus" which elicits the response, and "abient" response as one giving "less of the exciting stimulus." Whether every reflex-circle response will cause the organism to get more of the exciting stimulus, as Holt holds against Bok, or whether it will cause it to get less of this stimulus seems to me to depend on whether this response brings about other exteroceptive stimuli practically simultaneously with the response and only during or immediately following its recurrence, or whether it removes the organism from these stimuli. Only in the former case (see *Psychol. Rev.*, 33, 1926, 218-236) can the incoming impulses find outlet in simultaneously active motor channels (physiological gradients or nerve fibers). I see no other reason why the incoming impulses from a given stimulus, or even the proprioceptive impulses, otherwise should go out to the particular muscles that responded to the next preceding stimulus. What is the specific evidence? How can learning ever take direction if stimuli merely effect circular-reflex responses?

Holt seems partially to recognize this deficiency of his circular-reflex response position, where in a note (p. 42) he says: "By avoidance I mean immediate retraction from the stimulus so that the stimulation ceases." I see no need then for the circular-reflex principle at all, and Coghill's work hardly supports it, even though it is cited in this same note. Holt himself seems to have misgivings (cf. p. 43). The next chapter, on "Circular Reflexes and Progression: Inhibition," runs further into difficulties of a technical nature which would require too much space to examine in detail. Holt traces the probable expansion of coördinations from the mere (supposedly) circular-reflex type to hip, shoulder, and limb muscles, flexion and extension movements of the same limb, and correlations between fore and hind limbs, front and back limbs, and among all four limbs in certain animal forms. He then goes on to consider how the organism, on the same principles, becomes responsive by direct and indirect stimulation to "the pull of gravitation," *i.e.* how postural reflexes are developed. Here the proprioceptors continue to play a very large and increasing rôle. It is obvious that any movement of the limbs or head immediately effects changes in the contractions of different muscle systems and preparatory sets for further changes. All these postural tonic reactions are regarded as "purely reflex," not being impaired in decorticated animals; and according to Magnus they are dependent only on the red nucleus of the mid-brain, as the highest center, and do not even involve the cerebellum (p. 70). Yet several of these responses to gravitational conditions are so well coördinated as to involve all-or-none responses of elementary units working on the principle of interrupted responses (refractory phase).

Along with the developments in motor coördination goes "education of sensory surfaces," which Holt infers to be due to the gradual inhibitions or facilitations of impulses of opposing or of coördinating tendencies, until the several sensory points of stimulation coöperate. Here again there is danger of over-simplification in terms of mere discrete reflexes.

Recent results obtained by Beritoff (1926) and by Switzer (1930) show that a stimulus coming slightly after a response may get a control over it, though the conditioning is more difficult than if the stimulus precedes the response. Moreover, there are some indications that the temporal disparity between stimulus and response to be associated may be not merely a few seconds, but even 30 or more minutes (Pavlov). Holt sees two possibilities as indicated by these data: "The first is that a nerve current, after it has passed, leaves behind, at synaptic regions, a temporary condition which is analogous to a state of tension or 'charge' " (p. 88). The second possibility is that "an uninhibited *circular-reflex* (perhaps of low intensity) may be operative. At any rate, he sees in such hold-over effects of nerve impulses possibilities that "*a total afferent pattern* (not merely one or two afferent currents), may be effective in producing the next response, as hitting the next 'right' chord on a piano, or singing the next note, to continue the sequence of a musical selection that one has begun. Holt does not, of course, overlook other motor connections, though he regards them as not sufficient; and he also recognizes the numerous stimulations constantly affecting us from the general surroundings. Chain reflexes are set up between successive reflex responses, when the resistance becomes lowered (canalized), so that the final act of one functional unit in the series excites (directly or indirectly) the next. "Deep and early canalization" Holt maintains, is one of the two essential conditions that determine which shall be, in the life of the individual, the prepotent reflexes, or the (acquired) 'instincts.' Examples are the peristalsis of the gullet and intestines, relatively isolated from extraneous motor channels.

Many important details must be omitted here. In general, Holt describes four factors which make for integration: "first, the external reference of all learned responses: second, the mutual summation and inhibition of responses; third, simultaneously sustained responses with a residual locus of freedom (for random movements); and fourth, cross-conditioning" (p. 230). These are not to be regarded as wholly separate or coördinate factors. The second is particularly questioned in this regard. In the case of the third, the author holds that every response that is effective simultaneously with others—"whether tonic, postural, or actively phasic"—narrows the locus of freedom for random movements, thus making the action more definite and precise. Cross-conditioning is the reënforcement or canalization of certain overt activities by their association with or attachment to basic and necessary bodily rhythms—heart and lung activity and other autonomic processes—so that they become rather permanent tendencies or settings of the organism's motor predilections and general character. These predilections thus come to serve as "ready and characteristic paths of nervous 'overflow' in every situation of partially obstructed activity" (p. 226). A fifth possible integrative factor is 'motor block,' which "is closely related to inhibition" (p. 230). When any obstacle to certain motor activity is encountered there seems to be an overflow of nerve impulses into other closely related channels (though our ignorance is still too great to specify or to be certain). Various random activities, often not very accurately anticipated even at this advanced stage of learning, result. Some of these supply a way out of the difficulty and the organism moves on in accordance with its own inner drives and external, guiding stimuli. Self-interest, the constant movement away from annoying and blocking stimuli, is always the rule. Therefore "an appearance of 'end sought' always characterizes . . . the process of learning by random trial. This appearance has given rise to the much misleading speculation about a *vis a fronte.* In reality the *vis* is always strictly *a tergo*" (p. 239). "That it [behavior] should *all* be in the last analysis self-interested, follows inevitably from the physiological structure of every organism" (p. 240). The scheme is acknowledged to be but a very rough sketch of enormous complexities which can be filled out in detail only when our present knowledge shall have been greatly extended. Holt appropriately criticizes vague notions of the 'organism as a whole,' and denies that the organism ever does act as a whole. "To say that a 'whole' regulates or in any wise operates on its parts makes the 'whole' something other than the totality of its interrelated parts, a metaphysical other that lends itself to viscious thinking."

The book is so stimulating and vigorous in its constructive features that psychologists will await with interest the appearance of the promised second volume. There are fifteen pages of "works cited." The essay on "This material World," by Harold Chapman Brown, is reprinted from *The Journal of Philosophy,* 22, 1925, 197-214.

J.P.

Studies in the Dynamics of Behavior. By CALVIN P. STONE, CHESTER W. DARROW, LENA L. HEATH, and CARNEY LANDIS. Edited by Karl S. Lashley. Introduction by the editor. Chicago, University of Chicago Press, 1932. Pp. xiv, 332.

Three separate studies on the dynamics of behavior are reported in this volume. The first, by Stone, is on "Wildness and Savageness of Rats of Different Strains."

Stone used, in this study, five different kinds of rats between 3 or 4 mo. of age: wild trapped rats, pure albinos, crosses between the wild and the albinos, and between the half-breeds and albinos, and a group of yellow hooded rats. In his first experiment he attempted a quantitative estimate of wildness and savageness. In mean ratings, the groups arrange themselves in a descending order as follows: full-wilds, half-breeds, quarter-breeds, yellow hoods, and pure albinos. Differences in group variability are small. A similar quantitative study of younger rats should be made so that the effect of training on these activities might be measured, the pattern of the behavior studied, and the degree to which experience determines these patterns noted. While the effects of experience are hard to eliminate entirely, they are reduced in this study to a minimum. An interesting check on the results would be to use albino rats that had been allowed to grow up wild from birth. This should give some clue as to the stability of the change in the direction of tameness which has been produced in albinos by breeding and training.

To test the dominance of the hiding tendency over hunger, apparatus consisting of three lengths of stove pipe arranged in a rectangular form with a starting box at one end and a food box at the other was used. The hiding tendency was measured by the time spent by the rat in the stove pipe. In descending order of strength of the hiding tendency the groups ranked as follows: full-wilds, half-breeds, yellow hoods, quarter-breeds, and albinos. The author does not state or infer that this hiding tendency is inherited, but seems to associate it with the other forms of behavior characteristic of the wild group. How would this tendency in the albinos be influenced by living a wild life?

In the third task set the animals, the learning of a simple maze, it was found that the wild parent stock could not be induced to learn the problem. However, in terms of time and error scores the half-breeds and quarter-breeds were superior to the albino and yellow-hooded groups. Though no explanation of this result was found, Stone concludes that by systematic handling and strong hunger motivation would probably produce a degree of persistence and effort in genetically wild rats comparable to that found in the domesticated albinos.

In the second investigation, Darrow and Heath obtained measures of physiological changes elicited under standardized conditions and tried to check these measures against scores on the Thurstone Neurotic Inventory, on the Northwestern Introversion-Extroversion Test, and on certain available scholarship and intelligence ratings. Under laboratory conditions stimuli were presented to 90 college students to elicit "behavior of a psychological significance, such as anticipation or anxiety, relief, adaptation to repeated stimuli, summation of responses to repeated stimuli, conditioning to indifferent stimuli, and rate of extinction of the conditioned responses." Measurements were made of arm movement, blood pressure, respiration and the galvanic skin reflex. The stimuli employed were a standardized electric shock, verbal warning of shock, announcement of a rest period with no shock stimuli, and clicks of the pendulum used in governing the time of the electric shocks. The strength of the shock to each individual was so adjusted as greatly to reduce the variable effects usually obtained with a constant strength for all subjects. The shock was standardized in terms of the strength of a tetanizing current just strong enough to effect the flexion of the thumb.

Twenty-one different types of measure, both absolute and relative, of changes occurring during the stimulating situation were correlated with the questionary scores and with separate items within the questionary.

To increase the reliabilities, empirical groupings of personality items and of physiological reaction-tendencies were made, depending upon the relationships intrinsically found in the data. Ten different "personality constellations" were thus formed and separate scores for each computed. These were correlated with the standard scores of the physiological measures. "Physiological syndromes" of reaction-tendencies were then identified for each of the personality constellations. Correlations between physiological reactions and composite personality scores were generally low, only 14 of the 240 being above 0.20. The five highest coefficients were between 0.26 and 0.31. The authors conclude that their physiological data show no temperamental types, as determined by the questionnaires. The coefficients between the physiological syndromes and the personality constellations were again very low, the highest ranging between 0.30 and 0.38. It is acknowledged that from the point of view of predictive efficiency such coefficients are worthless.

The authors offer an important criticism concerning the measures used to indicate changes in resistance, which is applicable to the majority of studies on the galvanic reflex. They point out the need of a continuous record of resistance and the use of relative or percentage resistance change rather than absolute resistance change as a measure of the reflex. *Their argument finds support in the results of other investigations, and, if valid, reduces to a meaningless hodge-podge the recorded measurements of the majority of investigations on the galvanic reflex.* (Cf. *supra* 502ff.; 522ff.)

The third study is an effort to make a comparative evaluation of the various methods which have been used to measure emotion, emotional stability, and emotional psychopathy. The first part of the investigation is concerned with "a study of 100 delinquent boys" and the second with "a study of 112 delinquent girls." The following measures were used with both boys and girls: Thurstone's Personality Schedule, a test of humor, and an X-O test. In addition to these the boys were tested on a pursuit-meter and stabiliometer during which test measures of the galvanic reflex were made. They were also given Allport's Ascendancy-Submission Reaction tests, and their educational quotients were computed from grade-placement records. The girls were given Heidbreder's Introversion and Extroversion Test, Hull's Test of Suggestibility, and a maze test. Institutional records giving incidents of social history were available for both groups. Juvenile court records were available for some of the boys. Measures of temperature, humidity, and barometric pressure were obtained for the days during which the boys were tested on the pursuitmeter. Since most of the tests were constructed for use with college students the authors endeavored to adapt them in different ways to the delinquent groups.

The general conclusion to be made from the study of these two groups is that the measures used, in every particular, fail to show any diagnostic value in reference to emotional stability or emotional psychopathy. No relation was found between age, race, or social offense and emotional stability, or between scores on any parts of the various tests employed. All tests of emotionality failed to give diagnostic results when used with the juvenile delinquents, even those tests which had been especially adapted for these groups. Out of 134 intercorrelations obtained from the group of boys only, 10 were above 0.30 and of these only two were greater than 0.40.

The author, in view of some of his former investigations on the rôle of such factors as blood pressure, respiration rate, etc., in emotions, attempted particularly to determine the diagnostic value of the galvanic reflex. Much has been written of late on the use of this reflex as a sign of emotion. The general logic of the argument has been about as follows: (1) the autonomic nervous system, especially the sympathetic system, functions in the type of behavior called emotional; (2) the galvanic reflex is dependent upon the integrity of the motor fibers of the sympathetic system and is disturbed when the supposed nervous center of this system in the thalamus is injured, and therefore it can be used as a sure sign of the activity of the sympathetic system; (3) the galvanic reflex can then be used as an index of the emotional reactivity of the individual in which it is found. Landis' results are quite opposed to this whole line of reasoning. He found (1) no galvanic reflex invariably accompanying the objective signs of emotional disturbances; (2) no significant correlation between either the galvanic reflex and tests of emotion or this reflex and personality as judged from the previous records of the individuals; (3) no unequivocal evidence of a relationship between the reflex and the activity of the autonomic nervous system. In accordance with his formerly expressed views he concludes that the galvanic reflex is just another physiological change such as blood pressure, respiration, etc., which may or may not occur together with one or another type of psychological activity such as emotion or attention. The argument that the galvanic reflex is specific to emotion is thus not substantiated by experimental fact. Due to the present wide use of this reflex in studies of emotions his findings are both pertinent and important. The whole problem of the functional significance of the autonomic system in emotional responses should be reopened and further experimental investigations performed on the first two arguments above. Such investigations, however, will probably have to await the time that psychologists can agree on a set of criteria by which emotional responses can be differentiated. Valuable parts of the third study are the records of the individuals of both groups on the various tests used together with certain data from their social histories. These records are in the two appendices.

University of California C. W. Brown

Economy and Technique of Learning. By W. F. Book. New York, D. C. Heath & Co., 1932. Pp. vii, 534.

The author states in his introduction that the volume is intended primarily "to serve as a text for teacher-training courses in colleges and in schools or departments of education" Employed in the light of this aim, it should serve to give the student in such courses a stimulating introduction to some of the more widely recognized problems of learning and should prove of genuine value to those whose knowledge of the subject is confined to an elementary course in psychology (which the author advises as a prerequisite to its perusal).

Having defined learning as "the process of acquiring the ability to do something that the learner never did before" (p. 16), Book proposes (p. 21) as essential phases or conditions of the learning process: (1) a motivating condition within the learner that makes him react; (2) a stimulus toward which the learner reacts; (3) an appropriate reaction to the stimulus; (4) some consequent change or modification produced in the organism as a result of the stimulation and the responses thereto; and (5) a check on the learner's responses. Learning may occur, we are informed, on a variety of levels, among which the following are distinguished: (1) racial learning—evolution; (2) physiological learning—*e.g.* adaptation to bad air; (3) conditioning; (4) perceptual learning; (5) representative learning (thinking); and (6) learning how to learn.

Book postulates six "elements of learning" to each of which a chapter is devoted. (1) Element A: Strengthening of existing connections by exercise. (2) Element B: Weakening of existent connections by additional learning, inhibition or disuse. (3) Element C: Establishing of new linkages between new stimuli and old responses. (4) Element D: Linking of new responses to old stimuli. (5) Element E: Invention of new responses and linkage of them with old stimuli. (6) Element F: Linking of new adaptive responses to new stimuli.

The volume concludes with a section entitled "How to make learning most economical and efficient," in which the author considers motivation, proper arrangement of stimulus-situations, effects of ventilation, illumination and fatigue, use of knowledge of goal and of score, and adjustment to variations in the curve of learning and modes of obtaining maximal transfer-effect.

The range of topics is wide, and many of the concepts developed should prove of value to students of education in replacing outmoded principles of a non-empirical pedagogy. Aside from a marked redundancy in style, the book is easy to read, and the arrangement is coherent. Particularly valuable is a final chapter in which an effort is made to bring together and to arrange in systematic order the major categories of the book.

As a textbook intended for students in elementary courses, then, the volume merits favorable consideration and should be of value to teachers of pedagogical courses. We read with amazement, however, the author's statement (v) that "the book should be of special interest to psychologists . . . interested in the scientific aspect of the psychology of learning since the attempt is made in it to bring together and interpret *all the facts about the learning process that have been gathered by the experimental studies of learning during the past thirty-five years*" (Italics mine). The briefest perusal of the volume will convince the casual reader that Book could have written this statement only in sheer irony and in a deliberate attempt at hyperbole. If the volume actually included reports of 'all the facts of learning' that thirty-five years of experimental work have produced, the field of learning would be in a parlous state indeed!

To become acutely aware of this, one need only tabulate the footnotes (as the present reviewer has done) to be convinced that the volume could have been written only as an elementary textbook. Out of slightly less than 200 footnotes documenting the author's references to the literature, there are some 70 references to the author's own publications. While one would not object to this in an elementary textbook, it seems slightly out of proportion in a comprehensive view of 'all the facts of learning,' since only one other authority is mentioned more than a bare half-dozen times and even this one exception is limited to 10 citations. Aside from Book's own contributions there are some 50 references for the decade 1922-1932, but close inspection shows that only about a third of these are taken from primary sources, the other two-thirds falling to textbooks and other secondary sources. The last five years are represented by half a dozen citations from primary sources; and there is nothing to show the student that scores of experimental articles have appeared in the journals each year during this period. In fact, as one turns the pages of the volume, one is impressed by the lack of references to the more important of the modern experimental contributions, the chapter on the objective conditions of learning being a fair sample in this regard. The topic of forgetting is ignored altogether and the great mass of studies of verbal learning fares no better. The name of Ebbinghaus appears nowhere in the volume.

The author's use of apocryphal material, his free employment of the word 'law' to cover any principle he formulates, and his frequent strange liberties with logic may prove irritating to the critical reader. As an example of the first of this trio of irritants, we may quote briefly from the author's discussion of racial learning. "An animal species in its experience with its surroundings, like the individual members in their own more limited environment, is making progress in its ability to deal with its total environment. It is becoming more adaptable or, as we like to say, it is becoming more 'intelligent' as the ages roll by. *In a general way the individuals of each succeeding generation find themselves better able to cope with the difficulties that nature presents than were the individuals of the preceding generation*" (p. 9). (Italics mine.) After reading this, one shudders to think of the temporal proximity of the super-mouse and the super-oyster, to say nothing of the super-paramecium!

'Laws' are sprinkled so generously through the pages that space forbids detailed consideration of any of them; suffice it to say that any generalization, proved or unproved, appears to qualify as a 'law.' (Perhaps one should not be too severe with Book on this point, since certain of his contemporaries share his generous definition of this word.) We must content ourselves with a single illustration of his logical vagaries, leaving many others for the delectation of the critical reader. In discussing the effect of length of sleep upon work performed, Book writes; "No correlation was, therefore, found between the amount of sleep taken and the amount of work attempted and done. *In fact, those who slept the least seemed to do the most work*" (p. 393). (Italics mine.)

The reader may take exception to Book's continual use of *petitio principii* in resorting to the 'practical' whenever the going becomes difficult; and he will not be favorably impressed by the continual loose usage of such psychologically flavored terms as 'unconscious attention' (269), 'reversal of stimulation' (274), and 'efficiency' (429), the last being used where output alone is known. In fine, if the volume is intended for use as an elementary textbook in undergraduate courses, one may say only that it is superior in content and in organization to the vast majority of such publications. If, however, the author actually intends to offer it as a modern and comprehensive treatment of 'all the facts of learning,' he need expect little serious attention from critical researchers in that field.

Cornell University JOHN G. JENKINS

The Affective Consciousness. By H. H. BRITAN. New York, Macmillan Co., 1931. Pp. ix, 391.

The author of this book is Cobb Professor of Philosophy at Bates College, Lewiston, Maine. Its purpose is to present a systematic treatment of the affective consciousness, of which, the author states, there has been no such treatment since Ribot's *Psychology of the Emotions* was published in 1889. The text in mimeographed form was used as the basis of a course given to college students.

The word *affective* is used in its most comprehensive sense, as covering everything conveyed by the words *affection* and *emotion* in psychological usage. The scope of the book may be indicated by the mention of a few of its sixteen chapters: The Motives of Men, Pain, Pleasantness and Unpleasantness, The Emotions, Fear, Imagination and Emotion, The Ethical Emotion, The Aesthetic Emotion, The Religious Emotion.

The author at the outset refuses to enter into the discussion of the merits or demerits of behaviorism, intellectualism, the instinct theory; and takes the position that his own theory, which is interactionistic, will justify itself by the truth it can produce, although it is not plain whether he means the truth that will appear in the remaining pages of his book or in future years. He escapes the necessity of taking sides in any instinct-habit controversy by saying, "we possess, *either by nature or nurture,* the tendencies to respond in definite ways." In fact, the book can be characterized by the word *irenic.* It constitutes four hundred pages of placid and benign discourse, in which controversies are merely hinted at. The students to whom the contents of this book were delivered, except the more original minds among them, must have passed from the room where the lectures were given, saying to themselves, "The professor is in his chair; all's well in the psychological world; although there is some rumor of rough weather there, it is only a passing squall." The most characteristic statement in the book, which epitomizes the entire production and might be used as a motto, may be found at the bottom of page 13: "It is not our purpose to take sides."

Laying down the principles that ideas alone are not always sufficient to produce action, that the motive values of ideas are derived from their affective tone, and that pleasure and pain act as selective factors, the writer proceeds to glide through page after page of psychological commonplace. While it is all expressed in a genial manner and in a style that is sprightly and sometimes pungent, the surface treatment invalidates the book as a text for use in advanced courses, which the writer intended it to be.

The author seems greatly exercised over the possible ill-effect upon the young, of the Freudian theory of the complex and of repression; and refers to it more than once. He shares the common mistake of supposing that Freud taught that repression is dangerous always and for everybody, despite the fact that Freud has protested that he never did say this, and calls for any statement of his that would justify the misconception. It is, of course, only in the case of neurotics that bad results may follow; but the neurotic is out of luck if he does repress and also if he does not. Normal people at Bates College and elsewhere will go on successfully repressing, as they have always done; and this only fear that ruffles the author's emotional calm, he may safely dismiss.

The last statement, however, may not be quite true, for the author does show some agitation over modern art-movements, such as Cubism and Futurism. He calls them psychological lies, because man, so he says, can never be apprehended in terms of cubes and angles. He combines with what seems to be a mimetic view of art another that seems to be conceptualistic, for the exact lineaments of great men or women portrayed in art mean little, unless they typify something more profound and universal than just one more member of the human species.

Consequently, the author would have to withhold the designation *aesthetic* from any experience that consisted in one's absorption in a painting that did not, to use his words, lift the mind to Olympus and to fellowship with the gods; even if it did absorb the individual in some intriguing geometrical representation of the human form, so that he dropped his load of worldly care and enjoyed surcease for a time from the serious concerns of life, and all his mental processes were congruous with one another, and he did not have to do anything about it, except to enjoy the resulting repose. This latter view of the aesthetic experience, and of a reason for seeking it, would be to him unsatisfactory and even anathema, for the reason that art must make us noble.

Knox College R. T. WILTBANK

Die Psyche der weiblichen Jugend. By Else Croner. Pädagogisches Magazin. Lagensalza, Herman Beyer & Sons, 1930. Pp. 92.

In this volume the author, representing the intuitive-historical school of psychology founded by Eduard Spranger, supplements the latter's "Psychology of Youth" with valuable studies of female adolescence in several social strata. She compares puberty in the human species to revolution in the state and notes that the Crisis is more profound in woman than in man. After discussing the limits of the conception of adolescence, the author describes with great insight, the five principal psychological types among young women studying in higher institutions of learning. These types are characterized as: (1) Maternal, (2) Erotic, (3) Romantic, (4) Practical or Matter-of-Fact, and (5) Intellectual. The so-called Social type is regarded as a special form of the Maternal type, while the Artistic derives from the Romantic. In a section on the problem of individuality, the relation of the young girl to art, religion, love, and friendship is sympathetically interpreted in the light of a contrast between masculine and feminine attitudes toward life—woman's sense of fate versus man's sense of freedom. This leads up to a study of the sociological relationships of young girls within the home, school, profession, and society as a whole. Part I closes with a statement of the general laws governing mental and cultural development in girls. It appears that the sexes differ considerably in their response to the puberty situation. The period affects girls more profoundly. It does not result in intellectual retardation. A half tragic sense of devotion and willing sacrifice in the interest of the race appears and is peculiar to woman. Mental stagnation generally sets in at the beginning of the 20th year.

The second part of this work treats briefly the psychological development of adolescent girls of inferior social status—servants, factory workers, pupils of trade schools, etc. The same types are found here, but less clearly defined and the crises of the developmental years are less severe. An interesting classification of female domestics is presented: viz., (1) a submissive, trustworthy type perfectly adapted to their calling, (2) a reliable, but somewhat arrogant and militant type (Küchendragoner), (3) a flighty, vagrant type (Vagantin), and (4) the indifferent—along with sub and pseudo-types.

A third part offers a description of psychopathic types among young girls, which will be of considerable use to educators. The book closes with a characterization of the modern girl and the perils as well as advantages which surround her as a result of recent liberal educational methods.

Croner has a fascinating style and illustrates her principles by well-selected examples from the artistic productions of youth or of novelists who portray the life of youth. No work could create a more sympathetic atmosphere for the study of the modern girl. There are two bibliographies including in all 69 titles.

University of Cincinnati CHARLES M. DISERENS

Leistungsfähigkeit und Selbstbeanspruchung: Eine psychologische Betrachtung. By OTTO LIPMANN. Leipzig, J. A. Barth, 1932. Pp. 38.

This psychological essay is largely devoted to a painstaking clarification of a number of concepts important for those interested in the theoretical or practical aspects of the "science of work" (Arbeitswissenschaft). Lipmann has devoted years of effort to the elaboration of methods and concepts for use in a scientific approach to the problems of the human factor in economic life and has published only recently a comprehensive review of the entire field in his "*Lehrbuch der Arbeitswissenschaft.*" The present study touches upon some of the topics elaborated in his book and brings out in bold relief essential connections and relations among such things as "fitness," "fatigue," "willingness-to-work" (Selbstbeanspruchung), "practice," and "sleep."

In a brief introduction, the problem is defined in James' terms, where James speaks in his "Energies of Men" of the importance of periodic fluctuations of man's fitness and 'energy,' and where he deplores the lack of adequate information on the subject. Lipmann demonstrates in his essay what progress has been made in our knowledge of these things since James' statement in 1906. The first two sections are devoted to the definition of terms and the third section to conditions of varying "willingness-to-work," such as the subnormal condition in depressive swings, or the importance of the habitual "willingness-to-work" as a distinctive trait of one's personality make-up. A brief consideration of phenomenological and dynamic differences of "fatigue," "inertia," and "laziness" follows. The interdependence, in normal conditions, of the individual's "readiness-to-work" and the number and quality of the tasks at hand is emphasized. The last section of the study deals with reactions of the organism, which produce a compensation for energy expenditures, such as "compensatory occupations," various ways of finding "recreational outlets," and "sleep."

The author is able to combine a precise usage of the technical terms with a nicety of style that makes for enjoyable reading.

Johns Hopkins University MAX F. HAUSMANN

Der Aufbau der Farbenwelt. By DAVID KATZ. Leipzig, J. A. Barth, 1930. Pp. xviii, 484.

This is a revision of the author's earlier book *Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung,* published in 1911, and, like the former volume, it is dedicated to his teacher, G. E. Müller. After an introductory chapter on the modes of appearance (*Erscheinungsweisen*) of colors and the phenomenology of illumination, Katz reports experiments with film colors, surface colors, and transparent colors. Though these first sections of the new book follow the general outline of the earlier volume, much of the material is new. A few of the later chapters are wholly new. In discussing the application of color phenomenology to the art of painting, the author points out that aerial perspective is due not to the actual changes in the color tone of distant objects, but to the vagueness of their contours and the indistinctness of their details. In other words, the more distant the object, the less it appears as surface color and the more nearly it resembles a film color. Many old masters (Raphael, for example) failed to realize this and painted distant features of the landscape with the same sharpness of detail as objects near at hand. Some modern painters have shown great skill in indicating depth by the varying softness of contours, and have even taken account of the effect of accommodation.

Psychological problems involved in the technique of depicting illumination are also interestingly discussed. Subsequent chapters deal with color constancy and color contrast, measurements of the perception of illumination, and genetic psychology of color perceptions, including experiments with animals and children, and observations upon eidetic subjects. The final chapter discusses the color theories of Helmholtz, Von Kries, Hering, Bühler, Gelb, and others; and summarizes some of the author's own contributions to the field. Especially interesting is his discussion of the conditions under which chromatic illumination is perceived.

The book is generously documented with references to the research of other investigators. About 230 authors are cited.

West Springfield, Mass. MABEL F. MARTIN

Das Problem der Schülerbeurteilung. By A. BRUCKNER. Pädagogisches Magazin. Langensalza, Hermann Beyer & Sons, 1931. Pp. 90.

The author endeavors to find the relationships among three different measures of general intelligence taken on 200 students in the four upper classes of an "Oberrealschule" (approximate ages 14–19 yr.).

The students were asked to rank their fellow pupils according to general intelligence, and were warned in the instructions not to consider characteristics supposedly not included in general intelligence, such as ambition, industriousness, attentiveness, moral standards, and so forth. Students judged to have equal intelligence were to be given the same rank number. Teachers (about 6 or 8 for each student) were given essentially the same instructions. Along with the ratings, teachers and students were also asked to give some remarks explaining the reasons for their judgments. Two tests were selected as tests of intelligence: (1) a completion test from Tumlirz, in which 29 verbs were missing, and (2) an absurdity test. The criterion for scholastic attainment was the average mark at the end of the school year.

The following main correlations were obtained by the rank method (Spearman footrule): (a) between judgment of teachers and of students, 0.85 ± .08 to 0.97 ± .03; (b) between scholastic attainment and judgment of teachers, 0.85 ± .07 to 0.93 ± .03; (c) between scholastic attainment and judgment of fellow students, 0.64 ± .13 to 0.91 ± .04; (d) between test and judgment of students, 0.38 ± .15 to 0.69 ± .10; (e) between test and judgment of teachers, 0.27 ± .17 to 0.62 ± .10; (f) between test and scholastic attainment, 0.26 ± .15 to 0.60 ± .10.

The rather high correlations between ratings of teachers and students are consonant with most results from pooled ratings of 6 to 7 raters, but only when both groups have been influenced by common social environment (reputation). Although the correlations between test intelligence and the two factors, ratings and grades, cannot be directly compared with similar correlations obtained in American investigations because of the differences both in methods of grading and kinds of tests employed, yet it seems that the correlations are relatively low, a fact which may be due to the failure to use a more comprehensive standard battery for measuring general intelligence. In regard to the method of rating, more accurate modern technique might have been employed.

It is to be regretted that the author seems not to have had access to the rather extensive and important American literature in his field. The bibliography of 26 titles contains not a single American reference. A study of the above type might have been made infinitely more useful to science if pains had been taken to secure an international outlook over the field.

Mooseheart Laboratory for Child Research MARTIN L. REYMERT

Sexualpathologie, für Mediziner, Juristen, und Psychologen. By V. KAFKA. Leipzig and Vienna, Franz Deuticke, 1932. Pp. 170.

This book is developed from the author's lectures to his medical students but these have been modified to suit the work also to psychologists and lawyers. The author is a serologist who has taught psychiatry and his leanings are largely in the direction of organic psychiatry rather than toward psychology. Nevertheless, there is much in this book to commend it to psychologists. It is a straightforward treatise, relatively modern in its attitudes toward sexual pathology, and the arrangement is such that the newer viewpoints come to the fore.

In many books of this sort quantitative changes in the physiological sex act are not deemed worthy of discussion, but in the present work there is a chapter dealing with such variations as the speed of reaching the climax or components, in so far as such changes are related to normal effects of age. Other physiological variations in function are emphasized in such conditions as early sexual development and nocturnal emission. Changes, such as premature puberty, which might be either normal or abnormal, are carefully differentiated according to the features which might indicate abnormality. There is also a discussion of the variations in the partnership in the sex act.

The body of the book is made up of conservative discussions of masturbation, the various forms of sexual inversion, and such pathology as exhibitionism, sadism, and fetishism. These disorders are analyzed largely from the historical point of view and also from the point of view of the person who might deal clinically with them. The psychological changes which accompany the sexual abnormality, and the significance of the abnormality itself are not touched upon to any great extent. The use of psychoanalytical treatment is not stressed. Therapy is dealt with in single sentences, such as "in such cases there is a need for deep therapy." The legal comments are brief and give us little psychological insight into the juristic attitudes involved.

For psychologists who are not familiar with the literature of this field and who wish a safe and sane description of sexual psychopathology the earlier chapters of this book will probably be found to be of most value. One-third of the book is occupied with a discussion of the sexual changes accompanying the psychoses, the sexual picture presented by criminals, and the organic complications found in sexual pathology. The psychological material in this part is scarcely worthy of the attention of any other than a medical man.

Institute of Juvenile Research LOWELL S. SELLING

Über das hierarchische Prinzip in der Natur und Seine Beziehungen zum Mechanismus-Vitalismus-Problem. By EWALD OLDEKOP. Reval, E. Wassermann, 1930. Pp. 64.

This is a theoretical analysis and discussion of the problem. The author distinguishes between "physical *Gestalten*" and "organic *Gestalten*" or unities. The former is strictly dependent on its outer "topography." If this topography is changed there is invariably a change in the physical *Gestalt,* which is mathematically calculable; but an organic *Gestalt* (while also not *independent*) is not so directly dependent on these external conditions, since these conditions may vary to certain extents without effecting corresponding changes in it. A physical *Gestalt* for similar behavior or functioning might well have had any one of an infinite number of other forms, but an organic form (*e.g.* an organism) "carries its end within itself" (in its germ-cell determiners) and the development of its several parts are somehow regulated by this 'end' or inner factor. That is, the system 'strives' to attain a certain predetermined structure independently of outer conditions.

Objectively considered, one might add, this difference would tend to become a question of the relative immediacy or remoteness of controls by external factors and cannot be regarded as proof of vitalistic (or in psychology of animistic) assumptions in the sense of original forces, whatever that may mean. Even all human purposes are outcomes of certain conditions. The author points out that in the organic unities, which evidently are determined so largely by the environment of early phylogenetic forms, the present environment acts more indirectly and allows room for interplay of trial and error impulses (preceding the overt acts), thus permitting what might be called intelligent adaptations. There is, unfortunately, no adequate consideration of much recent work—by C. M. Child, S. T. Bok, M. Minkowski, et al.—on physiological gradients and consequent regulation of nerve-fiber growth as affected by the requirements of adjustment to external conditions of light, food, etc.

There is no index. J. P.

Zur Psychologie des musikalischen Gestaltens. By JULIUS BAHLE. Leipzig, Akademische Verlagsgesellschaft, 1930. Pp. 102.

Although the experimental controls do not meet the demands of the most rigorous researches of the laboratory, this study marks a forward step in the analysis of creative ability, especially in the field of music. We have first a scholarly and systematic presentation of previous approaches to the subject of musical creation and composition. The speculative method has contributed little to aesthetics and the meditations of artists and aestheticians give us little more than mythical interpretations of inspiration and embody the error of generalizing from one instance which does not possess the first ear-marks of adequate sampling. We are here reminded of the early diatribes of Wundt and others against uncontrolled self-observation. The empirical-historical techniques evaluated by Stern and the interpretative historical point of view of Dilthey and his followers are merely mentioned in passing, to be relegated to the realm of the unreliable and incomplete. The pathopsychological treatments of Prinzhorn and Tramer show much more promise but are not specific enough in details. The theoretically psychological and the interrogatory methods point all the more to the advantages of a straightforward experimental technique with the aid of other methods by way of interpretation and suggestion.

The task was set to 13 *O*s, almost all of whom had already composed music more or less extensively, to express themselves musically in terms of eight problems: (1) their momentary mood, (2) some personal experience of an emotional sort, and the emotional complexes of (3) hilarity, (4) sadness, (5) suffering, (6) pride, (7) anger, and (8) hate. They were also to represent in musical characterizations, (1) a 'skin flint,' (2) a psychoesthenic, and (3) a conjurerer. They were to set down their mental processes.

In the progress of the experiment it became apparent that the *Aufgabe* to express certain emotions did not lead exclusively to this result, and the same principle applied to the problem of characterization. The observer fashioned at first a concrete situation from which to lead off some definite emotional experience. He then tried to translate this into musical form. This is designated as the method of emotional empathy or transformation. Another procedure was to arouse a complex of ideas or elaborative experiences which should indicate or represent the solution of the problem. A third method was to imagine a situation in which the observer was certain to feel the emotion called for. The writer goes on to elaborate on these three procedures and to quote extensively from the writings and biographies of noted composers to substantiate his findings. By means of diagrams the author shows how the composer draws upon musical forms which have been associated with similar feelings in past experience. Some of these are integrated, some are abstracted, and some are isolated reproductions of Gestalt factors. The monograph is rich in allusions to musical works and shows a comprehensive understanding of both psychological and musical problems.

University of Iowa CHRISTIAN A. RUCKMICK

Die Grundlagen der Charakterologie. By TH. ZIEHEN. Langensalza, Hermann Beyer & Sons (Beyer and Mann), 1930. Pp. 372.

Les Principes de la Caractérologie. By LUDWIG KLAGES. French translation by W. Reol. Paris, Félix Alcan, 1930. Pp. 263.

The book by Ziehen consists of a series of lectures on characterology delivered at the University of Jena. The lectures attempt to cover a wide variety of interrelated topics, and to summarize the present state of knowledge in a still very uncertain and controversial field. Developmental stages in character, sex differences, the relation between character and constitutional type, the character of the criminal and of men of genius, racial and occupational differences, the study of physiognomy and handwriting, and the experimental approach, are among the topics which receive detailed, though not always very critical, consideration. There are also a number of suggestions as to practical methods of controlling character, methods very strongly reminiscent of the attitudes of an old-fashioned Puritan schoolmaster. The writer's personal contribution comes from his wide experience with pathological material, and his discussions are illuminated by case studies illustrating character trends in various clinical conditions. The quantitative studies which have occupied the attention of American students of character receive very inadequate consideration. The book is worth reading, however, as a convenient summary of the results of the more qualitative approach to problems of personality and character.

The first edition of the book by Klages, "Prinzipien der Charakterologie," appeared in 1910; the present French translation is of the 1928 edition. In the last twenty years Klages has achieved considerable prominence in the field of characterology, his excursions into graphology earning particularly wide attention. To the objectively-minded psychologist, however, this book will be something of a disappointment, as it is concerned mainly with metaphysical foundations, rarely tempered by anything resembling an empirical approach. Even the metaphysics has a somewhat antiquated Scholastic flavor; the discussion of the relative contributions of "Soul" and "Mind" to individual differences in personality seems more than a little out of date. Some of the other writings by the same author have made more substantial contributions to characterology as a potentially "positive" science.

Columbia University OTTO KLINEBERG

Psicologia dell'età evolutiva (Infanzia e adolescenza). By ENZO BONAVENTURA. Lanciano, Giuseppe Carabba, 1931. Pp. 251.

This is an uncritical treatment of child and adolescent psychology, which does not assume a knowledge of elementary psychology. The author gives in the preliminary part of the book (about 45 pages) the basic facts and principles of elementary psychology. The rest of the book is divided into two parts: the first deals largely with the genesis of various mental functions, indicating roughly the periods of their appearance and the nature of their development. The second part outlines the nature and use of certain intelligence tests selected with especial reference to the general purpose of the book. A good deal of space is given to the description and clarification of certain psychological facts and principles. In a brief outline of the methods of psychology and psycho-pathology the introspective method is regarded as imposing obstacles to mental behavior in that one is requested to be both the observer and the observed. "If you are observing your fear you are not afraid; if afraid, you cannot observe." This is of course intended only as an example of the difficulty in a selected field; a critical evaluation of introspection is not attempted. For his present purpose the author prefers a 'dynamic-functional' approach. Development is best understood by objective, genetic studies. In Part 2, though older divisions in psychology are mainly used, the treatment is more modern in a number of respects than would be supposed from the chapter headings, but there is something of a dearth as to experimental facts. This may be excused on the basis of its elementary nature.

The author, in Part 2, after a general introduction on views and methods of evaluating intelligence, reproduces the various tests in the Binet scale (1911), the Kuhlmann for early infancy (from the 3-mo. to the 2-yr. levels inclusive), the Stanford-Binet revision by Terman, and he also refers to the testing work of Bobertag, Decroly, and Burt. The De Santis tests for abnormal children and interrogations (concerning children's interests, sentiments, orientations, associations, imaginations, desires, etc.) are also included, and the 47 questions by Ferrari and Francia (1912) are reproduced. From these questions, it is pointed out, one does not get a measure of intelligence; but they afford useful information and help to the teacher. In an analytical examination of the mental functions, various tests of these supposed functions are described. These include the Porteus Maze (learning) tests, certain absurdity tests (solution of problems), tests of memory, imagination, etc. Some of the absurdity tests would be new to American readers. Profile tests are also considered and discussed. All this material is presented, of course, without much critical evaluation. There are appendices on the examination of the senses and on the calculation of correlations.

Very few references are made to American authors and work, despite the many contributions of late years to childhood and adolescent psychology. Experimental work on learning is sadly neglected in the book. The 3-page bibliography at the end includes from America only Terman's *The Measurement of Intelligence,* and Thorndike's book by the same title.

J. P.

The Psychology of the Inventor: A Study of the Patentee. By JOSEPH ROSSMAN. New and Revised Edition. Washington, D.C., The Inventors Publishing Co., 1931. Pp. x, 252.

The title of this book is somewhat misleading; there is really only little psychology in the book. The first and second editions both appeared in the same year. In the latter edition the format has been changed and some corrections and additions made. The author contends that one of "the outstanding characteristics of our civilization is its complete dependence on invention." This view is justified by his extremely broad meaning of "invention," to include all novel productions along lines of human interest and not simply in the technical field. He rightly attributes the confusions of thought relating to *invention* to its many and vague meanings. Legally an invention has a rather restricted meaning: new principles not connected with mechanisms for application to practical purposes, "purely mental" things, mere ideas, systems of doing business, principles of nature, etc. are not patentable (p. 23). The list of things patentable is, however, extensive and cannot be reproduced here. Invention, legally, does not arise from reasoning or from any logical deduction, neither is "the exercise of ordinary mechanical skill" invention. To quote from the decision of a judge: "But if we create not only by the operation of mind, but as well actually, physically, new means by which is necessarily obtained a certain specific end, means which are novel to the creator as well as to the world, which had never existed before, at least in the combination or in conjunction in which the creator causes them to exist for the first time, he who does this must, I think, be regarded as an inventor" (24 f.). Indeed definition of invention is so difficult that the courts have had to adopt the opposite method, that of defining what is not invention. To determine whether some one's creation is an invention has therefore become a very complex and expensive process. If an invention is patented, there are so many pitfalls to success, including clever tricksters who pretend to be interested in the patent and for some named sum and plausible reasons can help one get it placed or secured to the would-be purchaser, that most inventors find the business unprofitable. Consequently, with this increasing complexity, patents seem to be granted now mainly to engineers in the various industries and to engineer executives of such industries, and to consist more and more of improvements of a few older great and striking inventions. The government has evolved no ways of protecting the patentee: that burden must rest on himself.

Through questionnaires (unfortunately of a too general nature) to inventors, and from replies from 710 inventors with an average of 39.3 patents, the characteristics, methods, mental processes, occupations, motives, heredity, and training of inventors are investigated. Nothing that is probably not known results. There seems to be no special endowment, except a degree of general intellectual ability, revealed; this ability probably tending to run, as is well known, in families. It is clear that inventors know very little themselves about requisite abilities and even motives for invention. Unfortunately no detailed first-hand study of any inventive process is given. Such studies are certainly needed. Data of the desirable sort are only approximated in the report of one inventor—W. H. Smyth (63 ff.), working on the problem of soldering both ends of fruit cans simultaneously in the time previously required for one can; and these data certainly do not support the legal conception of an invention quoted above.

The author seems sound in his conclusion that there is no special sort of genius in the usual inventor, and that the procedure of invention is noting some defect in operation, some imperfection or some "need," which he sets about improving or supplying by a more or less trial and error method (even though he may have good technique within certain limits). Solutions often come about rather suddenly as if by chance observation of some principle in operation.

Due to the equipment of the author, and his extensive experience as Patent Examiner in the U.S. Patent Office, the book contains much information of particular interest to young inventors, and to research workers generally. The psychological theories of invention are stated very generally and are well known to psychologists. The interesting experimental work on invention begun by Josiah Royce, and extended to work on puzzles by E. H. Lindley (this JOURNAL, 8, 1897, 431 ff.) is not mentioned, even in the 10-page bibliography.

J. P.

The Measurement of Interest. By DOUGLAS FRYER. New York, Henry Holt & Co., 1931. Pp. xxxvi, 488.

The author has taken a very limited psychological field and attempted to survey everything that has been done in it. This effort is commendable and, the reviewer believes, has been eminently successful. Anyone wishing to acquire a background in this field of research will not have to seek any further.

The point of view of the book is essentially empirical. Fryer is chiefly concerned with the measurement of interests rather than with their theory. After differentiating between the subjective and objective aspects of interest, he treats of them separately. Then turning to more theoretical considerations the author points out sources of error in interest inventories and in the objective measurements. He proposes his own theory along the lines of acceptance-rejection, on the basis of which he distinguishes between interest and motivation. He then outlines a program of research, during which he points out the possibilities of material obtained in autobiographies and clinical examinations.

The book closes with five appendices. The first sketches the historical trend with regard to interest in education and industry, and especially the latter; the second treats in similar fashion of subjective interests; the third and fourth give scoring keys for various interest inventories; and the fifth and last deals with the interests of several occupational groups.

The book is not of the type that will be read carefully from cover to cover, but is more like a handbook which the reader will sample according to his problems. The vocational counsellor will find the results of numerous studies in the vocational field and suggestive schedules for conducting interviews. The educational psychologist will find material on children's interest. The industrial psychologist or personnel worker will get the historical background of his field and will find many pertinent suggestions for measuring interests. The person devoted to research can take this book as a starting point, and by use of it avoid many mistakes that have been made by others.

Ohio State University HAROLD E. BURTT

Association Theory To-day: An Essay in Systematic Psychology. By EDWARD S. ROBINSON. New York, Century Co., 1932. Pp. x, 142.

The subject of this book, association theory, is a central one no matter what one's special interest in psychology. Here is a treatment of it which is eminently sane yet provocative of thought and of reconsideration of one's own position. The style is facile and bright, in general barely avoiding the colloquial; occasionally it rises to the epigrammatic. Only the lack of a certain distinction keeps it from being really first-rate prose.

Robinson does not give us a compendium of experimental facts such as in other fields we have had from Fröbes and from Murphy. Critical experiments, it is true, are cited at all times and experimental data are in the center of the author's eye—possibly too largely to the exclusion of non-laboratory experience. But it is, rather, a critical essay, chiefly dialectic. This statement, of course, will condemn the essay forthwith in the eyes of those who do not realize the polar position of speculation in scientific advance. Robinson's treatment, none the less, will help, both to give meaning to known facts and to disclose new ones.

There are two attitudes commonly taken toward an historic theory like associationism. Köhler, I think it is, has somewhere expressed one of these very clearly. No one, he says, will hold this theory as I have stated it, but many persons *should* hold it because it is the central position from which radiate the doctrines they have actually espoused; and unfortunately this central position is, as anyone can see, quite absurd. Effective as this sort of argument is, it degenerates easily into the playful kicking of the stuffings from a straw man.

Robinson takes the opposite tack. He, too, tries to find a central position, a sort of greatest common denominator for associationism. This common denominator, however, is so sensible a thing it is difficult to see how anyone can object to it. "By association," he says, "we may mean simply the establishment of functional relations among psychological activities and states in the course of individual experience."

But of course each reliance upon a common denominator also has its weakness. Association certainly means that, but it also means more or we should all be associationists. Associationism is surely neither palpably absurd nor yet so sensible that none can deny it. Robinson himself proceeds at once to such concrete specifications as leave ample room for differences, and rightly so since it is doubtful whether a theory which cannot be denied has any meaning or value.

The reviewer admits to a marked feeling of impatience with associationism chiefly because of its relative sterility in practice. The doctrine is old enough by now to have borne more fruit. Perhaps this is because we have taken it seriously but not critically. Robinson proposes to take it critically and see what he can do with it. If the resulting systematic position is not strikingly original and therefore unlikely to arouse intense loyalties, it is a vigorous position which keeps close to the central tradition in psychology. Whether we supplant or supplement associationism, we shall do so more intelligently for this essay.

Ohio State University HORACE B. ENGLISH

Psychology for Advertisers. By D. B. LUCAS and C. E. BENSON. New York, Harper and Brothers, 1930. Pp. xv, 351.

The authors' aim, stated in the preface, is "to present the theoretical, though scientifically supported, psychological aspects of advertising. There are six parts, dealing with the "mental processes and natural tendencies of the customer," the "psychology of appeals," how to "construct the advertisement," the several "mediums for carrying appeals to customers," the "measurement of the effectiveness of advertising," etc. One chapter is given over to a discussion of "human differences," and the final chapter to "advertising and the thinking of the public."

The book is strongest where the authors' plan of presenting the experimentally supported psychological aspects of advertising is adhered to most closely. It is weak in the numerous instances of assertions made dogmatically with no hint to the "practical person" that corroborative evidence on these points is lacking. Magazine advertising is taken as the type exemplifying the modern trend in advertising because it "has made great gains in public approval and prestige. The advertisements have been improved in appearance, content, and in their wholesome frankness." Thus the stamp of "scientific" approval is placed on the advertising of such nostrums as those which claim to be panaceas for unpleasant breath, 'gingivitis,' athlete's foot, and dandruff.

The gratification of the authors in the "practical success" which advertising has enjoyed is evident throughout the book, and is expressed in the closing paragraph, which conveys the general tone: "Besides being responsible for the widespread reading of periodicals and other printed matter, it [advertising] has placed the radio in millions of American homes. For ten years a nation has thrilled at the magic of wireless and constant entertainment, all because industry is willing to supply that entertainment for the 'good will' we may reflect."

University of California at Los Angeles FRANK C. DAVIS

A Comparative Study of Those Who Report the Experience of the Divine Presence and Those Who Do Not. By ROBERT DANIEL SINCLAIR. University of Iowa Studies: Studies in Character. Iowa City, The University, 1930. Pp. 62.

The purpose of this study is to bring to light the hidden mechanisms that condition the mystical experience of those who report the "experience of the Divine Presence." For the solution of this problem a means had to be devised for the selection of two groups: the Positive, or those who had had such experience, and the control, or Negative group, those who had not had it. This took the form of a point-scale self-rating test in which the rater indicated the degree or quality of his experience by checking to the right or to the left of a colon at the center of two extremes, and also the degree of presence or the total absence of trait by the same checking device. A check on the reliability markings was made by using the chance-half method of correlation with the eight significant items for the whole distribution of 549 cases. The resulting correlation was 0.86 and, according to Brown's formula, the test as a whole had a statistical reliability of 0.92.

Fifty students representing the extreme mystics and fifty representing the extreme non-mystics were selected from the results of this point-scale self-rating test. These two groups were then each given a battery of tests to determine experimentally the differences between the groups in sensory characteristics, motor abilities, intellectual traits, and suggestibility.

The sensory tests consisted of measures of visual discrimination, weight discrimination, sensitivity to electric current, and auditory discrimination. In all these tests there were found no significant differences between the groups being studied.

The motor abilities were measured by a simple reaction-time test (Klopsteg chronoscope), a test for the rate of continuous movement, a test of muscle steadiness, of muscle coördination, of serial reaction-time, and of tracing mazes. Of these tests, those which involved little more than simple motor reactions showed no significant differences between the two groups; but when complicated reactions were involved, the Negative (control) group was markedly superior.

Other tests were the memory of the details of a picture, an Aussage test of memory, a slightly modified Woodworth-Wells direction test, a test of ethical discrimination, of abstract judgment, of spatial imagination, and one of intelligence. The average grade-points for the previous semester were also used for comparison. In every one of these, except the grade-points, the Negative group was far and reliably superior to the Positive group. In the study of suggestibility to electric current and to pictures, the members of the Positive group were much more open to leadings and promptings than the Negative group, the difference being reliable. Further study of the self-ratings by the point-scale indicated reliable sex differences and an interesting predominance of certain religious beliefs in the Positive group.

Peabody College MABEL F. NICHOLS

Educational Psychology. By WILLIAM CLARK TROW. New York, Houghton Mifflin Co., 1931. Pp. xv, 504.

Trow organized his *Educational Psychology* around a series of psychological nodes—"inherited nature, individual differences, and the learning process"—and the principles developed are briefly applied to educational situations as the story of adjustment unfolds. Methods have not been described in detail, "for that," Trow claims, "is not the function of educational psychology" (p. 116).

The book includes a wealth of quantitative material in tabular and graphic form, so skilfully handled that the tables and figures do not encumber the exposition nor burden the reader with their weight. Many other features of the volume are no less commendable. It admirably reflects the skeptical attitude of present day science toward the concept of instinct; it presents in concise form the recent contributions of psychoanalysis to individual adjustment; shows clearly the present confusion regarding the nature of intelligence; describes the theory of rational learning of Gestalt Psychology; and it gives the results of the recent researches on the problems of growth.

The chief points of criticism are his implicit faith in the constancy of the IQ, his views of the influence of heredity which are still deeply dyed in Galtonism, and his basis for interpreting (as well as his method for computing) the coefficient of correlation.

Trow writes, however, with "the sweet insouciance of lettered ease." With the results of investigations at his elbow, he deftly and lucidly pictures the process of adjustment, and brightens his exposition with happy touches of his unfailing humor.

University of Chicago FREDERICK S. BREED

The Heart Rate. By ERNEST P. BOAS and ERNEST F. GOLDSCHMIDT. Springfield, Ill., Chas. C. Thomas, 1932. Pp. xi, 166; 68 fig.

This monograph emphasizes the necessity, in any adequate study of the heart rate, of making continuous records of the heart beats while the subject pursues his ordinary activities as well as when he rests and sleeps, and of making these records by some method which leaves the subject unaware that he is being observed. For these purposes Boas has constructed the cardiotachometer, described and pictured in the second chapter. (This instrument is now being manufactured by the General Radio Co., Cambridge, Mass.) The study reported is based on continuous observations, "in each case extending over many hours," of the heart rates of 356 adult persons, divided equally as to sex in the several groups. Records are presented of 103 normal persons, of 77 persons during anaesthesia and operations, and of various patients with cardiac insufficiency, valvular diseases, myocarditis, Graves' disease, neurogenic sinus tachycardia, auricular fibrillation, and complete heart block. The book begins with a short historical sketch of the development and techniques in the investigations of heart rates from the remarkable studies of Herophilus, the Alexandrian physician of the 3rd century, B.C., by means of an especially constructed water clock, to the present time.

Results by the present authors reveal variations of so enormous proportions in the normal subjects (range of ages 20 to 40 yr.; mean age of the 51 men, 28.1 yr., and of the 52 women, 24.6 yr.) that the question may well be raised as to what is really an abnormal pulse rate. The basal heart rate (taken in the "post-absorptive resting state, early in the morning, shortly after awakening, and usually before rising," p. 22) is compared with mean, maximum, and minimum rates during waking and also during sleeping hours. For men the means are 61.4 ± 8.22

(basal) and 77.8 ± 9.43 and 59.4 ± 7.50; for women 69.9 ± 9.44 (basal) and 83.9 ± 7.80 and 65.3 ± 7.12, respectively. Numerous other conditions of which the pulse rate is a function are considered, and results are frequently compared with earlier measurements. Numerous graphs show changes in heart rate in normal and diseased individuals through periods of many successive hours. The book is valuable to psychologists investigating interrelations of various organic changes. Detailed data on individual cases are given in the appendix. There is a bibliography of 180 titles.

J. P.

Naturgeschichte der Seele und Ihres Bewusstwerdens. By EUGEN BLEULER. Revised ed. Berlin, Springer, 1932. Pp. xxvii, 268.

The subtitle, in English translation, is "mnemistic biopsychology," and perhaps conveys a clearer meaning than the main title. The long preface asserts that the book is not essentially different from its first edition. Psychology is introduced as a branch of biology correlated with "the remainder of" physiology and pathology. The author claims to have been the first to reject the kind of "localization" proclaimed by Meynert and Munk, and to substitute the modern neurological view of a purely acquired localization of function in any individual's nervous system. For what kind of reader the book is written, is difficult to understand. Certainly not for the immature college student. The style is conversational and the topics discussed are such as might become the subject of a discussion between two psychiatrists meeting accidentally in a club. This is not said in order to belittle the book, but to make clear what the reader may look for when he opens it. For example: "The 'psyche' is a brain function; and a borderline between the 'psyche' and brain functioning cannot be drawn." Those who are interested in "insight" will find a discussion of "the significance of consciousness and insight" to their liking. Other important topics are memory, perception, abstraction, illusion, intelligence, logical necessity, space, time, psychical energy. The book is dedicated to the memory of Goethe, deceased just a hundred years ago. This also helps to characterize the book for the prospective reader.

University of Missouri MAX F. MEYER

Psychologie. By ALOIS HÖFLER. 2nd ed. Edited by Aloys Wenzl. Vol. I. Vienna and Leipzig: Hölder-Pichler-Tempsky A. G., 1930. Pp. xv, 642.

The first edition of Höfler's *Psychologie* appeared in one volume in 1897. The manuscript for this new edition in two volumes was nearly completed when Höfler died in 1922. Wenzl has brought the work into its final form and added notes of his own. In the late nineties the work was widely known, not only because psychological treatises were then few, but also because it represented an authoritative account of the Austrian act-psychology which was then exerting great influence. The general systematic position of the new edition is the same, although there are shifts of emphasis. At the time of the first edition, the aim was to combat the dogmatic generalizations of associationism. Now, with associationism no longer a serious problem, the author is faced with the task of resisting the increasing wave of *a priori* and intuitional thinking which has swept over German psychology.

For Höfler the four basic classes of mental phenomena are ideations, judgments, feelings, and desires. The first volume of the new edition deals only with ideations. Few psychological works are as thoroughly documented as Höfler's and few psychological writers have equalled his ability in imparting information at the same time simply and critically. Even the footnotes are readable. In spite of its literary and scholarly excellence, the work seems now to savor of the past. Its historical value, however, makes it a most useful work for reference.

University of Rochester C. W. Fox

Behavior Aspects of Child Conduct. By ESTHER LORING RICHARDS. New York, Macmillan Co., 1932. Pp. xv, 299.

This book contains a series of lectures delivered by the author in 1929 to the Baltimore branches of the Child Study Association of America. Although the book offers nothing new, it is valuable for its emphasis on the need for *interpreting* behavior rather than *judging* conduct. For the interpretation of behavior, no single viewpoint is adequate: the contributions of the psychologist, the physician, the social worker, the teacher, the clergyman, and the parents must all be given due weight. Psychoanalysis is dismissed as of comparatively slight value: in the analysis of psychological strains, it is usually not necessary "to resort to tricks of string galvanometer, or hypnosis, or dream analysis. The causes of strain are, nine times out of ten, to be found in a little careful scrutiny of situations at home, in school, and on the playground, and of the personalities concerned in these situations" (p. 141).

The book gives the customary emphasis to early childhood habits, the danger of discussing behavior problems in the presence of the child, etc. As in most lectures and popular presentations, the style is rambling and rather repetitious. There has been some of the usual pedagogical over-simplification of the difficulties in treating real behavior problems. The book contains an eleven-page index, but no bibliography.

University of California HERBERT S. CONRAD

Whole and Part Methods in Trial and Error Learning. By ELLA MAY HANAWALT. Comparative Psychology Monographs. Baltimore, Johns Hopkins Press, 1931. Pp. 65.

This monograph contains a report of a carefully planned and executed experiment conducted in the Michigan Psychological Laboratory on 15 trained male albino rats. Five pairs of maze patterns were used. Each maze was built so that it could be used as a single whole maze or as four separate smaller mazes, or parts, each leading into the food box. Five types of part procedure in learning were compared with the 'whole' procedure. These were designated as the pure part, the progressive part, the direct repetitive part, the reversed repetitive part, and a modification of the repetitive part. One pair of mazes and two groups of rats were used in making each comparison. The rats in one group learned one maze by the whole and afterwards the other maze by the part procedure, and the other group reversed this order, learning by the part procedure first. The criteria for comparing the results included the number of trials, the amount of time, the number of errors, and the total distance run. The last criterion was considered by the experimenter to be the most significant.

The results, expressed in both percentages and absolute measures, indicate that the whole method of learning the maze is superior to either of the part methods. The order of efficiency in this experiment for the various methods was found to be whole, direct repetitive part, pure part, progressive part, reversed repetitive part, the modification of the reversed repetitive part. Results of this and of related studies by other investigators are not in entire agreement and seem to indicate that the optimum procedure may not be the same for all kinds of learning.

Peabody College EDGAR M. COOK

Mr. Eliot Among the Nightingales. By LOUIS GRUDIN. Paris, Lawrence Drake, Covici Friede, Inc., New York, American Agents, 1932. Pp. 71.

This essay by Grudin is a critical analysis of T. S. Eliot's recent essay *Poetry and Propaganda.* Eliot is recognized as an able representative of a group of writers who oppose modern mechanistic tendencies in science and philosophy with "a philosophy," to use Grudin's own words, "consisting of aesthetic intuitions" (p. 2). Grudin charges that Eliot, by the evasiveness of his problem, is involved "in practically every quandary of philosophy." He is forced by his method to seek illustrations from the intuitions of poets, from religious feelings, and from animistic and other vague notions to justify his views. "His procedure has been that of an excursion for words and notions to support his claims, wherever he could find them"; and he "has had to trust to meanings he could read into already available odds and ends belonging to various fields and gathered into a makeshift of critical doctrine. This is unfortunately the usual procedure followed by our literary critics" (p. 35). M. Grudin is himself the author of *A Primer of Aesthetics,* and has made this analysis of Mr. Eliot's essay largely to illustrate the dialectical method which he has been interested in developing to parallel the advances made in modern scientific method.

Would not some of the problems raised in these essays make a fertile field for psychological experiment employing quantitative methods of evaluating responses to certain objective situations?

J. P.

Criminal Statistics in Iowa. By CHARLES N. BURROWS. University of Iowa Studies: Studies in Social Science. Iowa City, The University, 1930. Pp. 112.

This study covers a period of 75 yr., from 1849-1927, inclusive. There is a graph and a statistical table for each county and four summary graphs. One graph shows the average criminal rates for the entire state of Iowa; the other three present the average criminal rates for the twenty-one most urban counties, the twenty-one middle counties, and the twenty-one most rural counties. The rate of crime, both of convictions and of sentences, is, in general, highest in the urban and lowest in the rural counties. The sources of material are the reports of the Secretary of State of Iowa, in relation to the criminal returns; and the reports of the Board of Parole of Iowa, with abstracts of the criminal statistics.

It cannot be too strongly emphasized that valuable as comparative tables may be for certain minor purposes, the percentages of the population convicted for crime and of persons sent to reform schools and penal institutions have no significance at all in themselves. Methods of dealing with and preventing crime have changed markedly in the period covered and no one knows whether an increase in the percentages really reflects a worse condition than a decrease. This is not overlooked by the author.

Peabody College KENNETH B. HAIT

A Challenge to Neurasthenia. By DORIS MARY ARMITAGE. Second Edition, Revised and Enlarged. London, Williams & Norgate, 1931. Pp. 64.

This little volume gives a popular summary of the treatment of neurasthenia used by Dr. L. S. Barnes who died suddenly in 1927. It is written from the patient's point of view and is practical, helpful, and interesting. The urgent need of sympathetic understanding of the neurasthenic is stressed. The etiology of the nervous disorders considered is not dealt with other than by saying, "Dr. Barnes found the origin of all neurasthenic troubles in fear."

University of Wisconsin KAI JENSEN

Interest and Ability in Reading. By ARTHUR I. GATES. New York, Macmillan Co., 1930. Pp. xii, 264.

This is a report of further progress made by Gates and his associates in studies of materials and methods of teaching reading. The book is written in two parts. Part I presents and interprets the results from experimental studies of various factors which influence interest and ability. Part II formulates the principles which should underly the teaching of reading and gives illustrations of the methods which have been found to be most efficacious in producing the desired results.

Perhaps in no other direction have the fruits of the application of psychological analysis and experimental techniques been more gratifying than in that of reading. The rule-of-thumb and intuitional methods of an earlier day are being supplanted in our more progressive school systems by methods like those described in this book.

University of California at Los Angeles FRANK C. DAVIS

Education of the Backward Child. By DAVID KENNEDY-FRASER. New York, D. Appleton & Co., 1932. Pp. viii, 236.

This is an interesting and practical book for the teacher concerned with the education of the child having an I.Q. between 50 and 80. It is the outcome of many years of experience in the training of teachers of mental defectives in Scotland. The author shows that every child should be given tasks commensurate with his inborn capacity so that he will succeed and receive the "feeling of contentment arising from suitable work well executed." Even backward children have the necessary mental energy to acquire enough of the rudiments of knowledge to become socially useful if this energy is 'canalized.' This is best accomplished by presenting work in such a manner that it will be carried out in the spirit of play. The project method is found to be very useful here. Many concrete examples are given of methods of teaching oral language, writing, spelling, reading, composition, and numbers. Many of the suggested methods would apply just as well to teaching the normal child who is beginning the study of these subjects.

Wesleyan College, Macon, Ga. RALEIGH M. DRAKE

Psychologie der Werbung. By Karl Marbe. Poeschel. Stuttgart, 1931. Pp. 132.

The first half of the book is a general psychology written in very popular style. The other half is a technical discussion of particular problems in advertizing which the author and students working under his directions happened to take up. The psychologist looking for suggestions concerning the effectiveness of show-windows and concerning methods of book selling, may find himself rewarded by a careful reading of this second half, which has the appearance of being written for a period of business depression although it was published six years ago.

University of Missouri — Max F. Meyer

Die Psychologie des Grundschulkindes in ihrer Beziehung zur kindlichen Gesamtentwicklung. By Oswald Kroh. Eighth edition. Langensalza, Hermann Beyer & Söhne, 1930. Pp. 352.

In this new edition of Kroh's well-known book on child psychology, only a few minor changes from earlier editions have been made. As formerly, there is an introductory section in which the history of the growth of interest in child development is briefly surveyed. This is followed by a longitudinal description of the general course of development during childhood and early adolescence. The third and longest section is devoted to a topical account of such aspects of the child's mental life as the awareness of self, intellectual development, observation and perception, memory (with particular reference to eidetic imagery), the development of social and religious life, emotion and its expression, etc. Throughout, the significance of the scientific study of child development as a basis for practical education and training is emphasized. There is a short list of selected references, chiefly German.

University of Minnesota — Florence L. Goodenough

The Indispensable Soul. By William H. Crawshaw. New York, Macmillan Co., 1931. Pp. v, 315.

This book contains a goodly amount of material, apparently collected by the author over a considerable period of years and arranged by him in support of the thesis that the soul is indispensable in any adequate account of the world as a whole (including not only mechanisms and the animal mind, but also the moral, aesthetic, intellectual, and spiritual life of man). Though many of us may be sympathetic with this point of view, it must be noted that the author seems to take delight in neglecting to define his terms. There is also a tendency to vague, but emotionally attractive statements, and such scientific authority as is quoted is used in so general a manner that, even when it is authentic, nothing in particular is supported by it. Some men may prefer a shot gun to a rifle, but it is much less efficient for big game. Crawshaw is Dean Emeritus and Professor Emeritus of General Literature at Colgate University, and, as Socrates remarked of Polus, in the "Gorgias," has apparently "attended more to the art which is called rhetoric than to dialectic." The merit of the book is that it is a sincere and rather attractive confession of faith.

Hobart College — Foster P. Boswell

Psychologie der Gegenwart. By H. HENNING. 2nd ed. Leipzig, Alfred Kröner. 1932. Pp. xi, 211.

This edition differs in no essential respect from the first published in 1925. Henning gives us, as before, a brief summary of the development of the newer psychology, followed by a survey of the principal problems and points of view in more than a score of the recognized fields in which psychologists have been active. In a few cases new paragraphs have been added to bring the chapters up to date, and a chapter on characterology has been included, but the principal improvement lies in the addition of new bibliographical material. In several cases, however, citation is not of the most recent edition. In both text and bibliography primary emphasis is laid upon German contributions, and an appendix on practical problems connected with the study of psychology can be of value only to German students.

Cornell University R. B. MACLEOD

The Rôle of Kinesthesis in Maze Learning. By D. A. MCFARLANE. Berkeley, University of California Press, 1930. Pp. 277-305.

This experiment was made upon the assumption that the kinesthetic stimuli in swimming a maze are materially different from those received in running a maze. This being true, if kinesthesis is an essential factor in maze learning, as it is generally considered to be, there should be no transfer from one kinesthetic situation to the other. The usual control was employed to keep all other sensory features constant while kinesthesis only was changed.

The experimenter found that so marked a change in the kinesthetic situation as shifting from running the maze to swimming the maze and from swimming to running had little effect on the ability of the white rat to traverse the maze.

It is urged that in studying kinesthesis in maze learning, a distinction should be made between the ability of the rat to know where its food is and those elements in the situation that determine its manner of reaching the food. The efficacy of kinesthesis is doubted in the latter and denied in the former.

Recognizing the value of the method used in the experiment, we doubt the validity of the assumption upon which it is based. There are probably a number of muscles, tendons, and joints used in common by the animals in running and swimming the maze; the rat responds as a total organism, and not in disintegrated form as here implied. It is questionable if the rat 'knows' where the food box is, apart from the kinesthetic elements which enable it to determine the proper manner of reaching the food.

Southern Methodist University JOSEPH U. YARBOROUGH

Information and Certainty in Political Opinions: A Study of University Students during a Campaign. By HAROLD S. CARLSON. University of Iowa Studies. Iowa City, The University, 1931. Pp. 48.

Among the recent investigations into determinants of opinion, Carlson's contribution will rank high. It indicates that the certainty with which opinions are held by 357 students is not an index of the information possessed relative to the political questions at issue. Analysis of data obtained through rating sheets, information tests, and a questionary, yields contrasts in certainty and information among the following groups: voters and non-voters; students who formed preferences for a candidate early or late in a political campaign; one party and mixed party groups; sex; groups differentiated by Iowa Placement Examinations; and groups separated by years of academic advancement.

One of the merits of the study is adequate statistical treatment of the data. Three appendices supplement the main report; these concern, respectively, related experimental literature, the instruments used in obtaining data, and statistical considerations regarding the measures obtained.

University of Minnesota CHARLES BIRD

Attitudes of Children Toward Law. By EARL G. LOCKHARDT. University of Iowa Studies: Studies in Character. Iowa City, The University, 1930. Pp. 61.

This study is an attempt to discover the attitudes of 'modern youth' toward certain social standards of today by applying an objective test to a representative group of 3500 Iowa school children in grades 4–9. The test consisted of 51 questions based on the ethics of obeying or breaking 20 laws—more or less familiar to children—when the situations involving the laws supply a motive for disobedience. Only those questions were retained (from a longer list of 60) which received identical answers when presented to the 50 most outstanding lawyers in Iowa. The children's answers were scored "Right" if they agreed with those of the lawyers, and "Wrong" if they differed. A reliability coefficient of 0.837 was obtained by correlating odd and even halves of 100 tests of the heterogeneous group selected at random.

To check possible influences on children's attitudes toward law, Form C of Sims Score Card for Socio-Economic Status and Form A of the Otis Intelligence Test were given. Sex was also noted as a possible factor. These tests indicate (1) that children of both sexes and in all social and civic conditions have approximately the same attitudes toward law as adults; and (2) that intelligence is not a factor in determining attitudes provided the mental age is sufficiently high to make possible the comprehension of the law involved.

Board of Education, Cleveland FRANCES E. LOWELL

The Mind: A Key to the Interpretation of Psychical Phenomena. By GEORGE E. REHBERGER. Phoenix, Md., Lord Baltimore Press, 1931. Pp. vi, 214.

This work is a deluded attempt by a physician to explain all of normal and abnormal psychology on the basis of the author's analysis of a few of his own dreams. The importance of his "discovery of the mechanism of dreams" and the blind conceit with which this discovery is used as a key to psychology reminds one strongly of little Jack Horner. The author apparently knows nothing of Freud's contributions to the theories of dreams, and his acquaintance with psychological literature in general is about as broad as that of the beginning student in psychology, although psychological terms are used freely—and loosely. The book contributes nothing from a medical standpoint. Altogether it is a weird combination of misstatements and wild speculation with a minor degree of plausibility. Students in advanced classes might find criticism of the book a valuable exercise in clarifying their own thinking on some phases of psychology.

George Peabody College J. E. EARL

Jewish Experiences in America: Suggestions for Study of Jewish Relations with Non-Jews. Edited by BRUNO LASKER. Various contributors. New York, The Inquiry, 1930. Pp. xiv, 309.

This is a book of illustrative discussions, facts, opinions, attitudes, suggestions for leaders, outlines, etc., prepared by an "undenominational organization" for discussion groups among Jewish people with the purpose, apparently, of developing better understanding, better attitudes, and better general human relations among different religious and national groups in America. This book is one of a series put out by *The Inquiry* with the purpose of promoting "deeper insight into the causes of social friction and more effective methods of dealing with it." The series is necessarily of a popular nature since prejudice is usually associated with ignorance.

The book represents the results of considerable rather thoughtful experience—if not scientifically controlled experiments—and it contains valuable materials and suggestions on group psychology. There is a 22-page reading list (mostly of Jewish literature), but, unfortunately, no index.

J. P.

The Abolition of Poverty. By HARRISON E. FRYBERGER. New York, Advance Publishing Co., 1931. Pp. v, 152.

This book is written from the economic point of view and psychological aspects of the problem are neglected. Poverty and plenty are not clearly defined, and are used only as relative terms. Ten definite measures for the abolition of poverty are listed, and three others advocated. None of these measures is original, and some are hardly compatible; for example, the contradiction inherent in the advocacy of competition among independent small stores and the discouragement of competition among producers by means of a protective tariff leaves the reader wondering. The neglect of the economic effect of foreign loans likewise leaves much to be desired.

The book seems to be the result of private reflections of a rather well informed moralist. It has the merit and the limitations of such a work.

Teachers College, Columbia University HOMER E. COOPER

The Dissatisfied Worker. By V. E. FISHER and JOSEPH V. HANNA. New York, The Macmillan Co., 1931. Pp. ix, 260.

This book deals with the emotional maladjustments which handicap the individual in making vocational adjustments, however well equipped for his work he may be by virtue of his native intelligence and technical training. The emotional problems particularly stressed by the authors are: feelings of inferiority with their compensatory mechanisms, fears, and difficulties of the sexual life. It is pointed out that a large proportion of vocational maladjustments are only a part of larger emotional problems which affect the person in all his relationships as well as in the work situation. The emotional difficulties of the individual are expressed, in his work relationships, in jealousy of other workers, lack of cheerful coöperation, self-pity, complaints of unfair treatment, labor agitation, bullying of subordinates, fault-finding, frequent changes of jobs and other manifestations of discontent. In the more serious cases of emotional maladjustment, there are also more serious vocational difficulties in the form of industrial accidents or neuroses.

The nature and causes of emotional maladjustments and their effect upon vocational adjustment are briefly and simply explained, and there are plenty of case study illustrations to supplement the general discussion.

Philadelphia Child Guidance Clinic PHYLLIS BLANCHARD

The Sex Technique in Marriage. By ISABEL EMSLIE HUTTON. New York, Emerson Books, Inc., 1932. Pp. xv, 156.

In nontechnical language, and in a simple and direct style, Dr. Hutton places before her readers the psychological as well as the anatomical and physiological facts of the normal sex life. The book is written in good taste. It contains nothing of an erotic nature—except perhaps its misleading title—which may offend the sense of delicacy of its readers. The information imparted is in accordance with enlightened opinion. The book may safely be recommended to all who wish accurate information regarding the sexual life—to the adolescent as well as to those entering the married state.

K. M. D.

The Nervous System: An Elementary Handbook of the Anatomy and Physiology of the Nervous System. By JAMES DUNLOP LICKLEY. London, Longmans, Green & Co., 1931. Pp. xii, 144.

This is a new edition of the book that appeared in 1912. It contains a brief summary of the anatomy of the nervous system which differs very little from that given in the earlier edition. The physiology is orthodox and very elemental. The book as a whole may be characterized as a collection of the tenets of neurological faith, which have little value to the psychologist, and it is doubtful whether they should be taught his students.

Columbia University W. N. CHAPPELL

Der Strafprozess gegen Philipp Halsmann: Aktenmässige Darstellung und Krimnalpsychologische Würdigung. By KARL MARBE. Leipzig, C. L. Hirschfeld, 1932. Pp. 163.

A review of the evidence and of the court procedure in the case of a pathological patricide from the standpoint of criminal psychology.

Trinity College ROBT. B. W. HUTT

The Development of the Gonads in the Gold Fish, Carassius auratus (L.). By FRANK A. STROMSTEN. University of Iowa Studies in Natural History, Vol. XIII, No. 7. Iowa City, The University, 1931. Pp. 45.

This is purely a biological contribution such as would interest embryologists and histologists. Nothing bearing on sex behavior is given.

Stanford University CALVIN P. STONE

Introduction to the Use of Standard Tests. By SIDNEY L. PRESSEY and LUELLA COLE PRESSEY. Revised Edition. Yonkers-on-Hudson, World Book Co., 1931, Pp. vi, 266.

The parts, chapters, and topics are identical with those of the first edition of this well-known book, with two minor exceptions in the general contents and one in the appendices. The topic of mathematics is given the space formerly allotted to algebra and geometry, a page and a half of new material on general science is added, and Appendix E, consisting of less than four pages on the "new-type" examination, is inserted. Occasionally, in topic discussions, citations of typical tests have been changed; *e.g.* "The Gray Oral Reading Check Tests are the best example in this field," takes the place of "The Gray Oral Reading Scale is perhaps the best example of an oral reading test." The expressed purpose, philosophy, interpretation, and exhortations regarding testing are virtually the same as outlined in the 1922 edition.

Milligan College JESS H. EDDS

Soziale Seelenhaltungen. By W. STEINBERG. Munich, Ernst Reinhardt, 1932, Pp. 151.

Three basic forms of subjective life are being distinguished: Psyche, soul, and spirit (Psyche, Seele, Geist). Psyche designates the totality of subjective modes of existence exhibited by man as a natural (*naturhaftes*) living being. Soul and Spirit are put in contrast to Psyche, as peculiarly human phases of subjective existence, soul implying more the emotional, spirit the rational mode of life. Much space is devoted to a discussion of the various manifestations of these basic forms in social attitudes, such as striving for social position, love, hate, etc. The discussion is almost purely philosophical, with little or no reference to the body of knowledge of experimental psychology and sociology.

The Mooseheart Laboratory MARTIN L. REYMERT
for Child Research

A Sociological Analysis of Certain Types of Patriotism. By EARLE L. HUNTER. New York, Paul Meisel Co., 1932. Pp. 263.

This volume is a doctor's thesis. The author attempts what he terms an inductive approach to the meaning of patriotism through an analysis of excerpts from various news items, editorials, and letters to editors gathered from the press of the country. He differentiates six types of patriotism which he names respectively, loyalty to state supremacy, institutional loyalty, national egocentrism, eclectic institutional loyalty, critical-mindedness toward the state, and national self-sufficiency.

The book is mainly sociological, interspersed with excursions into the field of political science. Although the author regards his work as an inductive treatment of the data he has collected there is in his inferences a tendency toward eisegesis, and it is difficult to escape the conclusion that his real purpose is to set forth certain preconceived notions, social and political, and that he uses his data as useful tools to corroborate them.

Aside from a few references to the psychological factors of patriotism the book is of little interest to the psychologist.

West Virginia University JOHN E. WINTER

The Cardiac Output of Man in Health and Disease. By ARTHUR GROLLMAN. Springfield, Ill., Charles C Thomas, 1932. Pp. xiv, 325, 15 figures in text.

This book is written chiefly for the medical profession. An excellent review, with criticisms, is given of the various methods used in the determination of the cardiac output. One chapter deals with the physiological variations affecting output, such factors as posture, sleep, menstrual cycle, psychic disturbances and temperature being mentioned. Stimuli resulting in such psychic disturbances as resentment, regret, and anger were found to increase the output. Applications of cardiac output measurements in pharmocology, physical therapy and clinical medicine are fully discussed. A bibliography of 483 titles is appended.

University of California C. W. BROWN

Child Psychology. By BUFORD J. JOHNSON. Springfield, Ill., Charles C. Thomas, 1932. Pp. xiv, 439.

In this volume, Johnson has brought together in extremely concise and well-documented form a large body of experimental data on the development and behavior of the young child. The approach is topical rather than integrative. The subject matter is drawn from the laboratory rather than from the philosopher's arm chair. The method of presentation presupposes an extensive acquaintance with the methodology and the vocabulary of child development research; a fact that probably puts the book beyond the grasp of any except the advanced student.

The main body of the book is divided into eleven chapters as follows: periods of growth, learning, infant responses, locomotion, manipulation, speech, attention and perception, thought, emotion, social behavior and personality, individual differences. There is a bibliography of 206 titles and an index.

University of Minnesota FLORENCE L. GOODENOUGH

The Wholesale Personality. By W. H. BURNHAM. New York and London, D. Appleton & Co., 1932. Pp. xv, 713.

The volume is a contribution in the field of mental hygiene, much on the order of *The Normal Mind* by the same author. The work surveys the whole field of personality mainly by bringing together the results of some of the most scientific studies available on the topic. The author emphasizes the biological basis of personality and argues against dualism. Personality, for Burnham, is "largely a social evaluation" of the organism's behavior in the light of consistency and wholeness. The importance of early training in normal development is emphasized with illustrations, and some space is devoted to the known and probable influences of glandular secretions on personality.

For the lay reader, the author has implied too much, whereas the trained mental hygienist will no doubt view the contents as sound information commonly known. The anxious lay reader, looking for suggestions, will probably finish the volume having discarded many of his former fantastic notions of personality. The book will assist any reader to a better understanding of his fellow man as well as of himself.

Milligan College JESS H. EDDS

The Problem of Genius. By WILHELM LANGE-EICHBAUM. Translated by Eden and Cedar Paul. New York, Macmillan Co., 1932. Pp. xix, 187.

This is a translation of *Das Genie-Problem, eine Einführung* (1930-1931) which has been reviewed in these columns (this JOURNAL, 44, 1932, 483). This book is a restatement of the theoretical conclusions arrived at in Lange-Eichbaum's larger book, *Genie, Irrsinn, und Ruhm,* which contains the biographies of over 800 persons "who have been regarded as geniuses." J. P.

By Word of Mouth: Informal Essays on Language and Psychology from a Teacher's Point of View. By TATIANA W. BOLDYREFF. Boston, R. D. Badger, 1931. Pp. 144.

The author characterizes himself as a "Russian exile and a nomad," having an interest in mother tongues and "other tongues" because of experience as a student of languages and naturalization in America, service as a translator, and, finally, observations as a teacher of languages. He is disappointed at the small degree of attention that psychological investigation has devoted to language and at the present results, but attempts only vaguely to indicate specific well-thought-out problems for investigation. In view of what little has been done scientifically this vagueness is not surprising. Suggestions are made with respect to bilingualism. The author does not believe the learning of an artificial language like *esperanto* would be successful, and he favors making one of the modern languages common to all nationalities, while each language group retains its one native tongue. An artificial language interferes with freedom of expression; it is an impoverished combination of elements from several natural languages, and hence stifles thought, and imposes intellectual and social limitations. Moreover, such an artificial language "would also greatly injure, perhaps, eventually totally dispense with the study of foreign languages." As to the question of whether two languages learned simultaneously would be mutually inhibitive, the book reveals an ignorance of some relevant psychological experiments.

There are several other indications of a lack of contact by the author with psychological literature and methods. We read that in learning a natural language "there is also a certain guiding principle developing into something like inherited linguistic instinct or intuition which puts one through" (p. 21). It is interesting to note that the author, an experienced linguist and teacher of language, should fail to see more accurately what this 'guiding principle' really is. The magic of the term 'instinct' as a general explanatory means seems to appeal to him. "Existence of slang, richness in certain languages of idioms, fondness of figures of speech, and so forth, what are these traits but indications of the *bilingual instinct in embryo?* And yet man is a social being and it is natural for him to adapt himself to his fellow creatures of another nation including their peculiarities of speech. That is what the bilingual instinct *was meant for*" (p. 26. Italincs mine). These are examples of the author's naïveté as to scientific attitude; and still one must admit that there are good ideas, and that psychology can profitably investigate problems relating to the development of language in a broader and more scientific sense than heretofore. The child clinics recently developed in America are already making young subjects more available for experimentation. J. P.

I Lost My Memory: The Case as the Patient Saw It. ANON. London, Faber & Faber, 1932. Pp. 305.

This account of gradual emergence from a sudden total amnesia, occurring in the spring of 1931, covering the years from 1911 to 1931, and including an extensive war experience and subsequent scientific career, is written by the patient himself and is a unique document. A letter to the reviewer from a responsible person says that the subject and his physician, who endorses, of course also anonymously, the account, "are men of first standing in England." The method of treatment consisted largely in causing the patient to call up visual imagery, apparently especially vivid in his case; his dreams were also studied, with a refreshing absence of symbolic interpretation, and free association was used. Various parts of the lost period were gradually recalled in fragments; the final clearing up was sudden, but the events of the first three days after the occurrence of the amnesia were not recovered. The introspective study of the points where resistance appeared is highly interesting. The cause of the amnesia was revealed as certain financial difficulties and apparently the memory of certain domestic difficulties which are not set forth in the narrative, the resistance being still thus far operative! M. F. W.

Secret Ways of the Mind. By W. M. KRANEFELDT. Introduction by C. G. Jung. Translated from the German by R. M. Eaton, with a preface by the translator. New York, Henry Holt & Co., 1932. Pp. xl, 188.

Although purporting to be a "survey of the psychological principles of Freud, Adler, and Jung," this book is essentially a presentation of Jung's system. In a few pages the Adlerian individual psychology is dismissed as a practical system of therapy which has given no answer to the important question 'of the nature of the soul.' Freud's system is discussed at greater length and given more scathing criticism. "The Freudian theory rests on fantasy" and likewise "gives us no theory of the soul." In general the discussion is philosophical rather than psychological. The book is too highly technical for any except the most advanced students and probably does not contain enough new material to interest the specialist.

Harvard University M. H. ELLIOTT

Classic Descriptions of Disease. By RALPH H. MAJOR. Springfield, Ill., Charles C Thomas, 1932. Pp. 630.

In a recent number of this JOURNAL (44, 1932, 590 f.) I presented a review of Fulton's *Selected Readings in the History of Physiology.* The present volume may be considered as doing for the classic accounts of disease what Fulton did for classic physiology. Major's volume is, however, of less professional interest to the psychologist.

Among the early accounts of disease, which may be of interest to psychology, are the following: *Tetanus* as described by Aretaeus the Cappadocian (Second or Third Century A.D.); *Tetany* as described by John Clarke (1761-1815); *Fröhlich's Disease* as described in 1901 by Alfred Fröhlich; *Apoplexy* as described by Johann J. Wepfer (1620-1695); *Erythomelalgia* as described by S. Weir Mitchell (1830-1914); and especially ten selections on *Thyroid Disease,* by writers from Paracelsus to Murray.

Brown University LEONARD CARMICHAEL

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MIND, BODY, AND SOUL IN MEDICAL PSYCHOLOGY

By Madison Bentley, Cornell University

It was a red-letter day for early man when he learned in his simple logic how to pass from pairs to trinities, from the coupling of two comparable things to a grouping by threes. The simplicity and the natural conjunction of pairs are obvious. You either regard as coupled two distinct individuals (as a pair of oxen or a dancing pair), or you alternate complementaries (right or left hand, the to-and-fro of the pendulum), or, finally, you set *this* in opposition to *that* (as up against down, sweet against sour, love against hate). Much primitive logic is comprised under these various forms of pairing. This *and* that, this *or* that, this *against* that; all combinations easily spanned and readily dealt with as units.

Even so complicated a performance as social estimation is usually compassed by the simple logic of two similars or two opposites. Take any one of fifty epithets by which we label our neighbors, our lovers, and our enemies, and you invariably find that it has its apt opposite. An individual is good or bad, learned or ignorant, adorable or intolerable, frugal or thriftless, and so on with the entire list. The individual who is branded by the one excellence is removed as far as possible from its opposing vice. Primitive thinking —the kind most used to this day in making our social adjustments—likes best to deal in high light and shadow and not to bother with the intermediate tone.

But a logic of three is much more sophisticated. There you cannot simply relate *this* and *that;* you cannot pass comfortably back and forth, and you cannot stretch a similarity until you have reached maximal unlikeness, as when you remove the east from the west or oppose sin and virtue. Some new form of integration is required, something with a wider span than the pair. One of our neatest devices for setting threes in order is the triangle. A corner or a side suffices for each and for each in relation to the others. Small wonder that the triangle with its significant properties has become one of man's most useful instruments for carrying and preserving his trinities! And it is not surprising that he has reserved the subtler logic of the triad for some of his most cherished and significant groupings, for his fine arts, his trinitarian godhead, and the notion of his own engaging and baffling personality. The Christian's God is Father, Son, and Holy Ghost; creator, intercessor, and comforter; three offices in one being. And similarly man is himself body (physical mechanism), mind (thinker and doer), and soul (mysterious and eternal member).

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* Accepted for publication July 5, 1933. Address delivered at the Mayo Foundation, Rochester, Minnesota.

Now it is not by chance that the medical arts should have treated man as a composite being. Called upon to heal before man had clearly triangulated himself, medicine wrestled with demons and spirits, matching its cunning to theirs, and seeking to purify and to sustain the essences of man. The sick instrument was body, stretched prone, broken, or distorted by pain, albeit a body moved by mysterious forces. The whole man was involved. The medicine man or doctor was at once wizard, healer, and minister to the tormented and possessed patient.

Simplifying and rationalizing its task, medicine sought gradually to free itself from responsibility to the soul or spirit. The doctrine of demoniac possession it came in time to desert, maintaining that its arts were to be directed to biological functions deranged by poisons and other pathogenic agents.

With the rejection of possession and of the soul as the guest and companion of the body, medicine would seem to have been on the way toward a like elimination of the mind. Anatomy lays bare the body: pathology displays the evidence within it of disorder and disease: physiology gives an account of the coöperative functions and operations of the body, either as running a healthy and undisturbed course or as deflected and disturbed by physical agents and bacterial invasions.

Now there is no doubt that a purely *somatic* medicine would be far simpler and far more satisfactory than a theory and art of cure which looked upon the patient as a being composed of earth and of immaterial essences. And medicine has valiantly tried from time to time to restrict itself to the earthly frame and to the bodily functions. But it has developed in the same cultural atmosphere as philosophy and as the natural sciences have; and it has preserved with them the common heritage of a double nature in man, half physical and half mental. The Cartesian doctrine of matter and mind, extension and inextension, is far more than a philosophical theory. It is a well-nigh universal way of regarding man; a way, to be sure, that may be disregarded by the experimental physicist, to whom the mental aspect of existence may appear only in the passive human observer. The world to be studied and controlled is for him physical through and through and subject only to physical principles and laws. Mind is present as the knower, but not as an object of observation. At most it is a source of calculable errors or of the annoying personal equation. When the physicist came upon the Cartesian man outside his laboratory—in his emotional life, for example—the non-physical part could always be referred to religion, to magic, or to the vagaries of psychical research. Animals, too, could be dealt with by the naturalist, as organized and living bodies, actuated perhaps by innate forces or instincts, but essentially mechanistic in their mode of operation and behavior.

Outside of the physical sciences, however, man has always been an enigma. Desire, fear, lust, and disappointment have been real and actual to him even when he was well enough to go to the laboratory, to business, or to sin. And when he fell sick, his somatic and mechanistic world became shaky and inadequate. The miseries and incapacities of illness, not to speak of the realities of life and death, have inclined man toward a binarian, if not a trinitarian, view of himself.

Moreover, the doctor as well as the patient has found it difficult to be purely somatic, to ignore non-physical factors and agencies. The double-man of Descartes has persistently stood before him whether for study or for cure. Without doubt the establishment of a doctrine of disease based upon parasitical and bacterial agents has at times tended to support his somatic view and to encourage his belief in the adequacy of physical medication in all forms of disorder. And his reluctance to admit non-physical pathogenesis has been strengthened by the mechanistic corollary that only the physical could cause the physical, since matter and energy form a closed and self-actuating universe. Mind doubtless exists, but it could never take the place of a bacterial agent in causing disease or of a physical medicament in curing it. If a bad stomach grew worse under worry or a frail heart faltered under fear, that could only happen because worry and fear had their neurological correlates which were doing all the bad business which the untutored laymen laid to his uncomfortable feelings. The feelings as felt were only mental epiphenomena and could not therefore alter gastric juice or disturb cardiac contraction.

Nevertheless, it has long been obvious in a hard-headed view of things that cheerfulness and courage are better medicines than worry and fear, and that an unrelieved strain in business may well be followed by digestive upset, twitching muscles, and pasty skin. Many men, especially those who have been responsible for the disorders classified as neuroses, have therefore modified their somatic views and have added *psychogenic* causes. The organicist, however, refuses to be moved, maintaining that all diseases are really organic, those appearing to be 'functional' only hiding for a limited time their real bodily character.

You know, far better than I, how active is the contention of the parties, especially as they battle in the field of the mental diseases. The nervous stomach and the delicate heart may themselves raise the issue; but when the somatists face the extreme disorientation of the psychoses and the vagaries of the neurotic, they find it difficult to pacify the psychogenicists with the promise that complete and adequate bodily explanations will presently be at hand and that, in the meantime, pious hope is better than recourse to the psyche.

Within our own times the problem of origins in mental medicine has been further complicated by a return from the simple pairing of mind and body to trinitarian views. In certain quarters man is not just conscious organism, *i.e.,* body and mind, but body-consciousness-unconscious. Again we have the triangle with the unconscious written in capitals at one corner. In some forms of that doctrine which admits the unconscious to membership in the organism, the unweighted triangular form of the trinity is scarcely sufficient. The unconscious is really basal; it is, so to say, the father-and-creator. Unconsciousness is aboriginal. The organism only comes to existence when pushed by its libidinous urge or *id.* It is life-energy. Only gradually appear body and the ego to complete and to round out the total being.

Now this return to a triune being is not a merely formal and speculative proposal. In the doctrine called psychoanalytical, the adherents profess an understanding of disease in terms of an actual three-fold organism whose fundamental and elemental member is the unconscious. To the proposal that the mentally disordered be understood, explained, treated, and cured in the name of this trinitarian doctrine, medicine was at first indifferent, then scornful, then combative, then argumentative, then infected, then divided within her own house, then . . . but who knows the future?

Now I am no more interested than you are in theories and doctrines, in speculations and assumptions, in the tenets of schools or the debates of believer and skeptic. To embrace, to belong, to champion, and to decry are not human attitudes and functions that attract me: I am not here to urge or to combat one doctrinal position or another.

At the same time, I seem to see in all this current debate among the somatists and the mentalists, the organicists and the functionalists, the psychiatrists and the neurologists, and analysts and the anti-analysts, new turns to old contentions about the one and the many in the constitution of man, and new turns wherein the psychologist should accept some responsibility and should offer—provided it is within his power—some service to the medical arts.

Were these conceptions of the dependence of disease upon the constitution of man merely historical views, remote in time and place, they might safely be left to the historian of medicine. But they are not. The single man, the double man, and the triple man are all present-day conceptions. Moreover, they determine present-day practices. It is not likely that they are all equally applicable to the same body of biological fact. If we should examine them dispassionately, without regard to their philosophical implications, we should discover—as I am quite sure—that each has its practical virtues and each its limitations and defects.

May we dwell upon them briefly and without contention?

(1) The first view regards man as diseased in his body, the disease referable to physical causes and agents, and curable (if at all) by material means. This view may admit conscious symptoms and it may also hold it to be possible to get at and to improve the bodily state by appealing to such conscious things as desire, mood, determination, and the like.

It is obvious that a large part of medical practice proceeds sensibly and with whatever success physical medicine actually attains, under this view. But it also appears that certain morbid states and conditions (notably some of the insanities, as well as the phobias, the impulsions, and the anxieties) are imperfectly understood and indifferently treated under the view described.

(2) The second view regards man as a paired double; as a creature composed of two complementary parts called body and mind; as a psychophysical or psychosomatic organism. As members or parts of the organism, the physical and the mental are coördinate, inextricably conjoined while life lasts. In one form this view has regarded the physical and the mental as different aspects of the same thing or substance (double-aspect theory); in another form the anatomical structures have been primary and the mental structures dependent upon them (dependence-theory); in still another form mind and body have been equally efficacious—albeit in different ways—in the conduct and the government of the organism (double-adaptation theory).

In one form or another this 'paired-double' view was generally sustained by most successful systems of psychology up to the opening of our century. It is obvious that it too may be, and has been, eminently useful in medicine. It has been especially popular whenever medicine has been inclined toward the biological view of adaptation and mal-adaptation, for here mind may be viewed as a well or a diseased instrument of adjustment.

In practice, medicine distinguishes physical diseases from mental diseases, though it has never given the term 'disease' precisely the same meaning in the two contexts, and it usually hastens to explain that 'mental disease' also implies the sick body.

This double-aspect view usually persuades the practitioner that when he is concerned with mental diseases he is also a psychologist. As a result he frequently writes a 'psychology of the abnormal' or a 'psychology of mental disorders' which is a miscellany of medical facts and theories with terms and fragments thrown in from general psychologies, which are usually antiquated; these terms and fragments carefully selected to fit the favored medical theory. The result is not a psychology in the psychologist's sense, but a medical medley and theory of symptoms interspersed with psychological terms.

(3) The third view opens the door—as we have seen—to the unconscious. We think first of Freud's doctrine and practice. The fundamental contention here is that libido or some other primal urge is a permanent and enduring part of the living being, and that it resides in, or makes up, the unconscious. Certain temporary and conscious desires may be repressed and thus be transformed and transferred to the unconscious. Here they are active and dynamic, seeking to escape and to determine action. Conflict ensues. The organism and its functions are disturbed. If uncontrolled, disease may follow. Where disease thus originates, its relief obviously depends upon making peace with the unconscious. The prescribed method is psychoanalysis. Disorders of this sort are said to derive from a psychogenic origin; but not psychogenic in the meaning of the second, or mind-body, view. Psychogenic as involving the unconscious or (to employ a later Freudian term) the *id*. Their exorcism is said, therefore, to depend upon 'depth-psychology,' a term now treasured by many adherents of this doctrine. In Freud's hands, this view emphasized sex as the source of the unconscious thrust or libido. There it also led to a host of accessory concepts; repression, transference, complexes, symbolism, dream-work, sublimation, and many others. Less violent adherents to this trinitarian view have made it evident that some of these concepts are unnecessary; that other motives may replace the sexual, transference may be reduced to an empirical personal relation, and the naked soul itself drawn from its cavernous depths to the twilight of the subconscious.

Were we asked to name the chief advantages to medicine from such a doctrine, we should suggest that, in our view, the gain lies less in adding a third and hypothetical party to the human constitution than in quite another direction. To be sure, this third constituent is conceived as powerful enough to supply the main motive power to the organism, and disturbing enough to generate a good half of the ills that flesh is supposed—by the somatists—to be heir to. But it is not, as I think, this powerful and hypothetical agent that most strongly sanctions and commends the psychoanalytical movement. It is, instead, the contention that disordered function has at times its source in the history of the organism, a history that is quite as truly private and biographical as it is racial and biological.

Wholly apart from this question of the psychogenic origin of disease, it has of late been made abundantly evident that many disturbances of the organism derive, at least in part, from earlier facts and incidents in the individual life. Shame, worry, remorse and fear, as well as unhealthful habits and practices of childhood, obviously have their untoward sequels in the later life. No other organism is so highly individualized in its life-course as is man. This unique position is chiefly due to human socialization, which makes the individual a member of a family, an active partner in many lesser and greater social congregates, and a center for local, folk, and social traditions. Man lives as much by social valuation and estimate as by the physico-chemical interchanges common to his kind.

Now this fact that biographical history counts as much—in adequate and in inadequate functions, in the healthy and defective living of men—as does the common biological sequence of growth, development, and decline has been stressed by the trinitarian, who is always insisting that cure must wait upon an analytical process which recovers, identifies, and estimates the biographical past. Whether or not we follow him in his assertions about the unconscious, the conflict and the symbol, is quite another matter. His speculative structure rests upon a bold and bizarre notion of the soul and of its libidinous nature, its innate greed for power, or its draughts upon the racial unconscious.

This is the trinitarian's mode of interpretation. But we must remember that biography may be conceived in wholly different and non-analytical terms. It may, for example, be written down in terms of genetic psychology, which first obtains samples of the life-course, taken directly from infancy, childhood, youth, and the several decades of adulthood. These samples may be drawn from many individuals, compared and described in longitudinal course as natural sequences in the individual life, as dependent upon stock, diet, education, family vicissitude, and many other factors. To reach the idiosyncrasies of the individual, this genetic method must, to be sure, call upon social psychology for principles of socialization of the individual, and it must look both forward and backward in the individual course to catch the long run of developmental tendencies in their actual sequence. In its backward glance, it may use *any* legitimate method of history; it is by no means constrained to adopt the speculations of the ego, the libido, the id, and the unconscious.

We must admit that, even in his most determined and reasonable thinking, man loves short-cuts. He will often tolerate the vagaries of a speculative scheme to escape the laborious collection and sober interpretation of facts. But to the patient and the industrious there is always available the empirical method of a sound genetic psychology and biology to take the place of an assumed unconscious soul which has been made to order to fit a speculative doctrine of origin and cause.

But let us return to our three men; our single man, our double man, and our triune man. Return to ask whether it is not possible by setting philosophical and medical tradition aside and by scrutinizing the human animal himself, to simplify our common problems.

The proposals which I shall have to make I have previously made for psychology;[1] but for psychology on the assumption that what is applicable within its general territory should be applicable also in all those special excursions which psychology has had to make in various directions; into the social field (social psychology), toward the study of children (child psychology), for the observation of other forms (animal psychology), the history of human varieties (anthropological and ethnological psychologies), and the study of human disorders (psychology of the abnormal).

It is a disturbing fact that there has never been a sound general and basal psychology which was also satisfactory in all these special fields as well. The result has been that the sociologists have made up their own sad social psychologies by borrowing from us a few resounding terms (such as predisposition, habit, group-mind, and social consciousness) to set off favorite sociological doctrines of their own; the biologists have generally picked out such doubtful concepts as instinct, reason, association, learning, and choice, to base their animal psychologies upon; while psychological medicine, psychiatry, and mental hygiene have either raked upon antiquated concepts from psychologies yellow with age (the commonest being that of instinctive powers) or else have given an air of modernity to their psychological writings by importing some such alleged novelty as psychoanalysis, behaviorism, or Russian conditioning, doctrines which have never yet been really acclimated in general psychology itself.

There is a certain excuse on both sides. These special jobs are comparatively new for psychology, and she has not yet learned how to do them well; and, on the other side, those seeking psychological material have not known just where and how to look. All the same, those persons turning away from psychology with only dry bones or with empty hands can well complain that "when they got there the cupboard was bare."

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[1] *Psychologies of 1930*, 1930, 95-115; *The New Field of Psychology*, Pt. i, *The Psychological Functions*, 1933, 1-12.

To be sure, our self-love is forced to add the consoling reflection that the borrowers have been much keener to take away from psychology some neat and striking bit of speculative doctrine easily applied than attested facts and principles. When they go abroad for help, the sociologist wants the 'original nature of man' wrapped in a two-ounce package, the educator asks for an easy formula for learning, the biologist seeks an adaptable mind that will explain survival under struggle, the advertiser wants a recipe for persuasion, and the medical man either adopts the ancient faculties of reason and of will, which are variously supposed to fall into disorder and disease, or else he adopts the more modern subconscious, the will to dominate, the libido, the Œdipus complex, or the phobia of castration.

But let us not waste time in the distribution of awards and of reproaches. Our main question lies directly before us. What is the best, *i.e.* the hardest-headed and the most useful, way of regarding man when we have to deal with him sick or well from the psychological side?

The *double man* has never been satisfactory either to you or to us. The hoary mind-body problem that he brings with him is a night-walking spectre that is never laid. Mind is obviously an efficacious thing; but how could it be? Mind seems to be diseased as well as body; but how—lacking tissues—can it be diseased? Mind should—since it is one-half of man—be described in laboratory and clinic. But how shall it be?

The *triple* man or human trinity is, like the divine trinity, a matter for acceptance and conviction. If you believe in the unconscious, libido, and the id, you will easily believe in a score of other alleged and accessory dynamic agents. If that is the sort of thing that you like, then it is likely that only a new conversion or a strict training in logic will make you dislike it and lead you to reject it as a doctrine unusable in medicine.

Where belief in this doctrine (I am not now speaking of the clinical *method*) has been incorporated into medicine, medicine has been threatened with a revolution, and I have observed that medicine is not fond of revolutions. At present medicine has no machinery for controlling such an innovation and it is not fond of practices and doctrines which it cannot control, which it must share with the layman, with a religion, or with the unauthorized practitioner. I do not intimate, of course, that many doctors of medicine of sound training and in good repute do not usefully employ psychoanalytical concepts and practices, sometimes, as it appears, to their great advantage and to the well-being of their patients. That is another matter. I speak only of the incorporation of the three-fold man into organized medicine; of man with a hidden soul as well as a revealed body and an open mind.

Let us look, then, at our proposed revision of the *single man.* The man who is a body. Ah, then—you may say—we come back to behaviorism, a modern and respectable view, a standpoint sometimes taken in psychiatry and supported in psychiatrical practice!

Now there are several brands of behaviorism, and I do not mean to afflict you by discussing and distinguishing them; though I must remind you that it is one thing to enter upon the serious and scientific study of bodily performance for its own sake and quite another to maintain that such a behavioral study is adequate to the knowledge of man, sick or well, of his conduct, and of his springs of action. Psychology engaged in behavioral studies long before the doctrine of behaviorism, advanced as a substitute for psychology, was successfully advertised.

It has been obvious to psychologists as long as experimental methods have supported them that 'objective' or behavioral methods have had a wide and distinctive use; but all except a few violent enthusiasts have always seen that many problems of human living, normal and abnormal, have never been successfully formulated, not to say solved, by this type of method, and that the addition of a creed of behaviorism did not really change the nature of man or alter the face of natural facts.

But behaviorism does not offer the only alluring view of the single man. There is at least one other view. To apprehend it, we should again try to forget all theories, doctrines, and hypotheses regarding the nature of man and to consider him as he concretely is. To this end we should follow him in his daily living. Let us, *e.g.* examine the specimens of him now in this room. We are sitting or standing by virtue of certain postural functions. Respiration, circulation, digestion, and the like are going on. Given my time and your knowledge, we could make out a tolerably comprehensive list of our common physiological functions now in course. But would that list exhaust our present activities? No. The most obvious (and possibly the most difficult) thing that you are doing is understanding. I speak and you listen. Nothing is more obvious. But search the biological sciences (anatomy, physiology, embryology, ecology, behaviorism, and all the others) as long as you will and you will find no adequate description of the hard fact that I address you and that you see me, hear me, understand me, and critically value my words.

Say all you will about receptors, nerve impulses, action currents, cerebral processes, and muscular contractions, and you will not describe (I do not say 'explain') these obvious facts. Declare in addition, if you will, that you are all responding to visual and auditory stimuli supplied by the room and by me. Still you have no more touched the central fact than the astronomer who should seek to describe a galaxy by saying that suns and planets stimulate and respond to one another. In more general terms we may say that there is a whole group of psychological functions—understanding, perceiving, remembering, thinking, emotion, and the like—that must be *added to the physiological functions* of which I spoke a moment ago. As functions of the living organism, neither group can be resolved into the other, and for an adequate description of human activity the psychological functions are just as necessary as the physiological functions are. But the psychological functions by no means imply 'mind.' There literally *is*—in our present view—*no mind,* in the sense of a mental kind of existence. Anatomy and morphology are the only fundamental sciences of existence so far as the living organism is concerned. When we say 'is' we should mean the integrated body. Naïve man added the fiction of a soul-mind because he thought that he should have some sort of enduring self that the brutes did not possess and that, moreover, would help him to understand many puzzling things about life. Sophisticated man followed and strengthened the naïve tradition and made it more circumstantial. Later, reflective man came to distinguish two kinds within the fiction of the soul-mind, the conscious mind annexed to the body and the soul behind it, which appears in the present preferred forms of libido, *élan vital,* entelechy, and the like.

When he is dressed in his working clothes and observes with the training of the laboratory behind him, man does not *observe* mind and he does not *observe* soul. What we all observe, structurally speaking, is the body, and what we all observe, functionally speaking, are the physiological and the psychological functions. When objects are announced to the living organism they are announced by way of perception, memory, or imagination—all announcing or apprehending functions; for thus the organism lays-hold-of, seizes, or apprehends objects and events. When predicaments arise they arise by way of the emotive functions. When thought-problems proceed toward solution they always proceed through the elaborative or thinking functions. All these functions are modes of activity not comprised within the list of operations studied by the physiologist.

But when we ask *how* these things are done—how we perceive, understand, think, and the rest—we can only answer that the *body does them.* We never observe the mind doing them. Mind is a fiction created to make an easy explanation, and when that fails we create some form of soul. Both entities are of great value for literature, religion, music, and other aspects of the *private and the romantic life* but are without a place in the sciences.

Sherrington somewhere remarks that we have no adequate notion of the variety of things which the body can do until we dismiss our prepossessions and observe it actually at work. As a matter of hard fact, we lose nothing for scientific description when we lose mind. The conquests of human knowledge, the amazing deposits of social tradition, the formulation of laws, and the creation of fine art and letters lose none of their dignity and none of their value to man when we discover that they are products of bodily structures, wrought out through the agency of functions which we may conveniently call 'psychological.' We can no more conceive social organization or poetry or human tradition as coming into existence without the mediation of the psychological functions than we can conceive the living cell without metabolism or a building without the operations of mason and carpenter. But knowledge and art, custom and law, appear to derive just as directly from the properties and the capacities of the integrated organism as do the physical products of gland and muscle, of blood and viscus. No more do we find it necessary to assume the mysterious existence of mind and soul than of vital fluid and animal spirits.

But we must also regard these psychological products in their own character if we would understand the psychological operations behind them. We must regard them not only because product always throws light upon function—as secretions and excretions of the body upon its physiological operations—but still more because the organism is constantly exposed to its psychological products and (in man, at least) its life-course is largely determined by them.

Some men have tried to persuade us that the physical order is only our percept, a product of our apprehension. That may be left to the philosophers. Our interest lies in the fact that all psychological products throw light upon their organic origins. They also form, so to say, the field within which the organism continues to operate. Especially do the social products of the organism. *i.e.* social organization, tradition, opinion, tabu, institutions, and morals, react upon the organism to temper those functions which made them and so direct and determine the subsequent life-course. Man proceeds through life, therefore, as through a sea of socialized objects, events, and occasions which his own functions have created. Since he always proceeds from infancy to death as a particular organism, reacting in a particular and individualized way to this fluid medium, his life becomes a personal biography and not merely a biological history of a pattern common to all men.

Now one more corollary of the functional view and we shall have everything necessary to a psychology of disorder. That corollary proceeds from the fact that function itself changes the organism. It changes it by disposing the organism toward future performance. When renewed, functions tend to repeat themselves in the old way. This is the functional fact of trend or predisposition. Every organism, *i.e.* living body, is the scene of a constant stream of functions all molding the organism toward future modes of operation. This means that no psychological function is wholly new or naïve. It has other functions behind it. This is the past active in the present. The body thus carries functional trends by the thousand, some general (as in right-handed performance or in a tendency toward sentimental effusiveness) and some specific (as the memorial recovery of a former event). How the body continues to hold these residues of earlier function we do not know. That it does we have abundant evidence both in physiology and in psychology. The past, without living again, is nevertheless able to color, determine, or govern the present.

Life therefore is cumulative in this functional sense. Its process is irreversible. And since its temper and direction are determined, first, by the specific character of an individual past, and second, by the specific reaction of specific products upon the organism, every life-course is bound to be individual and biographical.

In fine, then, we have a living body apt in many occupations and performing in many diverse ways and by many modes of activity, which we segregate into two great classes, physiological and psychological. The first mainly tend toward integrating the body in an active way and in interchanges between outside and inside materials and energies. The second mainly tend to make the organism "an active agent in *apprehending* itself and other objects, in *initiating* and prophetically *carrying out* its own activities, in *setting and resolving* problems, in *divining* the significance of things, in *communicating* and *participating* with others of its kind in the affairs of human groups, and in *combining* its beliefs, customs and knowledge into large organized systems. Where the physiological functions constantly fashion and reconstruct the living body, the psychological functions fashion and reconstruct knowledge, conduct, society, and the universal theatre of objects, events, desires, and values. For the one kind of function the living body is a *vehicle,* for the other it is an *agent.*"[2]

Every sort of function has its issues—the physiological sort turning out secretions, delivering oxygen, expelling waste, building new tissues and the like; the psychological sort fashioning objects, creating knowledge, crystallizing traditions, forming laws, and setting up values.

Some physiological products immediately serve the functions themselves. Witness hormones and such substances as adrenin and thyroxin. Others store fuel and energy against future use. Some psychological functions make tools for man. Others make a social medium in which men live.

As life goes on its course is constantly made and revised as the functions remodel and extend themselves through repetition and interaction and as the functional products return upon the organism to guide, govern, or afflict it. Knowledge is a psychological product which supports and directs the activities of the organism. Social prohibitions and tabus are products which obstruct desire and appetite, causing the type of emotive function which we call 'conflict.' One motive to conflict may lie behind, deriving from the persisting and cumulative bodily trends of the individual past; the other motive may lie without, deriving from those products which are deposited in social tradition and formulated command. Neither element of conflict lies in the mysterious depths of a hypothetical unconscious or in a hypothetical and persistent libido. The body on one side supports its functional trends and its persistent aptitudes; psychological products of previous functioning, on the other, reënforce and redirect the course of life. For the psychologist, at least, there would seem to be no necessity either for mind or for soul.

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[2] M. Bentley, *The New Field of Psychology,* Pt. i, 1933, 7.

Were the medical and psychiatrical arts to come seeking a fundamental view of human living and of human derangement, they would find in this empirical view of things a single, and more or less unitary, organism which is structurally a living body and functionally a seat of many operations. Both sorts of function it would find frequently deranged, disordered, inadequate and maladjusted. Its first task would be to describe and to understand the functional defect, its second to search out all those factors present and past which bear upon the derangement, its third to discover the bodily resources of function, both for its normal and its defective course, and, finally, to seek means and medicaments of relief.

The distinction between 'functional' and 'organic' as classes of disease would therefore vanish under this approach, for we should find always functional disturbance and always the bodily and extra-bodily ground for the disordered, as well as for the healthy, function.

A revision of the concept of 'mental disease' might well be considered, first because 'mental' (as implying 'mind') would be superfluous, and again because disordered function would by no means imply either a lesion or a psychogenic agent afflicting the body. Just as disorder in the running of the motor car often implies poor gasoline, carbon-deposit or defective lubrication, but not an *engine-disease,* so the deficiencies and the defects of schizophrenia and of the neuroses do not necessarily point to lesions, invading organism or pathological states of mind, but may instead lead the diagnostician to a blocking or stasis as between functional patterns or to an unwholesome descent upon the organism of afflicting and devastating products of antecedent function—our own antecedent function and the function of many another organism as well. The organism will have to be regarded less as an invariant biological sample of *homo* and more as an individual biography, constantly changing in time, and interacting with other biographies in individual ways and also with the great social deposits of our race and time.

Those bodily changes which directly underlie and outlive function will, at times, still be difficult to find—their full discovery may await new physiological and biochemical methods which we now only glimpse; but a persistent search for actualities of operation and of structure will be far better than a flight to fancied hereditary causes, to the gross paradigms of symptomatology, or to vagaries of 'mental symptom' and of 'unconscious cause.'

To the psychologist, of course, the concept of 'disease' is foreign, even to the psychologist of the abnormal. It follows therefrom that 'symptoms' and 'diagnosis' are not for him. Both imply disease. The offices of the diagnostician and of the physician are not his. The psychologist deals descriptively with his functions, with their bodily antecedents and mechanisms, with their history, and with their products. Defective and inadequate functions are as much his concern as normal and adequate functions are. This would seem to imply a fair division of labor as between psychologist and physician, and, as we may hope, a useful and amicable form of reciprocity.

THE PSYCHOLOGICAL SIGNIFICANCE OF THE HOROPTER

By ALBERT DOUGLAS GLANVILLE, Cornell University

The determination of the horopter, that is, of the locus of the points in space where, with a given fixation of the eyes, an object will be imaged upon corresponding retinal points, has long constituted an important problem in binocular vision. Since no one has ever made a universally acceptable determination of the horopter the problem still interests students of psychology and of physiological optics.

Nearly all of the early attempts to determine the horopter grew out of theoretical, mathematical, or anatomical conceptions of corresponding retinal points. Many of the later experimental determinations were based in part upon more or less casual observations of a single observer. In some instances these determinations were invalidated by structural peculiarities of the observer's eyes, and more frequently the conclusions drawn were distorted by a theoretical bias of the investigator. In the course of the last three or four decades a few sound experimental studies have been made; but, because of the uncertainties of peripheral vision, the observers have generally been instructed to report in accordance with some assumed criterion of retinal correspondence rather than to report whether the object was seen as single or as double. The outcome of these investigations has been, when the existence of the horopter was not denied, a variety of proposed horopters, none of which has proved to be generally acceptable.

The present study was undertaken for the purpose of determining the points in space at which, with a given fixation of the eyes in the primary position, a simple object will be *seen* as single. For the sake of making a fresh attack upon the problem, it has seemed desirable to leave aside the various intricate notions concerning retinal correspondence as well as the previously determined horopters and to attempt to determine an *empirical* horopter upon the basis of results obtained from observers who were instructed to report upon the precise character of peripherally perceived objects.

HISTORICAL SUMMARY

The literature on the horopter and on the closely related problems of retinal correspondence and of binocular single and double vision is so

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* Accepted for publication May 20, 1932. The study was carried out in the Psychological Laboratories of Cornell University under the direction of Professor Bentley.

voluminous that it would be inexpedient to give here anything approaching a complete summary of it. Nothing more will be attempted at this time, therefore, than to review briefly some of the more significant studies of the horopter and of related problems.[1]

Aguilonius, a Jesuit writer on optics, seems to have first used the term *horopter.* In his *Opticorum libri sex*[2] (Antwerp, 1613), he proposed the theory that the horopter is a plane passing through the fixation-point perpendicular to the median line of sight. He argued that the images from the two eyes were projected upon this plane and that only objects seen in this plane appeared single.

A markedly different form of the horopter was proposed about two centuries later by Vieth and J. Müller. Vieth,[3] in 1818, said that the visual plane cuts the horopter in a circle passing through the fixation-point and the centers of the lenses of the two eyes. Objects located upon this horopter should be seen as single, he contended, because their images would fall upon corresponding retinal points, whereas objects at other positions would produce images upon non-corresponding points and should therefore appear double. J. Müller,[4] writing a few years later, regarded corresponding retinal points as being anatomically fixed points which were located equidistant and in the same direction from the centers of the two retinas. Although he thought that the complete horopter should be a surface, Müller concerned himself with only the part of it passing through the visual plane. In accordance with his notion of the location of corresponding points, he showed that this horopter is a circle which is identical with that of Vieth. This circle is now usually called either the Müllerian horopteric circle or the Vieth-Müllerian circle.

In 1838 Wheatstone[5] attacked the validity of the theory of corresponding points and showed the inadequacies of the horopteric plane (Aguilonius) and of the Müllerian horopteric circle. Upon the basis of observations made with the aid of the stereoscope, which he invented, he demonstrated that objects not in the horopteric plane are very often seen, not double, as Aguilonius had argued that they must be, but single. Likewise, he demonstrated that objects whose images fall upon disparate retinal points may be seen single, thereby proving that objects not falling in the horopteric circle may be seen single. Furthermore, he found that under some conditions objects whose retinal images fall upon corresponding points may be seen double. From this latter observation he argued that it was an error to assume a necessary physiological connection between corresponding points. He also suggested that the theory of corresponding points was an unsatisfactory explanation of binocular single vision. These observations and arguments have been severely attacked by several writers who followed Wheatstone.

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[1] Brief summaries of the earlier writings may be found in H. Helmholtz, *Hdbh. der physiol. Optik,* III, (3 ed.), 1910, 394-396, and in E. Claparède, Quelques mots sur la vision binoculaire et stéréoscopique et sur la question de l'horoptre, *Arch. sci. phys. et nat.,* 3, 1853, 138-168. For a summary and discussion of the more recent experimental studies see A. Tschermak, Optischer Raumsinn, in *Hdbh. der norm. u. pathol. Physiol.,* 12, 1931, 891-916.

[2] The writer was unable to consult a copy of this rare work, but he has found it referred to by H. Helmholtz's *Handbuch* (3, 1910, 394) and by other reliable writers.

[3] G. U. A. Vieth, Ueber die Richtung der Augen, (Gilberts) *Ann. d. Physik u. Chem.,* 58, 1818, 233-251.

[4] J. Müller, *Zur vergleichenden Physiologie des Gesichtssinnes,* 1826, 71-83. See also his *Handbuch der Physiologie,* 2, 376-387.

[5] C. Wheatstone, Contributions to the physiology of vision, *Phil. Trans. Roy. Soc. Lond.,* 1838, pt. 2, 371-394.

On the basis of the theory of corresponding points A. Prévost[6] demonstrated the theory that the horopter consists not only of the Müllerian circle but also of a vertical line passing through the fixation-point.

Meissner[7] was the first to attempt an experimental determination of the horopter. He constructed a simple apparatus by means of which threads could be held in various positions so that an observer could report where the threads appeared single and where double. He recognized the necessity of making a large number of observations and of controlling movements of the head and eyes. From his results he drew the following conclusions: (1) When the eyes are in the primary position and the fixation-point is at an infinite distance, the horopter is a plane perpendicular to the median line of sight. (2) When the fixation-point is at some nearer position in the primary visual plane, the horopter is a line inclined toward the visual plane and dipping toward the observer, the inclination increasing as the fixation-point is brought nearer to the observer. (3) When the visual plane is turned upward, the inclination of the horopteric line increases, whereas when the visual plane is turned downward, the inclination of the horopteric line decreases until it becomes zero at 45° and then expands into a plane passing through the fixation-point perpendicular to the median line of sight. Meissner related these results to a rotation of the eyes outward in such a way that the vertical lines of demarcation coincide with the vertical meridians only when the eyes are fixated upon a point at infinite distance.

In 1858 Claparède[8] published a series of articles on binocular vision and the horopter in which he critically surveyed the earlier literature and then went on to propose a horopter based partly upon observations which he had made and partly upon theory and geometry. According to him the horopter is a complex surface which contains a vertical line passing through the fixation-point (the horopteric vertical), the Müllerian circle, and all circles passing through the nodal points of the two eyes and tangent to the horopteric vertical.

In a monograph on binocular vision Panum[9] proposed a modification of the theory of retinal correspondence which later played an important part in some of the explanations advanced to account for the fact that objects not on the Müllerian circle are not necessarily seen double. Observations which he made showed, as Wheatstone similarly had found, that an object whose retinal images fall upon somewhat disparate points might still be seen single. Panum supposed that a point *a* on one retina might be identical not only with point *a′* on the other retina but also with a number of adjacent points in a circular area around *d'*. He called these circles "corresponding circles of sensation." According to this theory, then, the image on point *a* might be fused with the image on any point in the corresponding circle of sensation in the other eye. This marked a decided change away from the notion of anatomical correspondence in the direction of physiological or functional correspondence.

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[6] A. Prévost, *Essai sur la théorie de la vision binoculaire*, 1843.

[7] G. Meissner, *Beiträge zur Physiologie des Sehorgans*, 1854, 1-121.

[8] E. Claparède, Quelques mots sur la vision binoculaire et stéréoscopique et sur l'horoptre, *Arch. sci. phys. et nat.*, 3, 1858, 138-168; Nouvelles recherches sur l'horoptre, *Arch. sci. phys. et nat.*, 3, 1858, 225-267; Encore un mot sur l'horoptre, *Arch. sci. phys. et nat.*, 3, 1858, 362-368.

[9] P. L. Panum, *Physiologische Untersuchungen über das Sehen mit zwei Augen*, 1858, 52 ff.

We come now to the consideration of the elaborate theories of the horopter worked out at about the same time by Hering and Helmholtz. To understand these theories in detail requires an extensive knowledge not only of the explanations of the perception of space advanced by each of these two men but also of physiological optics and of mathematics. We do not undertake to give more than a very general summary of these theories.

Before taking up Hering's[10] determination of the horopter we must first mention certain significant aspects of his notion of corresponding points. He assumes that when individual points of the retina are stimulated we have aroused, in addition to sensations of color, a feeling of direction and a feeling of depth. The feeling of depth is supposed to have equal and opposite values for each pair of corresponding points, but for any pair of retinal points which are symmetrically situated these values are equal and of the same sign. The feeling of depth of the temporal halves of the two retinas is positive and corresponds to an increase in depth; that of the two nasal halves is negative and corresponds to a decrease in depth. According to Hering, when two impressions on corresponding points are fused the total impression has the mean value of both the feeling of direction and the feeling of depth. Hering determines the form of the horopter in accordance with this theory, but we need not here follow his intricate reasoning; but we give his formulations of the horopter for near and for far fixation.

When the lines of sight are symmetrically parallel, that is, when a point at an infinite distance is fixated, the horopter includes practically the entire field of vision beyond a short distance from the eyes. Hering calls this a horizontal horopter. When the eyes are converged upon some nearer point there is a longitudinal horopter which is a cylindrical surface composed of a straight line perpendicular to the visual plane and which falls upon corresponding longitudinal sections of the two eyes. This horopter contains the Müllerian circle and a straight line passing through the fixation-point and perpendicular to it. Hering states that objects on the longitudinal horopter should appear as if they were in a plane perpendicular to the line of fixation. He calls this plane the *Kernfläche*.

Helmholtz's[11] determinations of the horopter are based upon the assumption that the eyes move in accordance with Listing's law of ocular movements, that the horizontal retinal meridians are coincident with the visual plane when the eyes are in the primary position, and that the apparently vertical retinal meridians are inclined so that, if one retina were placed over the other with their horizontal meridians coincident, the apparently vertical meridians would cross each other like an X, making an angle of about 2½°. Later writers, since they have usually found no evidence of this inclination in normal eyes, have presumed that it must be a defect of Helmholtz's eyes. It plays no small part in his views on the horopter, however, since he thought that corresponding points were determined in accordance with this inclination.

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[10] E. Hering, *Beiträge zur Physiologie*, 1, 1861, 20-64; 2, 1862, 81-170; 3, 1863, 171-224; 4, 1864, 225-247; 5, 1864, 287-358. Also his chapter on "Der Raumsinn und die Bewegungen des Auges" in Hermann's *Hdbh. der Physiol.*, 1879, esp. 375-379.

[11] Helmholtz, Über die normalen Bewegungen des menschlichen Auges, *Arch. f. Ophth.*, 9, 1863, 188-190; Über den Horopter, *ibid.*, 10, 1864, 1-60; *Hdbh. der physiol. Optik.*, III (3 ed.), 330-393.

Helmholtz distinguishes between a point-horopter and a line-horopter. The point-horopter is the locus of points which will be seen single, and the line-horopter is the locus of straight lines which will be seen single in binocular vision. When the lines of regard are horizontal and converged upon a near fixation-point, the point-horopter is composed of the Müllerian circle and the horopteric vertical through the fixation-point. When the fixation-point is in the median plane and at infinite distance, the point-horopter is a surface. In this instance the horopter is a plane parallel to and below the visual plane: it is the ground on which the observer stands. Helmholtz devises two kinds of line-horopters, a vertical and a horizontal horopter. The vertical horopter is the locus of lines seen single which are apparently perpendicular to the retinal horizons, whereas the horizontal horopter is the locus of lines seen single which appear to be parallel to the retinal horizons. Since the forms of these vertical and horizontal horopters for various positions and distances of the fixation-point are rather complex geometrical constructions, we shall not include them here.

Helmholtz states that the problem of the horopter is a purely mathematical exercise involving a knowledge of the movements of the eye. He did make a number of simple experimental observations, however, to bear out his mathematical and geometrical demonstrations.[12]

Toward the end of the last century Hillebrand[13] undertook experimental work to test an hypothesis which Hering had previously made; namely, that objects located on the longitudinal horopter would appear as if they were in a plane perpendicular to the median line of fixation (*i.e.* in the *Kernfläche*). Earlier observations had led Hering to presume that the point on the nasal half of one retina which corresponds *functionally* to a point on the temporal half of the other retina, and vice versa, would not be at exactly the same distance from the center of the first retina as the other point would be from the center of the second retina. He suggested, therefore, that the 'empirical' or 'physiological' horopter would not coincide exactly with the mathematically determined horopter, for which it is assumed that corresponding points are at equal distances from the centers of the retinas. It was on the 'empirical longitudinal horopter' that he assumed that objects would appear to be in the *Kernfläche.* Hillebrand determined by means of the haploscope, an apparatus for holding three vertical threads in various positions relative to each other and in various parts of the visual field, the positions in which the three threads at small eccentricities from the fixation-point appeared to fall in the abovementioned apparent plane. He found that these positions fell in a curve which deviated somewhat from the mathematical horopter. Since then this deviation has been referred to as the Hering-Hillebrand horopter-deviation. Hillebrand also concluded from his results that fixed, definite pairs of retinal points give the impression of an object lying in the *Kernfläche* and that this relation is independent of the degree of the convergence of the eyes or of the distance of the object. This conclusion fitted in with Hering's nativistic theory of space perception and contradicted Helmholtz's notion that, as a result of past experience, different pairs of retinal points would give the impression of objects lying in the plane in question when the distance of the fixation-point was changed.

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[12] Helmholtz, *op. cit.,* 395.

[13] F. Hillebrand, Die Stabilität der Raumwerte auf der Netzhaut, *Zsch. f. Psychol.,* 5, 1893, 1-59.

Accepting as an established fact Hering's assumption that objects lying on the longitudinal horopter are localized as being in a plane perpendicular to the line of sight, Tschermak and Kiribuchi[14] undertook to determine the longitudinal horopter experimentally, by finding the places where vertical lines appeared to be in the plane. They made two sets of determinations, one for plumb lines which were permanently in the field of view and another set for falling marbles. They found that the curve for the falling marbles (the drop-test horopter) was more concave towards the observer than the curve for the plumb-lines (the plumb-line horopter). Von Kries[15] later insisted that this was an investigation of depth-localization rather than a determination of the horopter. He argued that Tschermak and Kiribuchi described their results, not according to what they directly observed, but according to what they interpreted the results to mean on the basis of the assumption which they had accepted.

In 1907 Savage[16] criticized Helmholtz's determination of the horopter upon the grounds that it was based upon faulty optical constructions. Savage declared that Helmholtz had incorrectly placed the poles of the eyes and that he had incorrectly constructed the visual axes. He maintained, furthermore, that a line of vision is not an axial ray of light, as Helmholtz had said, but that it is a radius of retinal curvature prolonged. Savage then proceeded to demonstrate that the Müllerian circle passing through the fixation-point and the nodal points of the two eyes is not the true circle of single binocular vision but that the horopter is an isogonal circle passing through the fixation-point and the centers of retinal curvature which are the centers of rotation of the eyes. He also maintained that corresponding retinal points are points which have a common connection in the brain.

In 1924 Tschermak[17] and his student, Fischer, published a series of articles reporting results of experiments they had undertaken to determine the deviations of the empirical horopter from the Vieth-Müllerian circle under different conditions.

In the first of these articles, Tschermak[18] is concerned with a consideration of the so-called 'red' and 'blue' horopters. Plumb lines were suspended at various positions in the observer's field of view by means of the Hering-Tschermak apparatus. The eccentric positions were determined where the plumb line appeared to be in a plane through the fixation-point and parallel to the median line of regard. Red plumb-lines were used in one set of determinations and blue plumb-lines in another set. In both instances the horopteric curves obtained were flatter; *i.e.* they fell outside of the Vieth-Müllerian circle. The curve for the red plumb-lines was somewhat flatter than that for the blue plumb-lines. Tschermak explains this latter result in terms of the chromatic aberration of the eye. If, for a given accommodation of the eyes, red and blue objects are to fall on corresponding points they must be at slightly different distances.

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[14] A. Tschermak and Dr. Kiribuchi, Beitrag zur Lehre vom Längs-horopter, *Arch. f. d. ges. Physiol.*, 81, 1900, 328-348.

[15] In an editor's note by J. von Kries in H. Helmholtz, *Hdbh. der physiol. Optik*, III, (3 ed.), 1910, 315.

[16] G. S. Savage, A further study of the so-called horopter making ocular rotations easy of understanding, *Ophth. Rec.*, 16, 1907, 571-584.

[17] A. Tschermak, Fortgesetzte Studien über Binokularsehen, *Arch. f. d. ges. Physiol.*, 204, 1924, 177-265. The title given in this reference is the general title for the three articles. The specific titles for the individual articles will be given below.

[18] Tschermak, Über Farbenstereoskopie, *Arch. f. d. ges. Physiol.*, 204, 1924, 177-202.

Fischer[19], using the same method, verified Tschermak in regard to the red and blue horopters. He also found that the horopters for white plumb-lines on a black background and for black plumb-lines on a white background fall outside the Vieth-Müllerian circle. His results show that there is often a decided asymmetry in the shape of the empirical horopter-curves. For example, the curve for white plumb-lines may lie partly behind the geometrical fronto-parallel plane (the plane through the fixation-point and parallel to the line connecting the centers of the two eyes), on one side of the fixation-point, and entirely in front of it on the other side of the fixation-point. Fischer interprets this as indicating the existence of asymmetrical anomalies in the structure of the retinas. He also investigated the effect of length of exposure-time upon the shape of the empirical horopteric curve. The results of this part of the investigation confirm the earlier results obtained by Tschermak and Kiribuchi when they employed plumb-lines permanently in the field of view and also falling marbles. As the exposure-time is shortened the horopteric curve becomes sharper. Generally the empirical horopteric curve falls outside the Vieth-Müllerian circle, but with an exposure time as short as 0.2 sec. the empirical curve may fall within this circle. The 'white' horopter is flatter than the 'black' horopter, both for long and for short exposure-times.

In the third study of this series Fischer[20] adopted a new method of experimentation and repeated much of the work which we have just summarized. Previously he had made his determinations of the horopter upon the basis of the criterion of the apparent fronto-parallel plane, but in this investigation he employed the criterion of common visual direction. A brief description of the method of experimentation will indicate the meaning of this criterion. A peripherally seen plumb-line was so arranged that a small screen cut off the median part of it from one eye. When the retinal images of this plumb-line fell on corresponding points, the plumb-line was seen as one continuous vertical line; but when it did not fall on corresponding points, the median part of it which was hidden from the eye was seen as being either to the right or to the left of the rest of the line. The problem was to find for different eccentricities from the fixation-point where the plumb-line must be in order to be seen as one continuous vertical line.

Using this new experimental method, which Tschermak called the *Nonius* method, Fischer corroborated the earlier results for black plumb-lines upon a white background and for red and blue plumb-lines upon a black background. He did not, however, find that the white horopter, determined according to the criterion of the apparent plane, corresponded to that determined according to the criterion of common visual direction. Since the latter criterion seemed to be the better, he concluded that the white horopter cannot be accurately determined by the older method. With exposure times of less than 0.8 sec., Fischer found that neither the white nor the black horopter was as sharply concave as the previous determinations had made them. It seemed, therefore, that the criterion of the apparent plane can be employed as a basis for empirical determinations of the horopter only when relatively long exposure-times are used.

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[19] F. P. Fischer, Über Asymmetrien des Gesichtsinnes, speziell des Raumsinnes beider Augen, *ibid.*, 204, 1924, 203-233.

[20] F. P. Fischer, Experimentelle Beiträge zum Begriff der Sehrichtungsgemeinschaft der Netzhäute auf Grund der binokularen Noniusmethode, *ibid.*, 204, 1924, 233-260.

Jaensch and a number of his students[21] have recently made many studies of the so-called horopter-deviations in connection with their investigation of eidetic and non-eidetic types. They have found a correlation between the observer's type and the amount of his horopter-deviation as measured in terms of the criterion of the apparent plane.

This brief survey does not catalogue all of the conflicting views in regard to the horopter which have been held and many of which are still held, but it does indicate that the problem of the horopter has not yet been settled. It also indicates that since most of the determinations of the horopter have been made from a mathematical or physiological point of view, we have as yet no direct answer to the question where, with a given fixation of the eyes, objects in the visual field will be *seen* single. It seems to us, therefore, that a more strictly psychological approach to the problem is desirable.

The Experiments

The experiments which we are to report may be divided into two groups: (1) those in which the *O*s were instructed to describe as fully as possible what they saw when a stimulus-object was presented in the peripheral field of vision; and (2) those in which the *O*s were instructed to report merely whether the object was seen as single or as double. The experiments belonging to the latter group were quantitative in nature, and they were preceded by two series of descriptive experiments in order to train the *O*s to discriminate carefully between the singleness and doubleness of peripherally perceived objects. They were also designed to furnish us with information about the adequacy of peripheral perception under the conditions of our experiment, so that we could use this information in interpreting the results of the quantitative series. The results of the quantitative investigation showed, however, that it was advisable to follow with another descriptive series. The experiments will now be reported in the order in which they were undertaken.

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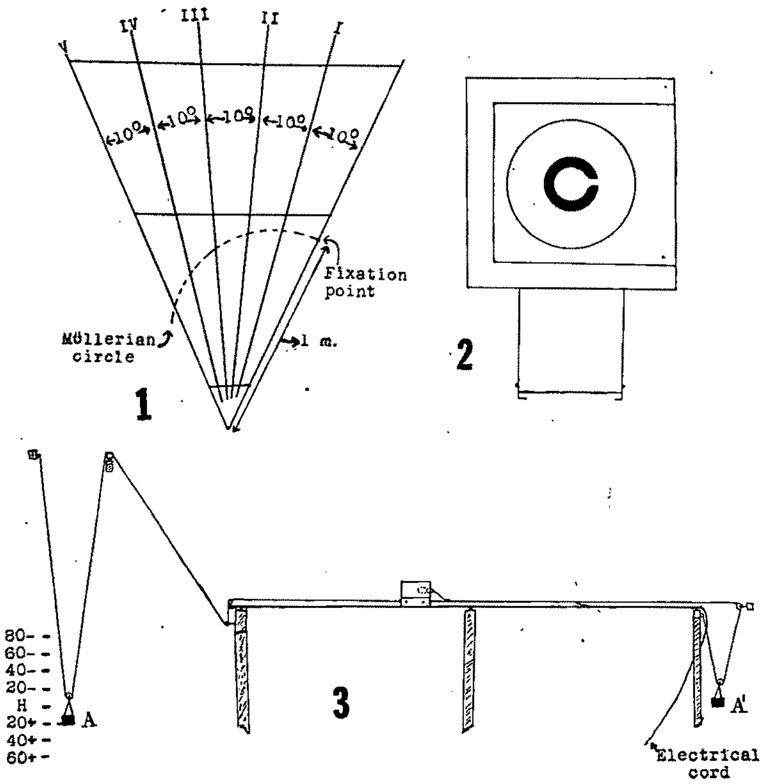
[21] See, *e.g.* E. R. Jaensch and F. Reich, Über die Lokalisation in Sehraum, in E. R. Jaensch, *Über den Aufbau der Wahrnehmungswelt,* 1923, 59-67; K. Krönke, Zur Phänomenologie der Kernfläche des Sehraums, *ibid.,* 137-148; H. Ruschmann, Über das Verhalten der Wahrnehmungen jugendlicher Eidetiker unter Störungsreiz, *Zsch. f. Psychol.,* 1930, *Erg. bd.* 16, 41-51; E. Mayer, Die Funktionsschichten der räumlichen Wahrnehmung, *ibid.,* 73-76; F. Kranz, Experimentell-strukturpsychologische Untersuchungen über die Abhängigkeit der Wahrnehmungswelt vom persönlichkeitstypus, *ibid.,* 119-123.

SERIES 1

Apparatus. The apparatus employed consisted of 6 horizontal steel tracks, each 78 in. long and 1 in. wide, along which light-boxes with a stimulus-object in the front side could be moved. One of the tracks extended straight ahead below and parallel to *O*'s median line of sight when the eyes were in the primary position, while the other five tracks diverged outward to the left at 10° intervals from one another. Each of these divergent tracks was the radius of an imaginary circle whose center was the point half-way between the nodal points of the two eyes. (See Fig. 1.)



FIGS. 1-3. SHOWING DETAILS OF THE APPARATUS

Fig. 1. Schematic diagram of the radial tracks
Fig. 2. Front view of a light-box
Fig. 3. Side view of one of the radial tracks

The light-boxes were 2 x 2 x 3½ in., and in each of them there was a small frosted 2-c.p. 115-volt lamp. Each box was mounted upon a block of hardwood with metal flanges attached to the bottom of it to hold it snugly upon the track and yet permit it to be moved easily along the track. (See Fig. 2, for front view.) In the front end of each light-box was a 2 x 2 in. brass plate in the center of which was cut a broken circle whose dimensions were in accordance with Landolt's optotypes.[22] The diameter to the outer edges of the circle was ½ in.; the width of the circular band and of the break was ⅛ in. The circular center of the brass plate containing the broken circle was so constructed that the break in the circle could be rotated to any desired orientation; *e.g.* top, bottom, right side, upper left, etc. Immediately behind the brass plate was set a sheet of milk-white ground glass. When the lamp in the box was lighted, the stimulus-object was seen, in direct vision, as a uniform, pale-orange, incomplete circle, a plain C set in a certain orientation. We hoped that this object would give us a crude test of the *Os'* visual acuity for the various distances and eccentricities supplied by our apparatus. The light-boxes were moved along the tracks to the desired positions by means of a system of cords and balance-weights. A strong cord was attached to each end of the wooden block upon which the light-box was mounted, and then it passed over a system of pulleys. Fig. 3 shows the arrangement of cords, pulleys, and the counter-balance weights, *A* and *A'*, for one of the light-boxes. When weight *A* was pulled downward, the light-box moved forward; when weight *A* was raised, counter-weight *A'* pulled the light-box backward. The height of *A* for a given position of the optotype on its track was indicated on a vertical scale. The pulley mechanism fell without *O*'s field of regard as he observed in the darkened room.

The current used in the lamps was from a 110-volt direct-current line, and by passing it through a Nichols rheostat it was reduced to 50-volts at a current of 0.045 amp. Preliminary tests showed that a current of this energy, 2.25 watts, gave a stimulus-object of the most satisfactory intensity for our purposes. The physical intensity of the light for all positions at all eccentricities was kept constant, since it would have been inconvenient to vary it proportionately for the numerous settings of the stimulus-object used.

The fixation-point was a small point of light with an intensity just great enough to permit it to be seen as a clear, pale yellow point without irradiations. This point was set in the front of a light-box on the track extending straight ahead in front of *O* (Fig. 1). It was 1 m. from a point midway between the nodal points of *O*'s eyes. Both it and the centers of the broken circles in the 5 light-boxes at the left were in the horizontal visual plane. Gross head-movements which would interfere with constant fixation were eliminated by the use of a biting-board. A point of light was adjusted for each *O* to the right of the point of fixation so that it fell just within the right side of the blind-spot of his right eye. When the fixation was proper, this point of light could not be seen; but it appeared as soon as the fixation slipped to the left. Although this device furnished a check only of lateral slips of fixation to the left, we trusted that since the objects to be observed were all in the left portion of the visual field it would reveal the slips of fixation which were most likely to occur.[23]

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[22] See E. Landolt, Die Reform der Bestimmung der Sehschärfe, *Arch. f. Ophth.*, 64, 1906, 598-611.

A Zimmermann kymograph controlled the 1-sec. exposure of the stimulus-objects: A sheet of paper with a horizontal slit upon the drum of the kymograph permitted a brush-contact to come against the revolving metal surface of the drum for 1 sec.[24] A system of switches enabled the experimenter to send the current from the timing device to any one of the light-boxes. The timing device, switches, and the balance-weights which the experimenter adjusted for the various positions of the light-boxes were all hidden from *O*'s view in a small booth made of beaver-board.

Procedure. The entire investigation was conducted in a dark-room. Ten minutes were allowed at the beginning of each experimental hour for partial dark-adaptation. The experimenter used in his booth a weak light while setting the light-boxes and recording results. This light was never on while the *O*s were observing and it did not interfere with their dark-adaptation since they kept their eyes closed while the light was on.

In the first series *O* made monocular observations with the right and left eyes as well as binocular observations in order to compare monocular with binocular vision under the conditions of our investigation. It was necessary to have such a comparison if we were reliably to determine how much each eye contributed to the binocular perception. The binocular and monocular observations were made in haphazard order, *O* being instructed before each observation to use both eyes, the right eye, or the left eye. When one eye was used, the other eye was covered with a blind. In the course of each experimental period (one hour) *O* made as many trials as time permitted.

The positions at which the stimulus-objects were presented were determined relative to the Müllerian horopteric circle.[25] They were either on the Müllerian circle or at the following distances before or behind it for the 5 eccentricities used: before, 20 cm., 40 cm., 60 cm. (excepting at eccentricities of 40° and 50°);[26] behind, 20 cm., 40 cm., 60 cm., 80 cm.[27] The stimulus-objects were presented at these positions in a haphazard order which was combined with the haphazard order of the binocular and monocular observations. In this series each position was used only 3 times, once for the binocular observations and once each for the two sets (right, left) of monocular observations.

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[23] When observations were made with the left eye alone, we had to depend entirely upon *O*'s unaided detection of slips of fixation since it was not possible to employ a similar point of light for the left eye which would not interfere with the light-boxes.

[24] The average of 50 trials was 1000.45 σ ± 70.12.

[25] Although the horopteric circles of the individual *O*s varied slightly from one another we held the circle at a constant average value.

[26] The apparatus did not permit the employment of a position nearer than 40 cm. before the circle at eccentricities of 40° and 50°.

[27] For convenience the various positions will be referred to in this manner: I indicates the radial track 10° to the left of the median line of regard, II indicates the radial track 20° to the left of the median line of regard, etc.; "H" following the Roman numeral indicates that the stimulus-object was set at the position where the horopteric circle intersects that particular radius; *e.g.* 20+ following the Roman numeral indicates that the stimulus-object was 20 cm. *before* the horopteric circle; and 20— indicates that the stimulus-object was located 20 cm. *behind* the horopteric circle.

The following instructions were read to *O* at the beginning of each experimental period:

> When the signal 'ready' is given, fixate the point of light directly in front of you. Make sure that the blind-spot light at the right is not visible. Shortly after the signal 'now' you will be shown a lighted area to the left of the fixation-point. Carefully observe this lighted area without change of fixation. When it has disappeared, close your eyes and report orally a descriptive account of what you have seen. Do not fail to report all slips of fixation which occur between the signal 'now' and the disappearance of the lighted area.

No mention of 'doubleness' was made in the instructions because we wished to see whether *O* would report it without an instructional hint. When a slip of fixation occurred, the observation was repeated later in the course of the experimental period.

Observers.[28] The eyes of all *O*s, excepting *D*,[29] were examined by an oculist, Dr. A. C. Durand. The results of the examination for the several *O*s were:

Fe: Vision without drops: R = 20/25; L = 20/40
with drops: R = 20/15 (lens: +cyl. 0.37, ax. 60) 9° exophoria
(cycloplegia) L = 20/40 (lens: +sph. 0.25, —cyl. 1.00, ax. 15) 1° left hyperphoria
Many fine dots in centers of crystalline lenses and many fine opacities in peripheries of both lenses. Abnormalities in both foveas, more marked in the left.

Fo: Vision without drops: R = 20/30+; L = 20/30
with drops: R = 20/30+ (lens: —sph. 0.25, —cyl. 0.37, ax. 100) 1° exophoria
L = 20/30 (lens: —sph. 0.25, —cyl. 0.75, ax. 75)
Media clear, except for remains of pupillary membrane across right pupil; fundi negative.

K: Vision without drops: R = 20/15; L = 20/15
with drops: R = 20/25? (lens: +sph. 0.50, + cyl. 0.75, ax. 105)
L = 20/30 (lens: +sph. 0.87, +cyl. 0.25, ax. 90) 1½° esophoria
Media clear; fundi negative.

L: Vision without drops: R = 20/20+; L = 20/15
with drops: R = 20/40-1 (lens: +sph. 0.50, +cyl. 0.75, ax. 105)
L = 20/50+2 (lens: +sph. 0.50, +cyl. 0.50, ax. 90) 2° esophoria
Media clear; fundi negative.

R: Vision without drops: R = 20/15?; L = 20/15?
with drops: R = 20/25? (lens: +sph. 0.25, +cyl. 0.25, ax. 90)
L = 20/30 (lens: +sph. 0.37, +cyl. 0.37, ax. 90) 1° exophoria
Media clear; fundi negative.

With all *O*s, vision was brought to 20/15 with lenses.

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[28] The regular observers for the entire course of this series were Miss M. Davidson (*D*), Dr. S. Feldman (*Fe*), and Mr. C. W. Fox (*Fo*). Shortly before proceeding to Series 2, Mr. E. Kemp (*K*), Miss E. Lowenstein (*L*), and Mr. M. Roff (*R*) made a few observations in this series. *K* completed the first 75 observations of this series; *L*, the first 43; and *R*, the first 96. All *O*s were trained in psychological observation. *Fe, Fo, K,* and *R* were members of the staff. *D* and *L* were graduate students in psychology.

[29] *D* left the university before the ocular examinations were made.

Results. Before considering the few reports of "doubleness" (two objects) obtained in this series, we shall present the information gained from a careful examination of our *O*s' descriptive accounts concerning the binocular and monocular appearance of the broken circles at the various peripheral positions. In general, the results obtained from the 3 regular *O*s agree, and, although fewer in number, the results obtained from the other 3 closely agree with these.

With very few exceptions *O* perceived the object as a well-defined circle with a break in it which could be definitely localized only when the object was presented at an eccentricity of 10° either on the horopteric circle at 20— or at any of the + positions in front of the circle at that eccentricity. Even at these positions the break in the circle was not always correctly localized, but in most cases of incorrect localization *O* stated that he had seen the break clearly when the light appeared and then had forgotten its location when reporting.

At the positions either on the horopteric circle or in front of it on the 20° radius, as well as at 60+ on the 30° radius, *O* still saw the objects as orange or yellowish circles. At these positions, however, the contours were extremely blurred, and the break in the circle was usually not seen, although some of the reports showed that *O* had noticed a blurred area 'somewhere' in the circular band of light. At the other positions on the 10°, 20°, and 30° radii, the objects were usually seen as circular or oval masses of yellowish light in which slightly darker centers could sometimes be distinguished. At the most remote positions on these radii *O* often saw nothing more than blurred patches of light.

At the + positions on the 40° and 50° radii, *O* saw the objects as relatively large, irregular masses of whitish light in which brighter and less diffuse nuclei sometimes appeared. At the more remote positions, the objects were seen as relatively small, almost point-like, circular areas of diffuse whitish light.

All *O*s except *Fe* and *K* reported that when the light appeared it apparently moved forward and then receded as it disappeared. This phenomenon was so pronounced for *D* that she assumed that the experimenter was actually moving the light forward and then backward. The 4 *O*s who observed this phenomenon did not report it in all of their observations, but their reports do not reveal that it was favored either by monocular or binocular vision or by showing the light in some particular region of the visual field. *D, Fo,* and *R* also reported gamma-movement in a number of instances; *i.e.* the light seemed to expand when it appeared and to contract when it disappeared.[30]

It is interesting to note that the *O*s were often extremely inaccurate when they attempted to estimate the distance of the more remote lights. *Fe, L,* and *R* occasionally localized lights 60— and 80— on the 10° and 20° radii as being nearer than the fixation-point. A few less striking errors were also made.

A comparison of the binocular and monocular observations reveals no striking differences between them for any *O*. As a rule, but not invariably, the binocular perceptions were clearer and included more details of the figures than did the monocular. A comparison of the monocular observations made with the right eye with those made with the left shows that for 3 *O*s the left eye gave slightly better perceptions than the right. *Fe* saw the object somewhat more distinctly with the left eye when the object was shown at the + positions along the 30° radius. At the positions along the 10° radius *L* saw the object more clearly with the left eye. *R* also saw the object more distinctly with the left eye when it was shown along the 10° and 20° radii. The reports of *D, Fo,* and *K* reveal no differences between observations made independently with the right and left eyes.

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[30] For the original description of this movement see F. Kenkel and K. Koffka, Beiträge zur Psychologie der Gestalt, *Zsch. f. Psychol.,* 67, 1913, 358-450.

Turning now to the reports of doubleness we find that, except for one doubtful report made by *D, Fo* was the only regular *O* to report doubleness. When the object was presented at V 40+, *D* reported that, although she was not certain, it seemed that just before the exposure ended she saw two blurred lights linked together. In no other instance did *D* indicate doubling.

In 16 of the 38 binocular observations, *Fo* reported the object as double. In 10 of these instances *Fo* was certain that he had seen the object double; but in the other 6 instances he was slightly doubtful. These doubtful instances occurred when the object was presented at the more remote peripheral positions, where *Fo* found it difficult to decide whether he saw the blurred patch of light single or as two blurred patches which fused into one another. Since he favored the latter interpretation in these instances they have been included with the other reports of doubleness. Eight of these 16 instances of doubleness were for positions before the horopteric circle, and the other 8 were for positions behind it. Since only 4 of the instances of doubleness occurred when the stimulus-object was at 20+ or 20— positions, we may say that the farther the stimulus-object was removed from the horopteric circle the greater was the likelihood that the perceived object would be seen as double. The doubling was also more distinct in these instances, whereas when the horopteric circle was approached the object was often seen as two interlocking circles or as two overlapping areas of light. Five of the instances of doubling occurred when the stimulus-object was on the 30° radius; the other 11 instances were about equally distributed over the other four radii.

L was the only other *O* to report doubleness in this series. In 16 binocular trials she reported doubleness 5 times.[31] When the stimulus-object was presented at position I 60+ this *O* saw two clear broken circles standing side by side with a small intervening space; but in the other 4 instances she was not at all certain that she had seen the object double. Since *L* made only 16 binocular observations, it would be pointless to attempt to indicate from her results that for her doubling is more likely to occur at certain positions than at others.

Fo's detailed reports give us interesting information about the way in which the binocular doubling of the object occurred and about the criteria which he used to determine whether the more peripherally seen objects were single or double. Occasionally *Fo* saw the object immediately as two circles of light standing side by side; but more often he first saw one circle which then "split into two circles." These circles were either interlocked or they stood side by side. When the object appeared as a blurred area of light, *Fo* observed that often it expanded laterally and then separated into two blurred areas. Frequently this separation was not complete; but where *Fo* could distinguish in the blurred area two nuclei of more concentrated light, he reported that the object was double. Eventually as soon as *Fo* observed that the object or area of light was expanding he expected that it was about to become double. In several of his doubtful reports *Fo* stated that this expansion had occurred, but that he had been unable to decide with absolute certainty that it had resulted in doubleness. So far as we can determine from *L*'s reports she observed no such development of doubleness. Neither can we determine that she used in this series any other criterion of doublness than the appearance of two objects either standing side by side or interlocked.

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[31] *L*'s reports of doubleness were for the following positions: I 60+, I 60—, I 20—, II 40+, and III.40—.

Since these results can be best discussed in connection with the results of the other series, we shall postpone further treatment of them.

SERIES 2

Since the 2 *O*s who did report doubleness in the preceding series often had difficulty in deciding whether they saw the object as single or as two closely interlocked circles, or as two nearly coinciding areas of light, and since the other *O*s did not report doubleness when the object was set at positions where it seemed almost impossible that trained *O*s should not see double, we decided to perform another and similar series of experiments with a more satisfactory stimulus-object. After a few preliminary tests with objects of various shapes had shown that a vertical bar of light could be seen double more easily than could the broken circle, we substituted the vertical bar and repeated the procedure of the previous series.

Apparatus and procedure. Except for a few minor changes which are mentioned below, the apparatus was the same as that employed in Series 1. The same wattage was used in the lamps to illuminate the stimulus-object in the front of each light-box, and the same exposure-time and dark-adaptation were used.

A vertical slit whose dimensions were 1/16 × 1/2 in. was cut in the center of the brass plate in front of each light-box. This slit replaced the C. When the light was turned on, the object was seen in direct vision as a clean-cut narrow vertical bar of light.

Fixation was controlled by having *O* maintain the fixation-point directly in the center of a circular negative after-image.[32] This negative after-image was produced by fixating briefly a small circular band of light (outside diameter, 1/2 in.) of moderate intensity.[33] When the circle of light was turned off, the fixation-point was presented immediately below. As long as constant fixation of the point was maintained, *O* saw the fixation-point in the center of the circular negative after-image, but the slightest wavering of fixation was immediately indicated by a movement of the fixation-point from the center of the after-image. The *O*s all found this method of controlling fixation much more satisfactory than that used in Series 1. Whereas they had always found it difficult to decide whether or not the light just within the blind-spot of the right eye had appeared while they were observing the object presented in the peripheral field at the left, they now found it relatively easy to detect any slips of fixation. Since we could not be certain that the circle of light presented to arouse the after-image did not disturb adaptation sufficiently to affect *O*'s peripheral perceptions, even though only a small area at the center of the retina was directly stimulated, we did not adopt this method of controlling fixation until we compared the results of two test-series made with all *O*s. In one test-series, the *O*s used this method of controlling fixation, and in the other they used the method employed in Series 1. Since we found absolutely no difference between the two sets of results, we assumed that we could safely use the second method.

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[32] This method of controlling fixation was proposed to us by Mr. Fox while he was observing in Series 1. In that series he once accidentally looked directly at the broken circle which was presented immediately to the left of his median line of regard, and when he regained his fixation he discovered that he had a circular negative after-image, but that the point slipped out of the center of the after-image if he slightly moved his eyes.

[33] Since slightly different intervals were required to arouse for the various *O*s a negative after-image which would last until the exposure of the stimulus-object had ended, the length of time that the circle of light was exposed was adjusted to meet the requirements of each *O*.

Except for a slight change in the instructions, made necessary by the change in the method of controlling fixation, the procedure was the same as that followed in Series 1. The instructions were:

> When the signal 'ready' is given, place your teeth in the biting-board and fixate the circle of light directly in front of you. After a brief interval this circle of light will disappear and a point of light will appear directly below. Fixate this point of light so that it is seen exactly in the center of the negative after-image of the circle of light which you have just seen. Shortly after the signal 'now' you will be shown a lighted area to the left of the fixation-point. Carefully observe this lighted area without change of fixation and when it has disappeared close your eyes and report orally a descriptive account of what you have seen.
>
> If a clear-cut negative after-image of the circle of light fails to appear before the signal 'now' is given, report this fact immediately.
>
> Do not fail to report all slips of fixation which occur between the signal 'now' and the disappearance of the lighted area.

Results. O's[34] descriptive accounts show that there were relatively few positions at which he saw the object as a vertical bar with sharp contours. There were many more positions at which he saw the object as a vertical bar with blurred or indefinite contours. At these positions there were often hazy streaks on each side of the object. At other times a halo of diffuse light surrounded the object. The following table shows for each *O* the positions at which he saw the object as having either clear or blurred contours.

It is important to note the wide range of positions at which most of the *O*s saw the object as a blurred vertical bar which frequently had vertical streaks of diffuse light on either side of it, since this appearance of the object probably influenced some of the reports of doubleness which are to be mentioned later.

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[34] The *O*s, for the entire series were *Fe, Fo, K, L,* and *R,* all of whom observed in Series 1. *D* was no longer available.

O	Clear	Blurred
Fe	I, 60+	I, 40+ through 40− II, 60+ through 20− III, 60+ through 20+ IV, 40+, 20+ V, 40+
Fo	I, 60+ through 60− II, 60+	II, 40+, 20+ III, 60+ through 20+ IV, 40+, 20+ V, 40+, 20+
K	I, 60+, 40+ II, 60+	I, 20+ through 60− II, 40+ through 20− III, 60+ through 20+ IV, 40+, 20+ V, 40+, 20+
L	I, 60+	I, 40+ through 80− II, 60+ through 60− III, 60+ through 20− IV, 40+ through 60− V, 40+ through H
R	I, 60+ through H II, 60+, 40+ III, 60+	I, 20−, 40− II, 20+ through 20− III, 40+ IV, 40+ through H V, 40+ through H

All *O*s saw the object as a circular, elliptical or rectangular area of light at one or two of the positions on each radius immediately behind the more remote limit of the region in which a blurred bar was seen. At all the other positions the object was seen as a blurred patch of light or as a small, indefinite point of light.

As in Series 1, *Fo, L,* and *R* frequently reported that the light moved forward when it appeared and then receded when it disappeared. *Fo* also observed that often the light expanded laterally to the right and left when it appeared and then contracted again when it disappeared. In addition to this expansion and contraction of the object *Fo* observed upon a number of occasions a sudden and dynamic brightening of the whole visual field when the object appeared. He named this phenomenon phi-brightening because it was analogous to pure phi-movement and *apparently* was not dependent upon a diffusion of light from the object itself. Three excerpts from *Fo*'s reports may help to make clear what he meant by phi-brightening.

> A very small and rather weak light which was localized rather far away appeared, and then was followed by a general brightening of the whole visual field. Then there was a darkening of the visual field and the light disappeared. . . . These changes were rather empty or analogous to pure phi-movement.
>
> A rather dim orangish light appeared rather far away at the left. Then there was this very extended, sudden brightening of the whole visual field. . . . When I see the general brightening it isn't quite like that, but it is more a dynamic movement through that part of the field . . . and yet at the same time it has to do with brightening.

There was a welling, expanding irradiation of a forceful character which brought the light farther front and made it more impressive. Then there was a gradual recession. Here the brightening is definitely a diffusion around the light making it larger and more impressive, whereas the phi-brightening has nothing to do with any such diffusion.

This phi-brightening usually occurred when the object was presented in the more remote eccentric positions; but it was occasionally observed on the 10° and 20° radii for either binocular or monocular vision.

The perception was occasionally slightly more precise with binocular than with monocular vision when the object was presented at the positions where it was more likely to be seen as a blurred vertical bar than as a bar with sharp contours or as a rectangular or elliptical area of light. At some of these positions monocular observation with one eye was sometimes superior for a given *O* than with the other. The following table shows for each *O* the positions at which he saw the object more distinctly with one eye than with the other.

O	Right Eye	Left eye
Fe	IV, 40+, IV, 20+, V, 40+, V, 20+	II, 60+ through H III, 20+, 40+, 60+
Fo	All positions on I and II	III, 60+, IV, 40+, V, 40+
K	I, 60+ through H	III, 60+ through H
L	All positions on I	
R	All positions on III	

At all the other positions there were no discernible differences between the monocular observations right and left.

An examination of the *Os*' reports for evidence of binocular doubling of the object shows that two *Os*, *Fe* and *R*, never reported it with certainty. There were several instances, however, where they believed that the perceived object might have been double. When the object was presented to *Fe* at I 60+ he reported that he saw it as a well-defined vertical line with an uncertain suggestion of doubleness about it. At positions I 40+ and V 40+, *Fe* reported a vertical bar with a vertical dark streak down the center. In later series this *O* interpreted such an appearance to mean that the object was double. When the object was presented to *R* at V 20+ he reported that it was seen as a vertical orange light with some sort of duplicity about it.

The following table shows for *Fo*, *K* and *L* the positions at which they saw the object double.[35]

Fo	K
I, 60+, 40+, 20−, 40−, 60−, 80−	I, 40−, 60−, 80−
II, 60+, 40+, (40−), 60−	II, 20−, 40−, 60−, 80−
III, 60+, 40+, 20−, 60−	III, (20−), 60−, 80−
IV, 40+, 80−	IV, 20+, H, 20−, 40−, 60−, 80−
V, H (Total 17)	V, 20−, 60−, 80− (Total 19)

L
I, 60+, 40+, (H), (20−), (40−), (60−), 80−
II, (60+), (40+), 20+, (40−)
III, 40+, (20+), (H), 20−
IV, 40+, (20+), (20−)
V, (40+), (H), (20−) (Total 21)

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[35] Parentheses around the symbol for a stimulus-position indicate that at that position *O* was not entirely certain that the object was double.

There are two instances of doubleness which *Fo* reported and several general tendencies shown by the results which should be mentioned. An examination of the above table shows that diplopia occurred slightly more frequently for *Fo* when the stimulus-object was placed behind the horopteric circle and that there was a decrease in the number of instances of diplopia with increased eccentricity. There was only one instance when *Fo* was uncertain whether the object had become double or had remained single. When the object was presented to him at position II 40—he observed that the vertical bar expanded to right and left, but he was not certain that it had become definitely double. In one instance, when the object was at V on the horopteric circle, *Fo* saw the object double when it was on the horopteric circle. At this position he first saw a single area of light, then there was a movement to the right, and finally he saw a second area of light to the right of the first.

K's results show that nearly all the instances of diplopia which he reported occurred when the stimulus-object was placed at positions behind the horopteric circle. There were also more instances of diplopia when the stimulus-object was on the 40° radius than when it was on any other radius. When the object was on the horopteric circle on the 40° radius *K* saw it as two spots of gray light.

Although *L* reported 21 instances in which she was at least moderately certain that the object was double, she was absolutely certain in only 7 of these instances. These 7 instances of certainty do not indicate that there was any given region in the peripheral visual field in which *L* was better able to distinguish between singleness and doubleness. There is evidence, however, that *the farther out* in the peripheral field the object was presented *the less likely she was to see it double.* It should be noted that 3 of *L*'s reports (less certain) of doubleness included observations made when the object was on the horopteric circle.

The ways in which doubleness appeared were practically the same for *Fo* in this as in the preceding series. Where binocular doubleness occurred, *Fo* usually saw the object first as single and then as double. There were two ways in which this change might occur. (1) Sometimes the object appeared to split into two which corresponded in size and shape to the initial single object. Sometimes there was complete separation of the two objects, but in other instances their adjacent sides overlapped. (2) In some instances the initial single object remained in its original position, but a second similar object slipped out from behind it and assumed a position to the right or left of the first. There were a few instances, however, in which, although *Fo* had not noted doubleness at the outset, he inferred that it had been there all the time since he had not *seen* the object single. The instances in which one object slipped from behind the other occurred only when the vertical bar was presented at positions within the horopteric circle. When doubleness occurred at positions beyond the circle there was usually a lateral expansion and then a separation into two objects. Although there were times when *Fo* could not distinguish sharply between the lateral expansion and a partial doubling of the object, he found that in general the doubleness was more clean-cut than it was in the preceding series with the broken circle.

The reports of *K* and *L* show no development of doubleness from an initial single object. If the object was seen double at all, it was seen double at the outset. These *O*s indicate, however, that they tended to use somewhat different cues as a basis for their reports of doubleness. *L* tended to see the more definitely formed objects double more frequently than the less definitely shaped objects, whereas *K* showed the opposite tendency.

Fo, K, and *L* also reported instances of monocular diplopia. Once when using the right eye alone *Fo* was certain that he saw the object break into two distinct objects. There were two other instances for the right eye and one for the left eye when the object appeared to him as if it might be double. *K* was certain, in one instance, that the object was double when it was looked at with the right eye alone, and there were four other instances for that eye when he 'believed' that the object was double. Using the left eye alone, *K* was very certain in 5 instances and moderately certain in three other instances that the object was double. Three of the instances of monocular diplopia occurred when the object was presented on the horopteric circle. The other instances occurred nearly as frequently when the object was presented within as when beyond the horopteric circle. The instances occurred more frequently when the object was presented on the 20°, 30°, or 40° radius than on either the 10° or 50° radii. *L* reported 22 instances of monocular diplopia, but she was absolutely certain in only 5 of these instances. Using the right eye alone 1 certain and 9 uncertain instances occurred, and using the left eye alone 4 certain and 8 uncertain instances. Approximately three-fourths of the 22 instances of monocular diplopia occurred when the object was presented at positions within the horopteric circle. They occurred most frequently on the 10° and 40° radii and least frequently on the 50° radius. Although the *Os* did not state explicitly in their descriptive accounts what the cue for monocular diplopia was, there are hints that the cue was a blurring or a diffusion of light on one or both sides of the object. It is significant that the positions at which monocular diplopia occurred were among the positions favorable to blurring or diffusion.

The results of this series will be discussed later in connection with the results of other series.

SERIES 2a

In this supplementary series *O* was instructed to draw as accurately as possible the appearance of the object when it was presented at selected positions for either monocular or binocular observation. As soon as the drawing was finished, *O* gave his descriptive account.

The series was undertaken for two reasons. We wished to obtain more accurate information about the single or double appearance of the object at various positions, and we hoped that the task of drawing the object would induce *O* to note carefully slight differences in the appearance of the object overlooked in the verbal description. The information gained from this series should also be useful in showing to what extent the various *Os* distinguished the less pronounced instances of doubleness and singleness.

The positions selected for this series were the horopteric circle, 40 cm. beyond, and 40 cm. within the circle, on each of the 5 radii. Employing a haphazard arrangement of the 15 positions used, the binocular and the monocular observations (right, left) were arranged in a haphazard order.

Except for the changes noted above, the *Os*, the apparatus, and the conditions were the same as in Series 2.

Results. The descriptive reports given by the *Os* in this series agree closely with those just described. Neither these reports nor the drawings show any striking differences, if we except the instances of diplopia, between monocular and binocular perception of the object at any given position.

Figs. 4-8 show typical drawings made by the 5 *Os*.[35a] First are given drawings made when the object was presented binocularly on the horopteric circle. All instances of binocular diplopia which occurred in this series are also included. Monocular results on the horopteric circle are omitted, since they are essentially the same as binocular. In order that the monocular observations may be compared, however, instances are included which show the object as presented to each eye alone at positions I 40—, II 40+, III 40— IV 40+, and V 40—. In general, the drawings from binocular observation at these positions are the same. Finally, all drawings which show monocular diplopia are included.

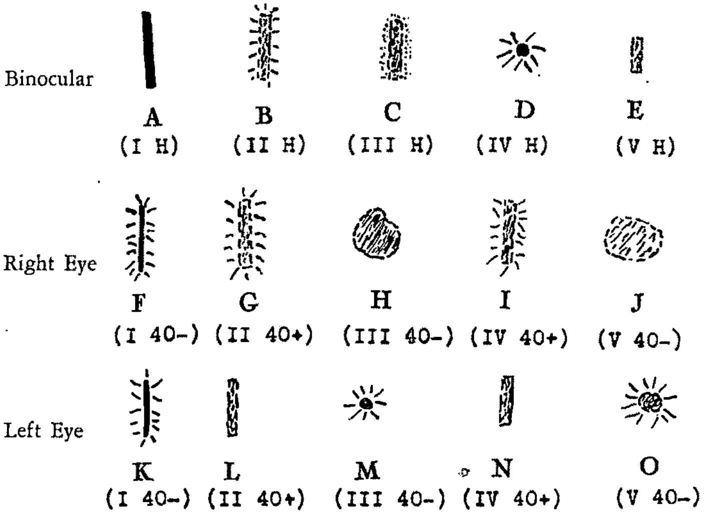

Fig. 4. Typical Drawings by *Fe*

The *Os* stated from time to time that it was extremely difficult to represent properly in a drawing the exact appearance of the object. For this reason slight differences in the drawings should not be regarded as significant. *Fo*, especially, complained throughout the series that he could not represent in a drawing the dynamic changes which the object frequently underwent, such as movement forward and backward, expansion and contraction, or the sudden splitting of one object into two. In a few instances (*e.g.*, when the object was nothing more than a

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[35a] In Figs. 5-8 all numerals not marked as minus should be read as plus values on the radii.

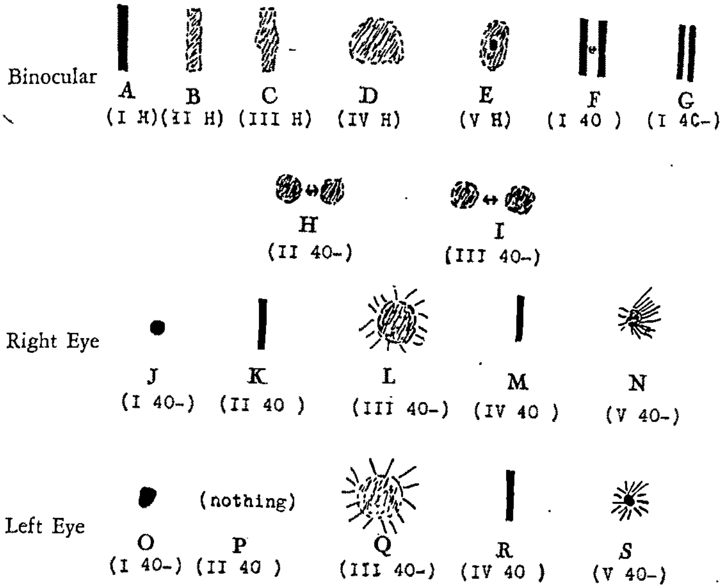

FIG. 5. TYPICAL DRAWINGS BY *Fo*

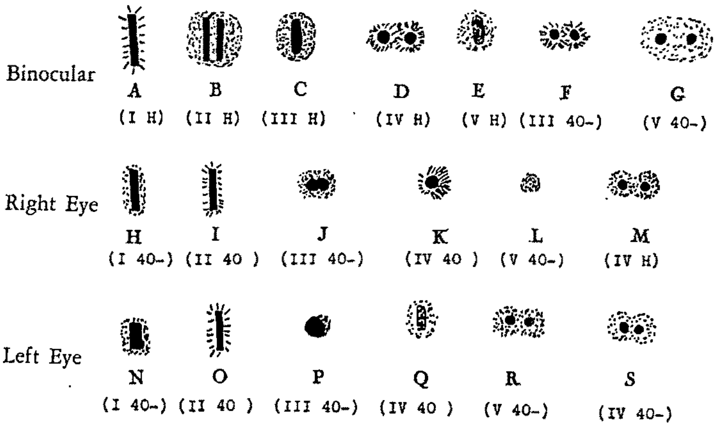

FIG. 6. TYPICAL DRAWINGS BY *K*

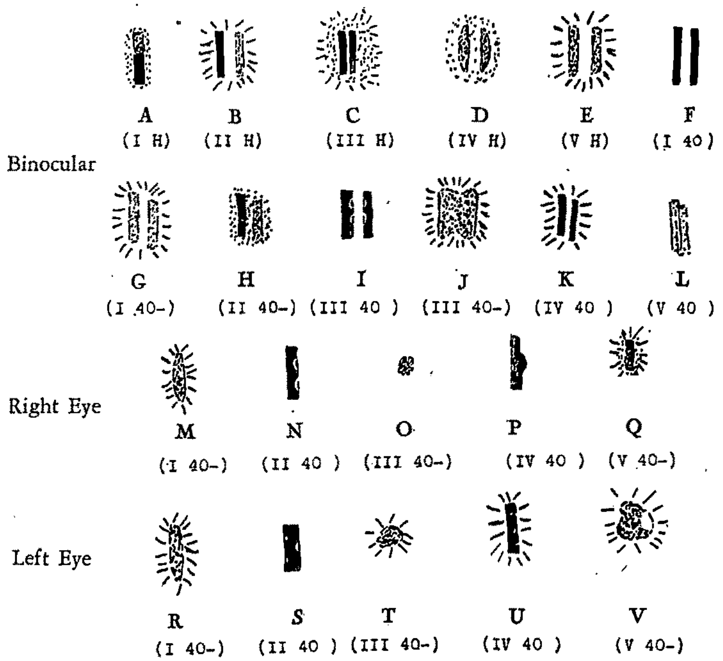

FIG. 7. TYPICAL DRAWINGS BY *L*

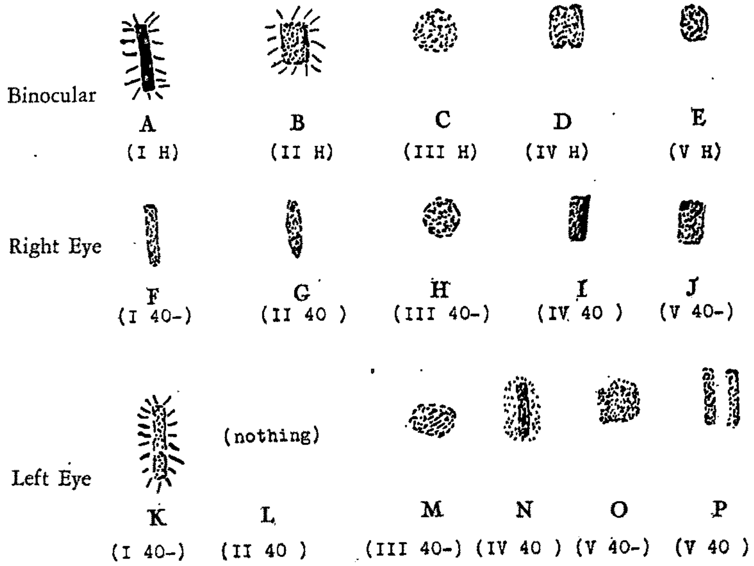

FIG. 8. TYPICAL DRAWINGS BY *R*

vague splash of light or when little more than phi-brightening occurred) he found it impossible to make any adequate drawing.

All *Os* adopted a relatively uniform way of representing various aspects of the object. Solid areas in the drawings indicate relatively solid and uniformly concentrated areas of light. Broken contours indicate that the boundaries of the object or of the diffuse light around it were hazy and indefinite, and ray-like lines indicate irradiations of light extending out from the object. Filmy, diffuse light, either within the limits of the object or beyond, is represented by stippling. The position of the object is given in parentheses below each drawing.

Fe and *R* did not report any instances of binocular diplopia, *Fo* and *K* each 4, and *L* 11. Drawings F to I, inclusive, Fig. 5; drawings B, D, F and G, Fig. 6, and drawings B to L inclusive, Fig. 7, represent the instances of binocular diplopia observed by *Fo, K,* and *L* respectively. *L's* drawing best show the various degrees of diplopia. For example, in drawings J and L two objects were not definitely separated while the *O*'s other drawings of diplopia show a pronounced separation. *L*'s drawings also indicate that there was no striking difference between the doubling on the horopteric circle and when the object was at a considerable distance from the circle.

Although *Fo* and *L* reported instances of monocular diplopia in Series 2, they did not report it at all in the present series. *K*, on the other hand, reported three instances of monocular diplopia, one for the right and two for the left eye (drawings M, R, and S, Fig. 6), and *R* reported one instance for the left eye (drawing P, Fig. 8).

When *K*'s diplopic drawings with monocular and with binocular vision are compared, no significant differences in the appearance of the double object are observable.[36]

SERIES 3

This series was undertaken in order to obtain a rough quantitative measure of the relative frequency with which doubleness occurs when the stimulus-object is placed at various positions either on the horopteric circle, within it, or beyond it. Since the results of our previous series were so varied in regard to diplopic vision under our conditions, we decided not to employ any refined quantitative method until a preliminary test had been made to show whether the results of such a procedure would indicate the region in the peripheral visual field in which objects most frequently appear single.

Apparatus and procedure. The apparatus, the stimulus-objects, the intensity of the light, and the method of controlling fixation were exactly the same as those employed in Series 2; but in this series all observations were made with binocular vision, and *O* reported only 'singleness' or 'doubleness' of the object without a detailed description.

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[36] *Fo* and *R* made no drawings when the object was presented at the position II 40+ because at this position the stimulus impinged upon the blind-spots of their left eyes.

The stimulus-object was placed at each of the following positions on the five radii: on the horopteric circle; at 10+, 20+, 30+, and 40+; and at the corresponding — positions. This gave smaller differences between the adjacent positions than those employed in previous series, and also confined the investigation to a smaller region of the peripheral field. These changes permitted a more thorough investigation of the region in which the previous results had indicated that there was a change from single to double vision. The stimulus-object was presented 20 times in haphazard order at each of the 45 positions. Two successive presentations at any one position were avoided. Doubtful reports were permitted, but the same positions were repeated later in the period without *O*'s knowledge.[37]

Instructions. The following instructions were read to the *O*s at the beginning of each experimental hour.

> When the signal 'ready' is given, place your teeth in the biting-board and fixate the circle of light directly in front of you. After a brief interval this circle of light will disappear and a point of light will appear directly below. Fixate this point of light so that it is seen exactly in the center of the negative after-image of the circle of light which you have just seen. Shortly after the signal 'now' you will be shown a lighted area to the left of the fixation-point. Carefully observe this area without change of fixation, and when it has disappeared close your eyes and report upon the singleness or doubleness of what you have seen. You may use the following terms: 'one,' 'two,' or 'doubtful.'
>
> In making your judgments try carefully to avoid guessing or making inferences. Do not fail to report all slips of fixation which occur between the signal 'now' and the disappearance of the lighted area.

Results. Table I shows the frequency with which the *O*s saw the object double when the stimulus-object was placed twenty times at each of the positions.

TABLE I

NUMBER OF TIMES THE OBJECT WAS REPORTED AS 'TWO' AT THE VARIOUS STIMULUS-POSITIONS

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
	10°	18	16	18	18	20	20	19	20	20	169
	20°	19	14	18	20	19	20	19	19	19	167
Fe	30°	18	17	19	19	20	20	20	20	20	173
	40°	10	13	17	15	16	17	18	18	16	140
	50°	8	9	16	16	19	18	18	19	20	143
Total		73	69	88	88	94	95	94	96	95	792
	10°	17	20	19	17	15	15	18	18	18	157
	20°	11	16	19	18	13	14	17	18	19	145
Fo	30°	17	19	18	15	14	14	9	16	16	138
	40°	5	11	13	12	8	9	6	5	8	77
	50°	2	3	2	1	0	2	1	1	1	13
Total		52	69	71	63	50	54	51	58	62	530

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[37] The *O*s were the 5 who served in Series 2 and 2a. Although *Fe* and *R* had not previously reported binocular diplopia, they were included to determine whether they would observe it under the more specific instructions of this series.

TABLE I (*Continued*)

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
	10°	0	1	0	2	1	1	8	18	17	48
	20°	7	3	5	13	12	17	18	19	19	113
K	30°	0	1	4	14	18	16	20	20	19	112
	40°	1	3	14	19	16	20	20	20	18	131
	50°	0	0	4	8	18	17	19	20	20	106
Total		8	8	27	56	65	71	85	97	93	510
	10°	20	19	19	20	19	19	19	19	16	170
	20°	20	20	20	15	9	5	4	5	5	103
L	30°	20	20	20	11	5	1	1	0	1	79
	40°	19	19	14	5	2	0	0	1	0	60
	50°	18	18	6	2	0	0	0	0	0	44
Total		97	96	79	53	35	25	24	25	22	456
	10°	2	0	0	0	0	0	0	0	0	2
	20°	0	1	0	0	0	0	0	0	0	1
R	30°	6	2	1	0	0	0	0	0	0	9
	40°	11	6	1	0	0	0	0	0	0	18
	50°	16	18	11	10	4	0	0	0	0	59
Total		35	27	13	10	4	0	0	0	0	89

There is a surprising amount of variability in the results of the 5 *O*s, as is shown by the differences in the total number of reports of doubleness and by the regions in the peripheral field where the doubleness occurred most frequently.

The table shows that for *Fo* and *L* an increase in the eccentricity at which the object was seen was accompanied by a decrease in the number of instances of doubleness. In the case of *Fo* this decrease was sudden after 30° and with *L* it was sudden after 20°. *Fe*'s results indicate a general increase in doubleness with increased eccentricity, but there were slight inversions at 30° and 50°. With *K* there was a general increase up to 40° and then a decrease at 50°. *R* showed an increase beginning at 30°, with most instances of doubleness at 50°.

If we consider the frequency of doubleness in relation to the nearness or remoteness of the object, we again find variability in the results of the 5 *O*s. Except for the inversion at 30+, *Fe*'s results show a slight increase in the instances of doubleness as we proceed from positions 40 cm. before the horopteric circle to the circle itself. From that distance on, the totals are approximately the same for all positions. A large amount of the increase occurred when the stimulus-object was presented on the 40° and 50° radii. In the case of *Fo,* there was an increase from 40+ to 20+, then a decrease, and finally an increase at 30— and 40—. These changes, as in the case of *Fe,* were most pronounced on the 40° and 50° radii. *K*'s results show a general increase with greater distance from the observer except for the inversion at 40—. These results show quite definitely that a ratio existed between the remoteness and vagueness of the object and the probability that it would be seen double. *L*'s results in this respect are opposed to *K*'s. With *L* the probability of seeing the object double decreased with remoteness or with decreased definiteness. While there was very little variation along the 10° radius, along the other radii there was a sharp decline in the instances of doubleness from positions in front of the horopteric circle. *R*'s results show clearly that for him there was a very limited region in which doubleness might occur. The possibility increased with the approach to the positions nearest the *O* and with greater eccentricity.

When the object was on the Müllerian circle all of our *O*s except *R* reported a larger number of instances of diplopia than would have been expected in the light of the traditional doctrine. *Fe, Fo,* and *K* reported 50% or more of the instances on the horopteric circle as double.[38]

When we consider that *Fe* had not reported binocular diplopia prior to this series, it is worthy of note that in this series he reported many more instances of diplopia than any of the other *O*s. His reports of doubleness increased as the series progressed. Of his first 450 observations, he reported 359 as double, and in the second 450 observations he gave 433 reports of doubleness.

The results also show, as can be seen from Table II, that the *O*s varied greatly in the number of doubtful reports given. An examination of the table will show that when the stimulus-object was presented at positions on the 40° radius *Fe* and *Fo* had the greatest difficulty in deciding whether the object was single or double. Decision was unusually difficult at that eccentricity for *Fe*. *L* also had considerable difficulty when the stimulus-object was presented on the 40° radius, but her greatest difficulty was on the 20° radius. *K*'s reports of diplopia show that he tended to report ambiguous and ill-defined objects as double, and it is probable that any confusion as to singleness or doubleness immediately led him to report

TABLE II

NUMBER OF TIMES THE OBJECT WAS REPORTED AS DOUBTFUL AT THE VARIOUS STIMULUS-POSITIONS

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
Fe	10°	4	10	5	6	2	3	0	0	1	31
	20°	6	2	5	5	2	3	3	2	3	31
	30°	9	9	7	1	2	1	3	3	2	37
	40°	5	9	31	14	11	19	11	5	7	112
	50°	13	17	11	8	9	3	9	9	7	86
Total		37	47	59	34	26	29	26	19	20	297
Fo	10°	0	0	1	3	3	2	5	3	0	17
	20°	1	2	3	3	7	5	8	3	5	37
	30°	6	2	3	7	12	8	9	7	4	58
	40°	1	9	11	3	10	19	14	6	12	85
	50°	1	4	1	1	4	4	2	7	4	28
Total		9	17	19	17	36	38	38	26	25	225

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[38] It should be noted that, although in Series 2a *Fo* reported that he did not see the object when it was presented to the left eye at position II 40+, he reported in this series 11 instances out of 20 as being double at that position. Failure to see the object was probably due to the fact that at that position the object impinged upon the blind-spot of his left eye.

TABLE II (*Continued*)

O	Radius	Position of stimulus-object									Total
		40+	30+	20+	10+	H	10−	20−	30−	40−	
	10°	0	0	0	0	0	4	0	0	0	4
	20°	1	0	0	0	0	0	1	0	0	2
K	30°	0	1	2	0	0	0	0	0	0	3
	40°	0	0	0	0	0	0	0	0	0	0
	50°	0	1	0	0	1	0	0	0	0	2
Total		1	2	2	0	1	4	1	0	0	11
	10°	0	0	1	4	7	4	1	8	3	28
	20°	0	0	1	10	21	4	13	3	4	56
L	30°	0	0	1	14	5	4	1	0	4	29
	40°	1	5	13	6	7	2	3	1	0	38
	50°	1	4	9	7	3	1	1	1	0	27
Total		2	9	25	41	43	15	19	13	11	178
	10°	0	0	0	0	0	0	0	0	0	0
	20°	0	0	0	0	0	0	0	0	0	0
R	30°	0	1	0	0	0	0	0	0	0	1
	40°	4	2	0	0	0	0	0	0	0	6
	50°	2	1	3	5	3	0	2	0	0	16
Total		6	4	3	5	3	0	2	0	0	23

'double,' which would explain the scarcity of doubtful reports. It will be seen that *R*'s reports of 'doubtful' usually occurred when the stimulus-object was presented at positions where a report of diplopia was most frequently given.

SERIES 4

The results of Series 3 indicated the need for more exact information concerning the basis upon which the *O*s distinguished doubleness from singleness. The large number of doubtful reports given in the preceding series showed that the *O*s did not, in all instances, immediately note the difference. There were apparently a number of cues which served as a basis for the report. Some of these cues had already been brought out in the previous series; but it seemed possible that we might, since the observers were now more thoroughly familiar with the experimental situation, obtain more exact and complete information.

Apparatus and procedure. The apparatus and the experimental conditions were the same as in Series 3. The 5 *O*s, using binocular vision alone, were asked to describe the appearance of the vertical bar of light when it was presented at each of the 45 positions employed in Series 3. The instructions were practically the same as those used in Series 2.

O	Single	Double	Doubtful
Fe	3	35	7
Fo	24	6	15
K	25	20	—
L	30	5	10
R	42	3	—

Results. The preceding table shows for each *O* the number of instances of doubleness and singleness, and also the number of doubtful instances.[39]

Fe's reports of doubleness were distributed fairly uniformly over all 5 radii with a slight increase for positions beyond the horopteric circle. The majority of his doubtful reports occurred when the stimulus-object was presented within the horopteric circle on the 40° and 50° radii. *Fo*'s reports of doubleness occurred only when the stimulus-object was placed at positions on the 10°, 20°, or 30° radii, and were scattered for positions within or beyond the horopteric circle. The object was also reported double at II H. Most of *Fo*'s doubtful reports were given for positions beyond the horopteric circle and on the 40° and 50° radii. With one exception (II 30+), *K*'s reports of doubleness were given when the stimulus-object was placed between the most remote positions, 40— and 10+. More doubleness occurred when the stimulus-object was on the 30°, 40°, and 50° radii than when it was on the 10° and 20° radii. *L*'s 5 reports of doubleness were given when the stimulus-object was presented at positions I 40+, I 30+, II 40+, III 40+, and III 20+. Her doubtful reports show that she had greater difficulty in distinguishing between singleness and doubleness on the 10° radius and at near positions on the 40° and 50° radii. At all the far positions the object was seen as single. *R* saw the object single everywhere except at positions IV 30+, V 40+, and V 10+. No doubtful instances.

It will be seen that, although these results are much fewer in number, they agree in general tendency at least with those of the previous series. This permits us to assume that the *Os*' comments in this series may be regarded as applicable to the previous series.

The comments supply much information about the appearance of doubleness at various positions and about the cues used in deciding upon the singleness or doubleness of an object in various instances. *Fe*'s reports show that in those instances in which he saw the object double he perceived two adjacent areas of light separated by a black strip or by a vague black region. Most of *Fe*'s doubtful reports were given when he perceived a black strip but was unable to localize it definitely with reference to the rest of the object. In a few instances he reported that at first he saw the object as single with a dark streak down through the center, but that later he reapprehended it as two objects. The reports given for instances of doubleness which occurred when the stimulus-object was placed at the various positions on the horopteric circle indicate that there was no significant difference between the doubleness which occurred there and that which occurred when the stimulus-object was placed either 10 cm. beyond or 10 cm. within the horopteric circle. General comments which *Fe* made near the end of this series furnish information which enables us to understand why he never reported doubleness in Series 1 and 2, and reported it frequently in Series 3. Since he had never perceived doubleness in the peripheral field he did not look for it until it was mentioned in the instructions used in Series 3. As soon as he began to look for it he noted that the object which he had previously taken to be single was frequently separated into two by a central vertical black strip. Any indication of a black strip was then taken to mean that the object was double. He also reported that when there was any marked blurring of the light he tended to give a doubtful report, because he was not certain of the presence or absence of the black vertical strip or patch.

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[39] Observations in which a doubtful report was given were not repeated in this series, as was done in Series 3.

Fo's descriptive reports of doubleness and of the appearance of the object agree closely with the reports he gave in Series 2. As before, the object was almost always single when it first appeared, and then it split into two objects. The splitting of the object was usually intimately connected with gamma-movement, and in several instances *Fo* could not separate the two; *i.e.* the object could be regarded as a single object which expanded laterally or as one object which became double. Observations of this kind often led to doubtful reports. There were instances when *Fo* gave doubtful reports because he could not decide whether a vague, hazy patch of light at one side of the clearer area of light was actual diplopia or whether it was a fragmentary halo belonging to the brighter area. When the stimulus-object was placed very near on the 40° and 50° radii, *Fo* reported that the object had an unusual molten appearance which tended to prevent it from being seen double.

K's reports in this series give little information as to his criteria of doubleness when the separation was not definite. He stated that he tended to give reports of doubleness if the light was blurred and contained two regions of greater brightness, and also that, as the experiment progressed, he expected the distant objects to appear double.

Toward the close of the investigation *L* reported fewer instances of doubleness than at the beginning. She stated that at the outset she was unable to distinguish between singleness and doubleness when the object appeared at the more distant positions because she confused blurring and doubleness. Later she rarely reported that the object was double unless she noticed that there were two definite objects with a distinct separation. Nearly all relatively shapeless or blurred objects were reported as being single. Her doubtful reports usually indicated that the object looked as though it were about to separate, but that the separation was too slight for the object to be actually double. *L* also reported that she found difficulty in overcoming the tendency to anticipate doubleness in objects near her.

Since *R* gave so few reports of doubleness, we have very little information concerning the cues on which he based his reports. The 3 instances of diplopia occurring in this series were all instances where the object was definitely seen as two objects separated by a dark area. There is nothing in *R*'s reports which indicates why he occasionally saw the objects double when the stimulus-object was placed at the more advanced positions on the 30°, 40°, and 50° radii, and rarely saw it double when the stimulus-object was placed at similar positions on the 10° and 20° radii.

Discussion of Results

The results given above do not demonstrate the existence under the conditions of this investigation of an empirical horopter which corresponds in any way to the Müllerian horopteric circle. Neither the generalized results of all the *O*s nor the results of any single *O* indicate that objects lying in a region adjacent to the Müllerian circle will necessarily be seen single and that objects lying elsewhere will necessarily be seen double.[40] The most that our results do indicate in this respect is that, for a given *O*, there are regions in the peripheral field where an object will be seen single more frequently than it will be seen double. These regions varied greatly from one *O* to another, so that we must attempt to account for the enormous individual differences.

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[40] Perhaps the results of *R* should be excepted, but they vary so widely from the results of the other observers that we shall disregard them temporarily.

The results of Series 3 show best in quantitative form the variability of results. The explanation of this wide variability can be based most directly upon the descriptive reports and the comments which the *O*s made in the earlier series of this investigation. These reports show that often *O* could not tell immediately from the appearance of the object whether it was single or double. As the investigation progressed each *O* worked out for himself criteria of doubleness. Although specifically instructed not to make inferences, the several *O*s' criteria of doubleness differed so widely that the general results showed marked variability.

In peripheral vision an object undergoes changes in its appearance, and, at times, some of these changes were utilized by *O* to enable him to decide whether the object was single or double. There were instances when *O* actually saw two objects standing side by side with a definite separation between them, but at those positions where the object was decidedly blurred and vague *O* took certain characteristics of the object to mean that it was single or double.

Brief consideration of a few of the dioptric factors involved will enable us to see more clearly the basis for much of the difficulty that our *O*s encountered. One dioptric factor which may have given no little difficulty is that of monocular diplopia caused by monochromatic aberration of the lens.[41] We have evidence that it did play a part, since in those series where the *O*s made monocular observations they occasionally reported the objects double. It is possible that this factor gave rise to some of the reports of doubleness which were made when the stimulus-object was set on the horopteric circle.

Another dioptric factor which may have partially determined the results was oblique astigmatism. Ames and Proctor[42] have shown that in the peripheral field there is, with a given accommodation, a very limited field in which an object may be placed so that it is not seen blurred as a result of oblique astigmatism. Since few of the positions at which our stimulus-object was placed fell in this field, this factor alone did much to prevent the object from being seen as a clear-cut figure with definite boundary lines.

Although the reports indicate that the dioptric factors had a fairly uniform influence upon all *O*s, so far as the appearance of the object was concerned, there were significant differences in the way these changes were interpreted by the *O*s to enable them to decide whether the object was single or double.

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[41] This phenomenon is discussed by Helmholtz, *Hdbh. der physiol. Optik*, I, (3d ed.), 1910, 161-163.

[42] A. Ames and C. A. Proctor, Dioptrics of the eye, *J. Opt. Soc. Amer.*, 5, 1921, 22-84, esp. 52-79.

One important factor in determining whether an object would be seen single or double was the instruction under which *O* worked. The importance of this factor is well shown in the case of *Fe*. During the early part of the investigation it did not occur to him that the peripherally seen objects might be seen double, and so he made no attempt to observe it. Later (Series 3), when *Fe* was instructed to report instances of singleness and doubleness, he reported doubleness more frequently than any other *O*.

To understand more adequately why *Fe* reported doubleness so frequently in Series 3 and 4, we must refer to his verbal reports. In the first few instances he noted that the two objects were separated from one another by a dark streak. He accepted the presence of a dark streak between areas of light as a criterion of doubleness. After a number of repetitions he then tended to report doubleness whenever he noted a black streak or area related to the object. Due probably to defects of his eyes,[43] *Fe* frequently perceived dark areas or streaks in the object. Once he was primed for these he naturally observed them more and more frequently as the experiment progressed.

Fo and *L* reduced the number of their reports of doubleness toward the end of the experiment, because with repetition they succeeded better in distinguishing diplopia from a figure composed of a vague object and a faint, detached halo. These two *O*s became more rigid in their criteria of doubleness, in that doubleness came to be something more seen than inferred. Having simplified and reduced their cues for doubleness, *Fo* and *L* reported less doubling when the stimulus-object was at the more eccentric and at the more remote positions.

Another factor which probably increased the number of *Fo*'s reports of doubleness was the connection which he observed between gamma-movement and separation. There were instances when *Fo* reported that he was uncertain whether the splitting had occurred; but there were probably other instances when he unintentionally inferred that the two had taken place together. In some of the instances of diplopia which he reported when the stimulus-object was on the horopteric circle he mentioned that gamma-movement had occurred.

K tended to report more doubleness with increased distance and eccentricity of the stimulus-object. When the object was seen as a blurred area he usually saw two regions of brighter light in it which he interpreted to mean that the object was double. On the other hand, a clear, definitely outlined figure was more insistently single.

There is nothing in *R*'s verbal reports which enables us to determine why he saw the object double much more rarely than the other *O*s and then only in a very limited region of the peripheral field. Evidently *R* did not report that an object was double unless he saw it as two objects with a distinct separation; but it is difficult to see why the object was seen thus only at the more eccentric near positions and not at the less eccentric positions.

Our results show that in this investigation seeing the object as single or double was relatively independent of its being imaged upon corresponding or non-corresponding retinal points. Central factors were usually much more important in determining the singleness or doubleness of the object.

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[43] See the reference in the oculist's report above to opacities in the lenses and to abnormalities near the foveas.

We may now attempt to explain why our results differ so radically from those of other investigators who have experimentally attacked the problem of the horopter.

One explanation which immediately offers itself is that others have generally used the criterion of the apparent plane; that is, finding the region of the visual field in which objects may be placed so that they appear to be in a straight line which is perpendicular to the median line of regard at the point of fixation; whereas we, in attempting to determine the empirical horopter, instructed our *O*s to report upon the singleness or doubleness of peripherally seen objects. From the point of view of the definition of the horopter as the locus of all points seen single with a given fixation of the eyes, our method of investigation seems psychologically more valid. Other methods, such as those used by Tschermak and his students, are based upon a more indirect, more physiological approach to the problem. By our method we are better able to discover the extent to which doubleness and singleness can be differentiated in various portions of the peripheral field.

Another factor which may help to account for the difference existing between our results and those of other investigators who have found that the empirical horopter is a band which surrounds and includes the Müllerian circle or lies within or beyond the Müllerian circle is that they worked with a lighted field while we worked with a darkened field. A lighted field permits more precise localization of the object, and knowledge of the location of the object with reference to the fixation-point might easily predispose sophisticated *O*s to see objects double only when they were much nearer or much more distant than the fixation-point was. We must not forget either that many of the investigators served as their own *O*s and thus, in many instances, knew fairly well the approximate location of the stimulus-object. It is not surprising, therefore, that, under their conditions, the empirical horopter was found in many instances to be a band. Since our *O*s worked under dark-adaptation they lacked all definite knowledge of the location of the stimulus-object. This statement is verified by their own comments. When, in their reports, they volunteered information about the location of the object it was shown that they frequently made gross errors. At other times, they stated that they were not at all certain how far away the object was. Therefore, since *accurate* localization played so small a part in their observations it is highly probable that at least some of the *O*s saw the object as double at certain positions, whereas they would have seen it single if it had been definitely localized for them as being, for example, at about the same distance as the fixation-point. It is unfortunate that time did not permit us to complete another series of observations with the same *O*s working under light-adaptation in order to test the possible influence of relatively definite localization of the object upon seeing it double at various positions in the visual field.

There is still another way in which the employment of a lighted object under the conditions of dark-adaptation may have increased the number of instances of doublings. A non-fixated object standing out clearly from its surroundings is more likely to be seen double than is one which is less sharply differentiated from the background. The writer has frequently noted that in binocular peripheral vision bright clean-cut objects tend to become double while less sharply defined objects in the same position tend to remain single. If a high degree of differentiation from the background does enhance doubling, then the frequent appearance of doubleness for our *O*s becomes intelligible. It is possible that even *R*, who saw doubleness infrequently, might have seen it less frequently with weaker stimulus-objects.

We may say, as a concluding statement of this discussion, that there is no single group of factors which will adequately account for all the results of all our *O*s. It is obvious that the physiological factor of the stimulation of corresponding and non-corresponding retinal points was not nearly so important a determiner as we might have expected it to be. From our results we might almost conclude that corresponding points have little functional significance except at the fovea, so far as binocular single vision is concerned. The dioptric factors involved in peripheral vision alone will not adequately account for the results. A change in the appearance of the object due to dioptric factors was accompanied in the case of one *O* by an increase in the instances of doubleness, while the same change was accompanied in another *O* by a decrease in these instances. Apparently the only constant and direct determiner was that of instruction, formal, self, and occasional,[44] and since this determiner often took different directions in the various *O*s, the results varied widely and took sudden turns when new instructions of any kind replaced a previous instruction. The sudden appearance of frequent reports of doubleness in the case of *Fe*, when he was instructed to distinguish between the singleness and doubleness, is an excellent example of the effect of formal instruction. The fact that *L* instructed herself to report vague, unclear objects as single, while *K* instructed himself to report them as double is an example of self-instruction taking opposite directions. One of the many instances of the effect of occasional instruction in determining the results obtained in this investigation was *Fo*'s tendency to report doubleness when he observed gamma-movement. He did not instruct himself to report doubleness when gamma-movement occurred, but after doubleness and gamma-movement had occurred together in a few instances he found that the appearance of the latter phenomenon alone frequently primed him to report doubleness even when he was not certain that he had seen a double object.[45]

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[44] M. Bentley, *The New Field of Psychology, Pt. i, The Psychological Functions*, 1933, 78-79.

SUMMARY

This study was undertaken to investigate by experimental means the significance of the horopter in visual perception. An attempt was made to determine under dark-adaptation those regions in the left portion of the visual field where a lighted object on a lightless ground is seen single and the regions where it is seen double. (1) Preliminary trials were first made with a broken circle as stimulus-object. The *O*s were instructed to report a full description of the object both in monocular and in binocular vision. Diplopia was not mentioned in the instructions because we wished to see whether *O* would discover it for himself. (2) When a vertical bar had been substituted as a stimulus-object—in order to facilitate the discrimination of single and plural objects—a like set of binocular and monocular observations was carried through. (3) In a supplementary series the *O*s made drawings of the vertical bar as seen with one or both eyes at certain selected positions. (4) Then each *O* made a series of 900 binocular observations in which he reported the object as single or double when the stimulus-object was presented at any one of the 45 positions (9 distances along each of 5 radii). (5) Following this series the *O*s were again instructed to describe the object and to state the criteria for doubleness at each of the 45 positions.

We found that a group of central factors is a more important determiner of singleness and doubleness than are retinal correspondence and non-correspondence. Since these central determiners varied in kind and in precision among our various *O*s, notable individual differences came to light in their binocular perception of single and plural objects from a single eccentric source of illumination. The results obtained by this experimental procedure fail to demonstrate any fixed region corresponding in any way to the Müllerian circle wherein objects are seen as single and outside of which they appear double. Since the doctrine of corresponding points implies the existence of a region in the binocular field wherein objects will be predominantly double for all observers with normal vision, and, since such a region or *horopter* was not found in this study, the significance of the doctrine of corresponding points is called into question, so far as it applies to the peripheral field under the conditions of this investigation.

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[45] Since the completion of the experiments reported here, we have begun a new series of experiments which indicate that a shift from binocular to monocular, or from monocular to binocular, apprehension of objects in the peripheral visual field enables the *O*s to distinguish more accurately between singleness and doubleness. The shift from binocular to monocular vision, or the reverse, is accomplished by covering either the right or left eye by silent blinds for the first or second half of the exposure-period. The complete results will be published later, but we may mention here that they also fail to confirm the geometrical horopter.

PROGRESSIVE CHANGES IN NON-FOVEAL PERCEPTION OF LINE PATTERNS

By Marjorie Bullard Drury, Cornell University

It is a commonplace that the activities of any living animal can be fully understood only when the developmental history of the animal has been made out. The commonplace applies as well to psychological as to biological history, for the psychological functions of the individual are no more static and fixed than the biological functions are. They are constantly modified under use and growth. Just as the body grows and develops, so does every psychological function undergo significant changes which may continue either for a short time or throughout a large part of the life-course of the individual.

The present study is concerned with one of these psychological operations, *i.e.* with perceptive apprehension, and with the changes which this operation undergoes during an exceedingly small part of the entire history of the individual. If we succeed in producing, under experimental conditions, significant functional changes of a progressive sort, we may be able to use these changes as an indicator of those larger and slower modifications which are ordinarily spoken of as 'individual development.' The primary necessity of our experiments has been to induce a functional performance at a crude and elementary level and then to watch it 'develop' at a relatively rapid rate. To meet this necessity we have chosen certain visual perceptions of unconventional pattern which depend for their initiation upon the excitation of limited, extra-foveal regions of the retina.

After the earlier post-natal years our perceptive functions are generally so definite in their courses that it is not easy to determine the developmental changes in them. Progressive modifications in perception, therefore, must be set up under conditions which permit them to be inspected, *i.e.* perceptions must be experimentally initiated which lack the stable and specific character of our definite and fixed operations and a means devised for carrying them through a series of modifications. The means adopted in the present experiments have been (1) to use stimulus-patterns that are relatively unfamiliar to the observer, and (2) to present these patterns under conditions which interfere with their full effectiveness. Whereas the

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device commonly employed in previous studies to bring about such changes has been the presentation of a stimulus-object under conditions which are *increasingly favorable to its effectiveness* (*e.g.* by extending the time of exposition), in the present investigation *constant conditions of presentation* have been used, and bare exercise under precise instruction has been depended upon for functional modification. The purpose of this method has been to avoid serious difficulties which attend the interpretation of those changes which are directly attributable to a shifting stimulus. The visual modality of perception we have used mainly because the imperfections and the limitations of peripheral vision provide a feasible and useful means for initiating perceptions of a relatively primitive sort. Possibilities are thus provided for determining whether progressive changes can be brought about, and, if so, for determining the nature of these changes.

HISTORY OF THE PROBLEM

General theories of the development of behavior. In the recent literature upon modifications in organic activity, there seems to be a continually recurring controversy between those who conceive of functional development as proceeding from original unitary totals toward organizations of greater and greater differentiation and those who conceive of it as proceeding in the opposite direction; namely, of increasing unification of originally independent parts.

In an account of recent progress in comparative psychology, Kafka suggests that both directions of change are conceivable.[1] Theories of the development of behavior he believes, therefore, should not be restricted to either type alone. He cites experimental evidence to show that there are two basic types of behavior corresponding to the motility of the organism. In the case of partially non-motile organisms (organisms that move with difficulty), the basis of behavior consists in local reactions which only gradually embrace more distant bodily parts and lead finally to reactions of the whole body. In the case of highly-motile organisms, on the other hand, the original reactions consist of those movements of the entire body by means of which the organism changes its place in the environment and only later in local reactions without progressive locomotion. Kafka believes that if this rule should be found to be generally valid, it would mean that the total coördination of behavior comes about in two ways; in the case of the less motile organisms by integration, *i.e.* by the increasing unification of relatively isolated reactions, and in the case of the more motile organisms by the differentiation of original unitary total reactions into relatively independent individual reactions. The assumption of the unitariness of behavior alone as an explanatory principle is, as Kafka believes, not justified,—for in certain cases the unitariness itself requires explanation. If, for example, the unitariness is to be considered in the true sense of the word as an integration and not as a bare summation, then, in the single reactions which apparently originate independently, a regulatory principle must already be effective which implies a reciprocal dependency of the individual reactions upon each other. It is seen therefore, says Kafka, "dass weder ein Nativismus, der von stationären Gestalten ausgeht, ohne sich um ihre Entstehung zu bekümmern, noch ein Empirismus, der voneinander unabhängige Elemente auf dem Weg einer bloss additiven Assoziation verknüpfen will, sondern erst eine genetische Methode den Tatsachen gerecht wird, welche zwar eine ursprüngliche funktionelle Beziehung zwischen allen Elementen des Seelenlebens voraussetzt, aber sowohl die Integration wie Desintegration oder Differenzierung sekundärer aus relativ primären Strukturen zum Gegenstande der Untersuchung macht."[2]

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[1] G. Kafka, Die Bedeutung des Behaviorismus für die vergleichende Psychologie und Biologie, *Ber. XII. Kongr. für der deutschen Gesellschaft für Psychol.* (Hamburg, 12.-16. April, 1931), 1931, 213-255.

Sander,[3] too, in a comprehensive survey of the results of Gestalt psychology, comes to the conclusion that *Gestalten* may change in two such ways. He declares that "Gestalten . . . stehen zwischen zwei polaren Gegensätzen ungegliederter Ganzheitlichkeit und der Stückhaftigkeit zusammenhangloser Teile. Sie stehen in dieser Mitte als etwas von beiden Seiten gleichsam Erstrebtes. Die Seele ist angelegt, diffus, Ganzheitliches zu durchformen und umgekehrt unverbunden Stückhaftes als Glieder zu einem gestalthaften Ganzen zusammenzuschliessen."[4] Articulated or differentiated *Gestalten,* that is to say, come into being either from the direction of unitary totals or from that of a coexistence of independent parts. He cites experimental evidence which will be examined later.

Two other relevant writers upon development are Coghill and Thorndike. Since the directions of development reported by them find representation also in the schemas of Kafka and Sander, it is not necessary to describe their doctrines in detail. Coghill's[5] investigations show that, in Amblystoma, development begins with certain generalized movements of the total organism and proceeds by a gradual and continuous differentiation of localized movements in various members. Thorndike[6] would have development of behavior proceed by the formation of 'connections' between 'situations' and 'responses.' His conception of bodily functions in development rests upon a fairly naïve theory of the integration of simple and independent neural units.

Conditions of visual form-perception. The investigations of conditions of form-perception fall into three classes: (*1*) those experiments which primarily deal with the central conditions of perceiving; (2) those primarily concerned with the peripheral conditions; and (3) those which aim to discover the direct effects of exercise of the perceptive functions with maximal constancy in both sets of conditions.

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[2] *Ibid.,* 223.

[3] F. Sander, Experimentelle Ergebnisse der Gestaltpsychologie, *Ber. X. Kongr. für Exper. Psychol.* (Bonn, 20.-23. April, 1927), 1927, 23-88.

[4] *Ibid.,* 61.

[5] G. E. Coghill, *Anatomy and the Problem of Behavior,* 1929. Coghill attributes this behavioral development to the nature of development in the nervous system, *i.e.* the increasing differentiation in behavior he assumes to be the counterpart of neural growth. Kuo has recently modified this correlation on the basis of experiments upon bird-embryos. He thinks that Coghill has under-rated the importance of environmental conditions to the behavior of the organism. (Z. Y. Kuo, Ontogeny of embryonic behavior in aves: III. The structural and environmental factors in embryonic behavior, *J. Comp. Psychol.,* 13, 1932, 245-271.)

[6] E. L. Thorndike, *Educational Psychology,* I, 1913; *Human Learning,* 1931.

(*1*) *Central conditions.* Studies are here included which aim to investigate the characteristics of such functions as perceiving and remembering when the influence of the stimulus upon these functions is limited. Sander states that such studies contribute to our understanding of the way *Gestalten* come to arise.[7] If his contention is correct, then these studies are relevant to the present investigation. The experiments are of two kinds: (*a*) those which have investigated the activity of perceiving when the influence of the stimulus is reduced to a minimum; and (*b*) those which have investigated the changes in reproduction of perception at successive intervals of time after the original perception.

(*a*) *Experiments with 'reduction' of influence of stimulus.* In his investigation of entoptic phenomena Purkinje found that, when the sun is viewed through the spread fingers moving back and forth before the closed eyes, the light-shadow patterns which arise show regular and symmetrical forms.[8] Goldschmidt later confirmed these observations and found similar characteristics in entoptic phenomena produced in other ways,[9] *e.g.* by pressure upon the closed eyes.

Another group of experiments cited by Sander as throwing light upon the dispositional tendencies of the organism are those of Rüsche[10] and Kirschmann.[11] Rüsche investigated the visual forms outlined by successively presented points briefly exposed, and concluded that there was a rounding out of contours. Kirschmann investigated the recognition of geometrical figures in indirect vision. He, too, found a tendency to round out contours. Polygons, for example, were seen as circles. There seems to be a similarity in the results of these various experiments in showing a tendency toward simpler and more regular figures when the relative effectiveness of the stimulus is decreased.

(*b*) *'Reproduction' experiments.* Here the stimulus is used merely to initiate the function. The *O*s are given a perceptive experience and then instructed at ever-increasing intervals to describe or to reproduce by drawing their memory of the original perceptual object.

In 1897, Philippe investigated changes undergone by memory-images.[12] The studies were first carried out with 6 adult *O*s. Five objects were employed, three of which were common and the other two less common (a screw with a flat head, a tailor's button, a tie pin with a cameo of the head of Caesar, a fleur-de-lis stamped in copper for hanging a picture, and a small Japanese mask of lacquered plaster). Having closed his eyes, *O* was presented with the object on the palm of his right hand and was instructed not to move his hand. He then drew a picture of the perceived object. Another drawing was made of the same object after *O* had handled it. He was then shown the object, which ordinarily differed little from his last presentation. At the end of 15 or 20 days, *O*, who had considered the experiment closed, was asked for new drawings. The same request was made a month later and thereafter at intervals of one or two months. The experiments were repeated on 30 pupils (5-8 yr. old) of the nursery schools of Paris. The changes in the "mental images" observed took three directions; (1) the image disappeared through a weakening or a confusion of details, (2) the details of the image were replaced by others which changed it to a different form, and (3) the images evolved through a series of changes toward an image-type, representing the groups to which the original image seemed to belong.

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[7] F. Sander, *op. cit.*, 57.

[8] J. Purkinje, *Beiträge zur Kenntnis des Sehens in subjektiver Hinsicht,* 1819.

[9] R. H. Goldschmidt, Beobachtungen über exemplarische subjektive optische Phänomene, *Zsch. f. Psychol.*, 76, 1916, 289-436.

[10] F. Rüsche, Über die Einordnung neuer Eindrücke in eine Vorhergegebene Gesamtvorstellung, *Zsch. f. Psychol.*, 10, 1900, 265-338.

[11] A. Kirschmann, Über die Erkennbarkeit geometrischer Figuren und Schriftzeichen im indirekten Sehen, *Arch. f. d. ges. Psychol.* 13, 1908, 352-388.

[12] J. Philippe, Sur les transformations de nos images mentales, *Rev. Philos.*, 43, 1897, 481-493.

Similar directions of change were later found by Wulf,[13] who presented to his *O*s a given visual form for 5-10 sec. (depending upon the complexity of the form) and determined the changes occurring in the sequence of reproductions of these drawings through a period of time. Wulf concludes that the changes produced are of two kinds, sharpening (Präzisierung) and levelling (Nivellierung). Sharpening is exaggeration or emphasis of a characteristic or peculiarity of the presented figure. Levelling is omission, toning down, or weakening of a characteristic. Wulf distinguishes three specific kinds of changes that may cause sharpening or levelling: (1) normalising (*Normalisierung*), *i.e.* a change in the direction of a familiar object, (2) emphasizing (*Pointierung*), *i.e.* exaggeration of a characteristic or peculiarity, and (3) structurally-conditioned change or direction of change (*Struktiv Veränderung*) which is determined independently of (1) or (2)—*e.g.* a form tends to be symmetrical even where a normalising apprehension works in the direction of asymmetry. *Gestalt* psychologists cite such experiments as Wulf's as furnishing further support of the law of *Prägnanz, i.e. Gestalten* tend to change in the direction of greatest simplicity and regularity, increasing the prominence of the essential features of the *Gestalt*.

In order to check the conclusions of Wulf, Gibson[14] used two series of fairly simple geometrical forms, one series made up of straight lines and one made up of curves. The first group of 14 figures was visually exposed for 1½ sec. each and the *O* reproduced what he could. Then there was a repetition of the exposure and another reproduction. In some cases a third exposure was given. With a second group of figures the exposure-time was 2 sec. The author named five tendencies which were apparent in his results. (1) Object-assimilation, "the term used to describe the phenomenon in which the perception of the figure involves visual or verbal imagery of some familiar object or shape, and where the reproduction which follows is clearly changed so that it more nearly resembles the familiar thing than does the stimulus-figure." (2) Verbal analysis, which "occurs when the stimulus form, instead of being associated with a single familiar object or shape, is analysed verbally in any of a variety of ways and when the reproduction is so changed that it is wholly or partially a product of the verbal analysis rather than a representation of the form itself." (3) Figure assimilation, which "in its simplest form may be defined as the phenomenon where one of the stimulus-figures is apprehended by the *O* as in some respect similar to a second stimulus-figure, and where the ensuing reproduction is clearly changed so as to resemble the second figure more closely than does the original." (4) Completion and disintegration, and (5) Rectilinearity. Gibson criticizes Wulf on the ground that the figures used by Wulf *could* change in only two directions and also because his own results seemed to show that it is the perceptual habits rather than the laws of configuration (*e.g.* Prägnanz) which condition the changes observed. "The changes observed in these experiments," says Gibson, "have not been interpreted as evidence for a single law determining the changes of configurations, but rather as evidence for the existence of perceptual habits which have arisen in the individual during experience. In general, the nature of a change found in the reproduction depends upon the manner in which the figure was apprehended."[15]

(2) *Peripheral conditions.* Since the present study involves the presentation of patterns in peripheral vision, it is relevant to consider those investigations which

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[13] F. Wulf, Beiträge zur Psychologie der Gestalt; VI. Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373.

[14] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

[15] *Ibid.*, 39.

deal with the effect upon perception of the special physiological and anatomical conditions of the peripheral retina and of the dioptric media.

Ames and Proctor undertook to determine the characteristics of the images of those objects upon which the eye is not focussed.[16] Their studies give a detailed account of dioptrics of the peripheral retina. The authors conclude that

> "the retinal picture . . . can be described in general terms as being a picture in which objects at the center of interest, or focus-point, are depicted in considerable detail but not with microscopic detail. . . . Objects lying to one side of the line of vision are . . . less clearly depicted, the lack of clearness increasing with the angle of obliquity, the accentuation of detail and edges in such objects depending upon their position relative to the center of interest. . . . All oblique objects are distorted and changed in shape varying with the amount of their obliquity. This distortion is shown in the bowing out in their central portions of straight lines which do not pass through the center of interest and a reduction in size of oblique objects."

The authors have carefully mapped this "barrel" distortion. The results of their studies need not here be considered in further detail since the mechanism of peripheral vision is not the primary concern of the present investigation.

Zigler reviews previous investigations upon perception in peripheral vision and proceeds to a comprehensive investigation of the subject.[17] Zigler's object was "to determine whether qualitative differences in the perception of form can be found at various places in the peripheral field of vision." He argues that previous studies of the nature of form in peripheral vision (Aubert, Kleitman and Blier, and Geissler) do not provide an adequate solution to this problem. Zigler's method was gradually to move a given form from the periphery to the fovea, instructing the *O* to report changes in perceptive experience. From his results he concludes that "four different fields of apprehension of form are demonstrated in the visual field in the four major quarters." In the outermost (figureless field) the background only is perceived; there is no figure. In the next field (formless figure) an unorganized mass of meaningless marks or lines is vaguely seen and the principal dimension of the figure reported. In the third one (form-like figure) there is an uncertain figure-like formation which suggests several names in succession. Mutilated figures and the alteration of details are not perceived. In the innermost region (clear figure) details are accurately perceived. Zigler believes that his results support the assumption that differences in form-perception are due to a retinal factor. He decides that "the reduction in density of retinal terminals in the periphery and the fact that an optic fiber in indirect vision supplies a number of terminals, whereas in the fovea each optic fiber supplies only one or at most two terminals, is probably a sufficient explanation of the difference existing between central and peripheral perceptual adequacy." Zigler grants that "central or cerebral factors play a part, since in the form-like region a tentative classification tends to produce alterations in the appearance of a vaguely perceived figure." But he concludes that "the prepotency of retinal function is indicated by the fact that the several forms tentatively assigned in this region are invariably reduced to the true form as soon as the figure is clearly seen."

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[16] A. J. Ames, C. A. Proctor, and B. Ames, Vision and the techniques of art, *Proc. Amer. Acad. Arts & Sci.*, 58, 1923, 36.

[17] M. J. Zigler, B. Cook, D. Miller, and L. Wemple, The perception of form in peripheral vision, this JOURNAL, 42, 1930, 246-259.

Galli has investigated the perception of forms in peripheral vision,[18] using the same method as that used by Zigler. *O* was shown, indirectly along four meridians, simple geometrical figures—letters, numerals, and "meaningless figures." Galli finds five phases in the perception of figures under his conditions: (1) a vague impression of an indefinite thing in the field of vision, (2) the substantiation of the presence of an indefinite object, (3) the appearance of rudimentary forms or figures, similar to known figures, (4) perception of form, and (5) designation.

In this method (Zigler, Galli, and others) of employing conventional forms, figures, and symbols, it is difficult, if not impossible, to estimate the effects of recognizing, instead of 'seeing,' the known forms. To touch off a verbal name or other epithet, it is only necessary that the stimulus-pattern should be sufficient to suggest 'that form.' Practice and repetition make *O* more and more expert in distinguishing the several members of the series (seen at times with full foveal adequacy). The experiment comes to be, therefore, more a process of coaching in the interpretation of indistinct signs than a discovery of retinal adequacy at various removes from the fovea.

(3) *Influence of functional exercise upon perception.* Gottschaldt found that certain simple 'configurations' were not recognized by his *O*s when they became geometrical parts of a larger configuration, which tended to mask or destroy them as simpler figures.[19] He found, too, that, even though the simpler figure had been previously presented to an *O* as many as 500 times and had become completely familiar, this masking effect of the total persisted. Despite the many repetitions of the smaller figure it was not recognized when 'embedded' in the larger. The simpler figure was not observed at all when the *O* was instructed merely to report everything seen, and it was observed only rarely when *O* was specifically instructed to look for it. This result has been regarded as destroying the theory that organization, present in phenomenological experience, can be explained by past experience. Gottschaldt contends that "hier hatte sich ergeben, dass die Wirkung solcher gehäuften Wiederholungen auf das Wahrnehmen von Figurationen an sich überhaupt äusserst gering, bei den dort verwendeten Graden von Gestaltbildung nahezu gleich Null zu setzen ist."[20]

Dickinson's problem was to obtain an "introspective description" of the temporal course of perceptive experience.[21] He briefly presented simple "meaningful forms" (arrangements of letters and playing cards). On the basis of reports of his *O*s, he concludes that "there appear to be three levels in the progress of . . . visual perception from its initial moment to its termination." These levels he designates as visual pattern, generic object, and specific object. By 'experience as visual pattern' Dickinson means "that experience here is a mere 'thereness,' clear in contour and in detail of illumination, but lacking in logical meaning." By 'experience as generic object' he means that these forms have taken on generic meaning. By 'experience as specific object' he means that "a definite label has accrued and the forms have taken on a stability which previously they lacked." This study of Dickinson's is concerned, not with changes taking place in the perceptive function as a result of repeated exercise, but rather with the temporal course in a given perceptive performance.

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[18] A. Galli, La percezione della forma nella visione periferica, *Pubbl. Univ. Cattol. d. S. Cuore,* 6, 1931, 1-27.

[19] K. Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, *Psychol. Forsch.,* 8, 1926, 261-318; 12, 1929, 1-37.

[20] *Ibid.,* 12, 1929, 2.

[21] C. A. Dickinson, Experience and visual perception, this JOURNAL, 37, 1926, 330-344.

Dickinson seems not to have been aware of a similar, but more analytical, description of perceptive levels or stages carried through several years before by Rogers.[22] Rogers distinguishes figurational ('nonsense' objects), depictive (specific objects), abstractive (presentation of a class), and symbolic perceptions.

A still earlier experiment upon progressive modifications in perception was that of Judd and Cowling.[23] These authors exposed for 10 sec. a figure made up of joined straight and curved lines. *O* was required to draw the perceived figure. After another exposure of 10 sec., *O* made another drawing. This continued for ten or more times with the same figure. The drawings were made under various conditions, with closed eyes, with seen movements of the hand, with both movements and resulting drawing seen. The authors conclude that "the recognition of the figure is gradual, some *O*s beginning at the beginning of the figure and working it out in detail, others beginning at other points in the figure; that the greatest amount of error is in the middle of the figure; that there is a difference in the rate of mastery of the size and relative position of the lines; and that in certain cases parts of the figure may improve for a time and the same parts may later be partially or completely overlooked." The emphasis in the experiment is upon improvements in accuracy, a matter obviously different from the nature of perception itself.

Hempstead investigated the perception of gray forms slightly different in brightness from their backgrounds.[24] The figure was exposed for 5 sec., and the *O*s then drew and described it. The figure was exposed again, with an increase in the brightness between the figure and the ground, and another drawing was made. There were 5-30 changes in brightness and 5-30 drawings made for each of 142 figures. Hempstead concluded that figures under these conditions develop according to two principles, symmetry and similarity, and that "each observer has certain habits of illusion, or certain typical modes of associative completion, which persist with certain modifications throughout his record."

Wohlfahrt studied the genesis of *Gestalten* in the "developed consciousness."[25] Wohlfahrt's experiments and those of Sander (which follow) indicate the characteristics of the psychological functions which are emphasized by the Leipzig laboratory, *i.e.* the nature of the original totalities, the feeling characteristics of these totalities, and the predispositional (*strukturelle*) factors in the operation and development of the psychological functions. To study the process by means of which *Gestalten* tend to be formed, Wohlfahrt presented, in a lightless room, small luminous line-figures and magnified them in successive stages until they were finally experienced as end-

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[22] A. S. Rogers, An analytic study of visual perceptions, this JOURNAL, 28, 1917, 550-558, esp. 576.

[23] C. H. Judd and D. J. Cowling, Studies in perceptual development, *Psychol. Monog.*, 8, 1907, (no. 34), 349-369.

[24] L. Hempstead, The perception of visual form, this JOURNAL, 12, 1900, 185-192.

[25] E. Wohlfahrt, Der Auffassungsvorgang zur kleinsten Gestalt, ein Beitrag zur Psychologie des Vorgestalterlebnisses, *Neue Psychol. Stud.*, 4, 1927, (cited by Sander, *op. cit.*, 57).

Gestalten. In the original stage the figures were found to be of a total, unitary character, undifferentiated and difficult to name. Then there occurred a circular contouring that continually shifted and changed about. The contours in this stage began to differentiate, but were always labile. The enclosed parts remained unarticulated for a long time and were characterized by a complex 'feeling-like' quality. Finally, they began to differentiate into regular forms. New stages were reached suddenly and the more and more detailed articulated parts grew out of the previous simpler totals. In this *Vorgestalt*-period the figures were enormously labile and were "filled with internal life and movement." According to Wohlfahrt's description, this lability increased in places to a pulling, pushing, or tearing, usually just before a new member broke off from the regular scheme. Only at the end-stage, with the preponderance of the stimulus-conditions, did the lability go over into a certain definiteness and quietness. Wohlfahrt concludes that development proceeds, first, by way of an increasing differentiation and articulation of details from the original unitary total, secondly, in a transition to stability from the originally labile and unstable state, and, thirdly, in a transition from figures with a richness of feeling-quality to those showing to the experiencing subject a greater coldness and objectiveness. Wohlfahrt, himself, does not separate the directions of variation in this three-fold way. He discusses them all as parts of the same process. But it is uncertain whether the fusing and identification of these modes of change are necessary.

With respect to the developmental changes in perception and upon the basis of Wohlfahrt's experiments and his own unpublished investigations, Sander makes the following statement. "Die Untersuchungen zur Aktualgenese von Gestalten sind in doppelter Hinsicht von Bedeutung: sie zeigen einmal, dass wie in der Phylo- und Ontogenese so auch hier psychische Ganze mit dem Merkmal der Gestaltheit sich entwickeln aus relative ungegliederten, in das Gesamterleben eingeschmolzenen Ganzen von gefühlsartigen Qualitäten. Diese gefühlsartigen Qualitäten sind nicht blosse Begleiterscheinungen des Gestaltprozesses, sondern funktional wesentlich als Werdensgrund. Ohne das zunächst gefühlsartige Erleben ganzheitlicher Zusammenhänge ist auch in höhern Schichten keine produktive Gestaltbildung möglich."[26] Sander says further that the dispositional structures of the organism tend in the direction of closedness and unitariness toward simplicity and regularity, and toward symmetry and "articulation in the preferred space-directions."

With the exception of the method of Judd and Cowling, the technique used for the purpose of obtaining progressive modifications in perception has resulted in progressive changes which may be a function of changes in the stimulating conditions. The results cannot therefore be accepted as giving valid information upon the course of the perceptive function in the adult organism. While the literature has raised certain problems upon which the present results may bear, it has furnished but little reliable information upon the specific problem here investigated.

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[26] Sander, *op. cit.*, 59.

THE EXPERIMENTS

A series of 12 line-patterns was presented at 4 places in the periphery of the retina, one temporal (T) and one superior (S) position at 10° from the fovea, and one nasal (N) and one inferior (I) position at 22° from the fovea. Individual measurements were made in order to avoid the blind-spot. Forty trials with each of two *O*s were given at each of these four positions. In sixty supplementary trials with the same *O*s, and in sixty and seventy-five trials respectively with two additional *O*s, only two retinal positions, T 10° and 20°, were employed. Each of the 12 patterns in the series was presented at each of the explored retinal positions.

No pattern was presented twice in immediate succession. The sequence of presentation of the various pattern-groups was haphazard. When all four retinal positions were employed, the first three patterns (Patt. 1-3) were presented (say) at T 10°, the next three (Patt. 4-6), at S 10°, the third three (Patt. 7-9) at N 22°, and the last three (Patt. 10-12) at I 22°. In the next presentation of the series, Patt. 1-3 might be given at S 10°, Patt. 4-6 at N 22°, Patt. 7-9 at I 22°, Patt. 10-12 at T 10°, and so on. When only two retinal positions were employed, Patt. 1-6 were presented (say) at T 10° and Patt. 7-12 at T 20°. At the next presentation of the series, Patt. 7-12 were presented at T 10° and Patt. 1-6 at T 20°, and so on. A haphazard order also within the pattern-groups was always maintained.

The 12 patterns used (Fig. 1) were chance linear arrangements obtained by drawing the configurations which resulted in dropping from a height upon a plane surface three wires of unlike lengths. The selection of the twelve patterns to be used was made upon a number of variables of form and arrangement detected by preliminary foveal observations with trial *O*s who were not used in the main experiments.

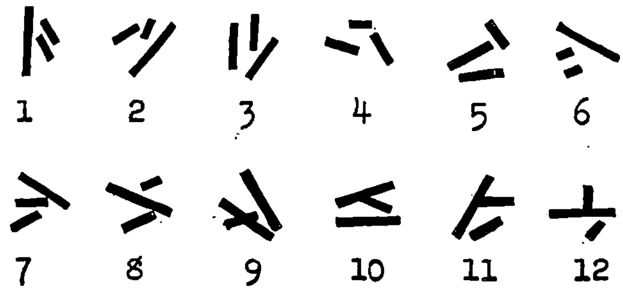

FIG. 1. STIMULUS-PATTERNS

The apparatus consisted of the large Hellpach perimeter with its arm held stationary in a horizontal position and modified by the addition of a stationary vertical arm.[27] On the vertical and horizontal arms were placed four light-tight metal boxes each containing a 5-watt lamp with rheostat-control. The distance from the stimulus-object to O's eye was 1.09 meters. In front of a circular opening 1½-in. in diam. in each light-tight box, glass slides were placed. The slides were of colorless ground-glass and the patterns, cut from black gummed-paper, were pasted upon the slides. Over the patterns on the glass slide was pasted translucent white paper which permitted the pattern to be seen only by transmitted light. The lights in the boxes were controlled by a fall-apparatus with an exposure-time of 100 sigma ±1.3. The visual field, with the exception of the 1½-in. circular openings, was of neutral gray shade-cloth, 6 ft. high and following as a cylindrical surface the curved horizontal arm of the perimeter. The general illumination, provided by 4 field-lights, was weak daylight. The intensity of the general background, as measured by the Holophane light-meter, was 0.3 millilamberts and that of the stimulus-ground, 39.0 millilamberts. Weak daylight illumination was used to avoid shifts of fixation which easily result from the sudden appearance of a light upon a lightless ground, and also to avoid large changes of retinal adaptation. A small mirror, suspended by threads from the ceiling and floor of the room at half the focal distance, served as a fixation-point.[28] Biting-boards were used to aid fixation. The left eye of *O* was covered by an optician's patch. Where shifts in fixation were reported, in spite of our precautions, the pattern in use was discarded and reintroduced later in the series.

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[27] W. Hellpach, *Philos. Stud.*, 15, 1898, 525 ff. Cf. W. Wundt, *Grundzüge der physiologischen Psychologie,* 2, (6th ed.), 1910, 189 f.

After *O* was seated in place and before the linear pattern was exposed, the following instructions were read.[29]

Draw and give full commentaries upon what you see. Avoid sets and consider each presentation upon its own merit without reference to any other."

O then fixated his own pupil in the mirror,[30] using the biting-board to keep his head in a steady position. After the signal "Fixate, ready" a trip on the pendulum of the control-apparatus was released and the pattern was illuminated. *O* was asked to indicate on each drawing of the figure as seen the degree of certainty as to the reliability of his drawing and commentary.

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[28] Described by K. M. Dallenbach, this JOURNAL, 42, 1930, 116.

[29] The observers were members of the staff of the Department of Psychology at Cornell University, C. W . Fox (*F*), A. D. Glanville (*G*), J. McV. Hunt (*H*), and Dr. R. B. MacLeod (*M*).

[30] The eyes of the *O*s were tested for errors of refraction and the following reports were received from the oculist, Dr. Albert C. Durand, Ithaca, New York.
M: Vision without drops: R = 20/50+; L = less than 20/150
with drops: R = 20/70 (lens: +sph. 0.50, —cyl. 1.75, ax. 169 =
(cycloplegia) 20/15) ½° esophoria
L = less than 20/150 (lens: +sph. 1.25, —cyl. 5.00, ax. 25 = 20/40) 1° left hyperphoria
Media clear; fundi negative
G: Vision without drops: R = 20/15?; L = 20/20
with drops: R = 20/20? (lens: +cyl. 0.25, ax. 75 = 20/15) 1½° esophoria
L = 20/20??. (lens: —cyl. 0.50, ax. 120 = 20/15) ½° right hyperphoria
Media clear; fundi negative. No lenses ordered and none used in present experiment.
F: Vision without drops: R = 20/30+; L = 20/30
with drops: R = 20/30+ (lens: —sph. 0.25, —cyl. 0.37, ax. 100 = 20/15) 1° exophoria
L = 20/30 (lens: —sph. 0.25, —cyl. 0.75, ax. 75 = 20/15)
Media clear except for remains of pupillary membrane across right pupil; fundi negative. Lenses ordered but not used in the present experiment.

RESULTS

The terms descriptive of the apprehended object employed by the *O*s may be classified under three headings; viz., characterization (1) of the linear parts or members of the figures as perceived (referred to below as *P*), (2) of the relations (R) of the linear parts to each other, and (3) of the total (T) figure as seen. More precisely stated, the characterizations are as follows:

- (P) Apprehended linear parts
  - (a) number
  - (b) direction
  - (c) length
  - (d) width
- (R) Apprehended relation of parts
  - (a) to each other
  - (b) dominance of any member
- (T) Apprehended total figures
  - (a) in terms of figure
    - (i) integrity or unity of figure
    - (ii) general direction
  - (b) reference beyond the figure
    - (i) affective tone
    - (ii) dynamic character
    - (iii) familiarity

Progressive changes in perception. Those authors who have discussed the possible directions of development in behavior and in psychological functions have suggested that these directions can be completely summarized by allowing for two types of change, (1) the increasing differentiation of an originally unitary total and (2) the integration of originally independent parts. An examination of the temporal sequences of our *O*s' drawings and reports shows, however, that neither of these specific directions of modification was found in the present experiments. The changes that did occur were mainly modifications in the perception from a labile, plastic, unstable beginning to a condition of stability.

In our results, the evidence for progressive change is of two kinds. The most striking evidence is provided by the sequence of drawings. The first drawings made by a given *O* with a given stimulus-pattern differ considerably from each other. But in the later drawings a stage is finally reached in which the drawings assume and retain a certain fixed form. Perception has become stabilized.

It appears from the drawings and the verbal reports that this final stabilization in the apprehended pattern has occurred for *G*, *F*, and *H*. For *G*,

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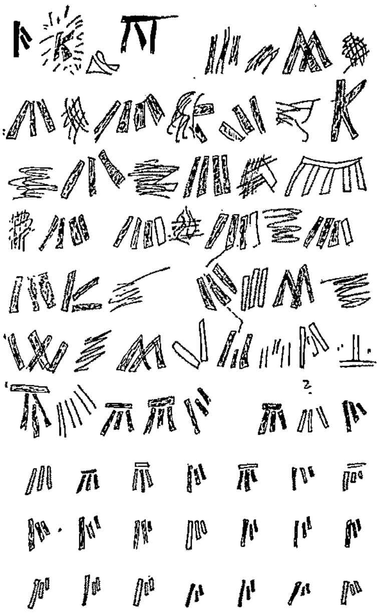
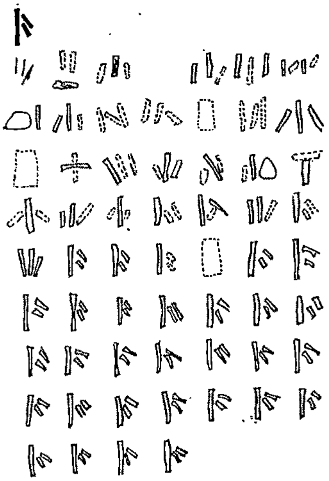
H: Vision without drops: R = 20/30; L = 20/20
Further tests not made

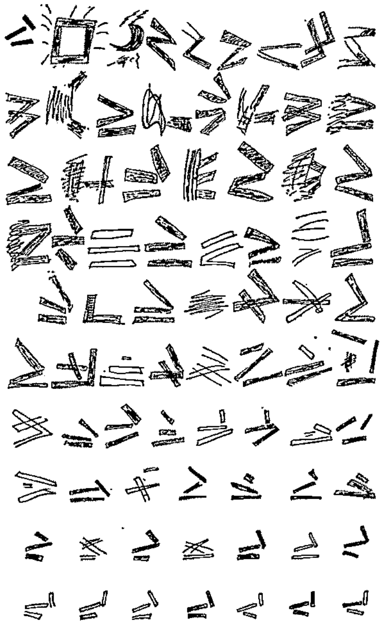
Our study has profited from the interest and the skill of an exceedingly competent ophthalmologist and oculist, who has appreciated the special difficulties in determining conditions in extra-foveal vision.



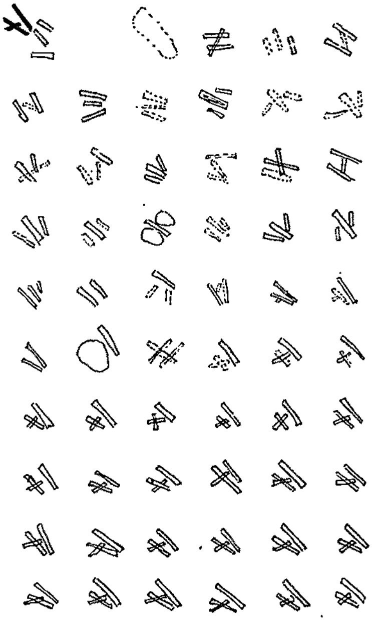
2 Observer H Pattern 1



3 Observer G Pattern 1



4 Observer H Pattern 5



5 Observer G Pattern 9

FIGS. 2–5. OBJECTS AS DRAWN BY *G* AND *H*

Patt. 1 retained its final form after the 28th presentation at 10° and after the 35th presentation at 20°; Patt. 5 the 34th (10°) and the 39th (20°); Patt. 9 the 39th (10°) and the 34th (20°). For *F*, Patt. 1 retained its final form after the 71st presentation (10°); Patt. 5 at 72nd (10°); and Patt. 9 at the 68th (10°). Very little was seen by *F* at the 20° and 22° positions. For *H*, Patt. 1 stabilized at the 63rd presentation (10°) and at the 64th (20°); Patt. 9 at the 61st (10°) and 63rd (20°); and Patt. 5 at the 65th (10°) and 68th (20°).

Typical progressive changes in apprehension are portrayed in Figs. 2-5, which contains the entire series of drawings made by *G* and *H* from the presentation of line-patterns No. 1, by *H* of No. 5, and by *G* of No. 9. In the upper left-hand corner of each of the four series of drawings stands the stimulus-pattern presented for 1/10 sec. at some peripheral part of the retina as described above. The successive drawings are to be read in lines from left to right beginning at the top.[31]

In Fig. 2, it is obvious that *H* first apprehended highly variable objects. In many of the objects occurred either three or four lines in arrangements suggestive of the line-pattern given, but with no fixed order. There is a tendency for the lines to converge at one end and to diverge at the other. Various spatial orientations occur. Twice *H* stylizes the object as 'K.' Several times the object is M-like. It is only near the end of the sixth horizontal line of drawings that *H* gives the essence of the line-pattern; a vertical accompanied by two down-right diverging lines. This then slips away, but returns, and is repeated again and again with slight variants. Here stabilization occurs. It is to be noted that the stabilized form is never wholly faithful to the light pattern presented to the eye.

In Fig. 3, *G* never draws the wide variety of objects that H draws. Nevertheless, it is obvious that the retinal stimulus-pattern is for a long time inadequate to a perception faithful to the pattern. Sometimes there is a complete blank. Sometimes there is great uncertainty (light broken lines in the drawings). But presently appears something-vertical-and-something-leading-off-downward. This something soon stabilizes and we have a succession of drawings which never 'copy' the line-pattern but always thereafter indicate its essential features.

Fig. 4 shows a progression very similar to that of Fig. 2. The organism's resources for perceiving are wholly inadequate but *H* struggles valiantly, soon hinting at the characteristics of the line-pattern. Again it is obvious

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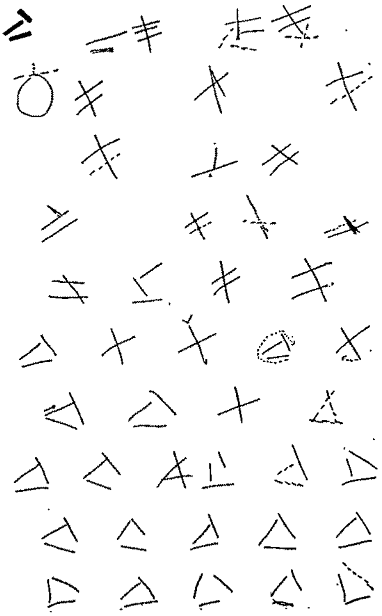
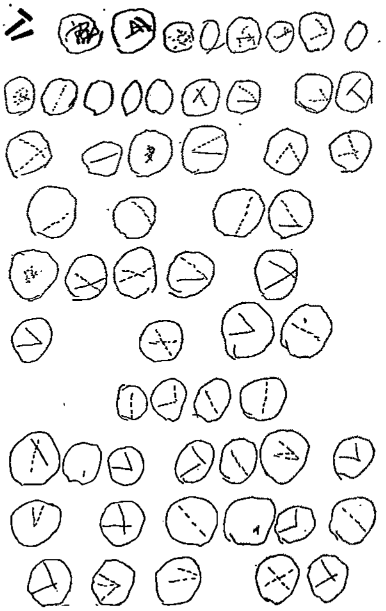
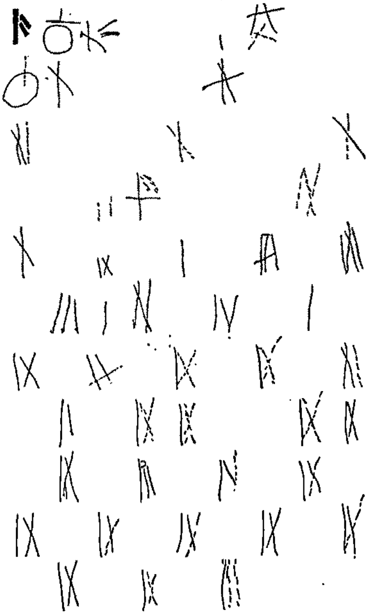
[31] The adequacy of each *O* in delineating an apprehended object by a linear drawing was determined by the reproduction of foveally-seen objects requiring the same kind and degree of skill in representation.



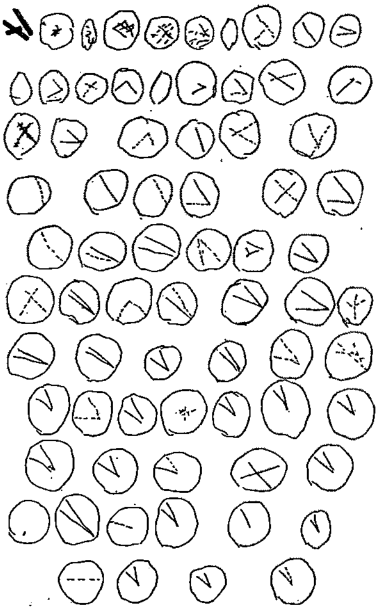
6 Observer F Pattern 5



7 Observer M Pattern 5



8 Observer F Pattern 1



9 Observer M Pattern 9

FIGS. 6–9. OBJECTS AS DRAWN BY *F* AND *M* that the general orientation of the object is lacking and that angles, distances, and juxtapositions are loose and shifting. Gradually the details fall into their approximate places; but a detail is commonly found, only to be lost and later rediscovered. In the 7th and 8th horizontal lines the function is showing decided progress. The 9th line is almost stable, and the 10th approaches, with but minor variants, the 'final' object.

In Fig. 5, *G* runs true to his earlier performance (cf. Fig. 3), but with a new line-pattern, No. 9. The task is clearly of about the same degree of difficulty as before, and stabilization appears at about the same place. A close comparison of Figs. 3 and 5 will make it evident that in spite of *G*'s vague groping and high uncertainty throughout the twenty or more early trials, the two stimulus-patterns, Nos. 1 and 9, are exerting an unlike influence upon the organism, directing it toward unlike apprehensions. When it is remembered that these patterns are presented day after day and week after week on various peripheral areas and in a chance order for the entire set of 12 patterns (never in immediate succession), it is striking that the individual *O* should exhibit such a high constancy in the direction and the rate of his perceptive accomplishment.

Upon an alleged explication of an original totality, which might be expected gradually to rise from its ground and to acquire specificity, we have little affirmative evidence to offer in our drawings. There is here not much to comfort the *Gestalttheoretiker*. Our commentative reports do not offer much more. Verbal delineation in terms of parts, relations, and totalities (see above) present virtually the same ratios from the first observation of a given stimulus-pattern to the 60th, 70th, or 100th. Out of several thousand descriptive phrases, recorded along with the drawings, we can find no evidence that progress is, under the terms of our experiment, either a march from a vague totality toward a differentiated multiplicity or a march from unrelated dismembership to an integral unity.

Scarcely less instructive than our results with *G* and *H* are those with *F* and *M*. *F* failed to base any perceiving operation upon stimulus-patterns impressed at an eccentricity of 20° from the fovea. At the 10-degree positions he came to fairly stable forms, but with many failures and with 'final' forms which can scarcely be said to represent the delineations of the stimulus-patterns. Figs. 6 and 8 reproduce *F*'s drawings for patterns No. 5 and No. 1. In Fig. 6, a loose triangularity appears, and in Fig. 8 something like a "IX." The wide disparity between retinal input and psychological product may be due in part to dioptric defects. The art of dioptrics finds a great difficulty in defining the exact relations between the impinging light-pattern and the resulting stimulus-pattern where the light falls obliquely upon the eccentric retina. Foveal vision is much more easily and correctly defined by oculist and optometrist. There may have been unusual dioptric distortions of the pattern in the eyes of *F*. Whatever the inadequacy of these primary conditions of perceiving may have been, however, the fact of stabilization in this *O* is clarified by his comment that he had a fixed expectation (self instruction) of some familiar form, and that when such a form appeared (*e.g.* the triangle or the IX above) he regarded the perceived object as final and so continued to draw minor variants of this form. The common illusory distortions of perceived objects under expectation or disturbing emotion are well authenticated even where the conditions of perception are much more nearly adequate than in the cases cited.

The perceptive functions of *M* also call for special remark. An inspection of Figs. 7 and 9 will make this clear. Finding it exceedingly difficult to 'see' anything in the circular lighted area, *M* reported many blanks and often simply outlined the circle and inserted vague lines or dots within it. No certain progress toward such forms as vaguely represented for *F* the stimulus-patterns now appear. The unlike inclinations of patterns No. 5 and No. 9 (as here presented) are scarcely to be recognized in *M*'s drawings. It is probable that ocular imperfections again play a part. We must note, however, a fixed comment of M. Conviction that his perceptive resources were not up to the task early led him toward discouragement and to a self-instruction of anticipated failure to perceive clearly and adequately. Here the vague and formal drawings, which tend to become stereotyped, probably represent rather an honest attempt to perform a task than the natural progress of a psychological function under the guidance of stimulus. The results of *F* and *M* suggest, then, the deflective effect of a dominating self instruction. Wanting the report of this diverting cause, the results would have to be referred (as they usually are in such cases) to the broad category of "individual differences."

In our historical section, researches were cited which had found either 'affectivity' (Wohlfahrt, Sander) or 'familiarity' (Philippe) to be a characteristic mark of perceiving. We therefore were at pains to control these factors. If either of them played a part, we should expect these 'feelings' to stand related to our process of stabilization. Such things as 'disturbing,' 'pleasing,' and 'displeasing' appeared in the reports of only one of our *O*s (*G*), and here almost without exception before the stage of stabilization appeared. They are obviously related to the unsatisfactoriness of the apprehended object under the demands of a self-appointed task or *Aufgabe* and not to the progress toward stabilization. Nothing like the 'internal life and movement' of Wohlfahrt's descriptions came out of our observations. These alleged 'dynamic' qualities may be a function of the materials used or (as seems to us more likely) of self and occasional instructions. The leveling, emphasizing, assimilating, and changing-toward-type of Wulf, Gibson, and Philippe may occur in repeated perceptions of certain conventional designs and objects, under certain instructional tasks, but we found no trace of such changes in our simple apprehension of unknown patterns. That curious phenomenon called 'familiarity' was often reported by *G* and *H;* but it came both in the early stages and in the later, *i.e.* both before and after stabilization had been attained. The mark of familiarity attaching to the object perceived stood, therefore, in no specific relation to the attainment of the stable stage of apprehension.

With the Zigler type of conventional object, quite a different result might well be expected. Our non-conventional and 'unknown' patterns, on the other hand, lent little incentive to look for, or to recognize, familiar figures, which might then have become 'stabilized' and invariable.

SUMMARY

The study has attempted to determine and to record successive changes of a functional sort in perceiving. In order to produce these changes in the simplest way, unknown and unconventional collections of three short lines of varying lengths and arrangements were used as a stimulus-pattern upon the peripheral retina. Under short exposure (1/10 sec.) perception was at first vague, indeterminate, and uncertain. With many repetitions, however, the perception suffered notable changes.[32] The initial stage was followed by hints in the perceptive product of the line-members, spatial orientation, and internal relations of the stimulus-pattern. After gains, shifts, losses, and regains in these particular moments, the processes and the products of perception became stabilized and certain. Whether the stable object represented the stimulus-pattern or sharply diverged from it, it was maintained with slight variations until the final exposure of the appropriate light-pattern. Since this progression appeared for each stimulus-pattern, although 12 unlike patterns were carried through together in a chance order which never permitted the immediate repetition of any single light-pattern, it seems likely that the orderly changes described represent fundamental stages in the growth or development of a fairly simple function in the human adult under simple and constant receptorial conditions and with carefully controlled instructions. The results supplement other studies of perception which have employed known and conventional object-stimuli and which have therefore dealt with more complex and more variable conditions both within and without the organism. Our results offered no support to either of those alternative theories which regard perceptive apprehension as progressing from (1) an initial undifferentiated totality to a diverse multiplicity or (2) from an initial inchoate multiplicity to a well-integrated configuration. In our experiments, the main progress was from a labile, uncertain, and inconclusive function to a fixed, stable, and conclusive performance whose end-product was a determinate and abiding object.

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[32] After describing the temporal changes which occur in habituation, Bentley insists that, "while we think most naturally of the consolidating changes which we have just now discussed as referring to action, and especially to those action-trains which give us skill, it is important to observe that every one of the psychological functions passes through, under its own exercise, such modifications as we have described. No mode continues just as it began. And this fact is the beginning of those progressive changes in the human being which we call, when taken change-by-change, learning, and, when taken all together, psychological development." *The New Field of Psychology: Part i, The Psychological Functions,* 1933, 64.

A RECIPROCAL REFLEX SYSTEM IN THE ACCOMMODATION-CONVERGENCE RELATIONSHIPS

By EDWIN FORBES TAIT, University of Pennsylvania

For many years there has been considerable speculation among workers in psychological and physiological optics regarding the relationship between the functions of accommodation and convergence in preserving clear single binocular vision. Helmholtz advanced the theory that these functions normally go hand in hand and that they are, in their mutual relationship, the manifestations of a built-up association not belonging to the class of congenital involuntary concomitant movements.[1]

Interest in the problem was lacking for many years and it remained without experimental investigation until the general development of visual psychology and physiology brought it to the attention of Sheard,[2] who presented, in 1917, the problem in a concise and systematic manner for the first time. Sheard recognized the importance of the psychological aspects of the problem and began his work by attempting to reduce the judgmental errors inherent in the older purely subjective methods by the development of a more objective system for the determination of the accommodation and convergence values. The methods then developed are the basis of those used in the present investigation. So far, however, it has been impossible to utilize wholly objective methods and thus the present investigation assumes a considerably more psychological than physiological character.

A fairly complete citation of the meager literature up to 1920 will be found in the paper by Sheard.[3] As a result of the work reviewed, Sheard comes to the conclusion that 70% of persons possessing low refractive errors, but with otherwise normal eyes, show from 2–7 prism diopters of accommodative exophoria at a fixation-distance of one-third meter, and he further concludes that the optimum condition is one where there is from 4–5 prism diopters at that distance.

When binocular vision at near is required, as in reading, it is evident that the visual axes must converge toward the point of fixation. The amount of accommodative exophoria represents the amount of that convergence which is supplied by the fusion faculty in the interest of single binocular vision, but as this usually represents only about one-fourth of the whole, the major part of the convergence

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[1] H. von Helmholtz, *Handbuch der Physiologischen Optik*, 3rd. ed., 3, 1910, 48, 51.

[2] Charles Sheard, *Dynamic Ocular Tests*, 1917.

[3] Sheard, On accommodative exophoria, *Amer. J. Physiol. Opt.*, 1, 1920, 234.

present in normal cases must therefore be due to the convergence innervation stimulated by the use of the accommodation and designated in this paper as accommodative-convergence.

Weymouth, Brust, and Gobar,[4] in 1925, support Sheard's conclusions and add somewhat to the historical background.

Ames and Gliddon,[5] using an adaptation of the Pereles and Hess apparatus, as described by Howe,[6] have shown that the actual accommodation used in reading rarely corresponds exactly to the fixation-distance, due to the ability of the visual perceptive centers to resolve retinal images even when they are formed by multiple diffusion circles. For a fixation-distance of one-third meter, however, they found the accommodative lack to be only from about 0.08 to 0.25 diopters. Values of this order, important as they are in a survey of accommodative phenomena, may safely be ignored in this investigation as they are considerably less than the expected experimental error. For all practical purposes, therefore, the amount of accommodation required for a fixation-distance of one-third meter may be considered as three diopters.

All the preceding investigations, however, are concerned either with the characteristics of accommodation or with the effect produced upon the convergence by the exercise of accommodation or accommodative effort. The possibility of a comparable or reciprocal reflex wherein accommodation may be the result of convergence or convergent effort has not heretofore been considered.

The object of this investigation is to establish the presence or absence of reflex accommodation due to a convergence stimulus, and if it apparently is present, to ascertain its values and characteristics as far as possible. It is obvious that the only approach is from a combined objective-subjective standpoint, obtaining, independently, the convergence value subjectively and the accommodative value objectively.

If, by suitable methods, the accommodation resulting from the convergent impulse, if any such be obtained, be measured and compared with the amount of convergence present when the subject is accommodating, with the fusion faculty non-operative, any positive correlation between the two would support Helmholtz's theory of a true association relationship, built up as the result of simultaneous use of accommodation and convergence in the early years of life.

No objective procedure would be sufficiently valid in the measurement of convergence due to the fact that the point upon the cornea through which the visual line passes and upon which an objective determination must depend cannot at present be determined accurately. The amount of accommodation, however, can best be determined objectively as skiametry affords the most accurate means with which to calculate the total refraction at any given distance.

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[4] F. W. Weymouth, P. R. Brust, and F. H. Gobar, Ocular muscle balance at the reading distance and certain related factors, *ibid.*, 6, 1925, 184-205.

[5] A. Ames and G. Gliddon, Ocular measurements, *Presessional Vol. Sec. Ophth., Amer. Med. Assn.*, 1928, 11-47.

[6] Lucien Howe, *The Muscles of the Eyes,* 1907, 329-330.

Skiametry, as used for many years by refractionists, is carried on by the use of a skiascope or retinoscope, which, in its simplest form, is an adaptation of the ophthalmoscope and consists essentially of a small round mirror with a sight hole in its center. When the light from a suitable source of illumination is reflected into the eye of the subject, the observer, peering through the aperture in the mirror, observes the illumination in the subject's pupil and notes that if he tilts the mirror slightly the pupillary illumination apparently shifts in a direction either coinciding with, or opposite to, the movement of the incident light. Meanwhile the attention of the subject is directed to a suitable fixation-object, which may be at any distance, and lenses are placed in front of his eyes so that the reflex movements become extremely rapid and eventually are completely neutralized. This point of neutralization occurs when the subject's retina, which is here acting as a secondary source of illumination, is conjugate with the principal nodal point of the observer's eye. Knowing the distance between the skiascope and the subject's cornea, the lense value in diopters can easily be calculated.

Most modern skiascopes have a self-contained illuminating mechanism, but are used in precisely the same manner as the simpler type. Dynamic skiametry is skiametry carried on while the subject is converging to, and fixing upon, a near object. This modified system, first described by Cross,[7] and developed further by Sheard,[8] and considered by the latter as the most accurate existent method of investigating the negative relative accommodation, presented little difficulty in application.

With an emmetropic, or normal eyed, individual or one made so by the proper corrective glasses, there must be approximately 3 diopters of accommodation exerted in order to see small print distinctly at a distance of 1/3 m. If fixation is constant but the accommodation varied by the introduction of convex lenses, there will be some relaxation of accommodation in most cases, but rarely will the individual, if under the presbyopic age, accept the full 3 diopters of convex lens power which would relax the accommodation completely. Usually there will be, as shown by clinical experience, a greater or lesser amount of accommodation retained, apparently due to the presence of convergence for, when the latter is relieved by the application of prisms base-in, the full convex value of 3 diopters is usually promptly accepted.

In an attempt to determine the accommodative status subjectively, however, acceptable judgments as to the point of beginning blur are difficult to obtain, probably due to the ability of the visual centers to resolve blurred retinal images. In the experience of the author, objective determinations of the refractive, and therefore accommodative, status may be carried on by the aid of dynamic skiametry with a possibility of error not exceeding ½ diopter. In addition this procedure permits the use of sufficient initial convex lense power to promote ciliary muscle relaxation to the point of the convergent-accommodative innervation. The difference between the spherical lense value found by dynamic skiametry and that accepted subjectively by the subject would, when subtracted from the 3 diopters necessary for focussing at 1/3 m., be equal to the amount of accommodation stimulated by the convergence to, and fixation upon, the near-point fixation-object.

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[7] A. J. Cross, *Dynamic Skiametry in Theory and Practise,* 1911.

[8] Charles Sheard, *Dynamic Skiametry,* 1920.

It is possible to measure the amount of accommodative convergence used by an individual under given conditions of near-point fixation by dissociating the fusion faculty by the application of a prism with the base down in front of one eye and then bringing into vertical coincidence by measured prisms the diplopic images produced by such a procedure. The base-in prism value necessary for coincidence represents the accommodative exophoria provided that the distance ocular muscle imbalance, if existent, has been previously corrected. The total convergence of the visual axes from parallelism to fixation at 1/3 m. may then be calculated in accordance with the formula developed by Weymouth, Brust, and Gobar.[9] The accommodative convergence may then be calculated by subtracting the accommodative exophoria from the total convergence as found by the above formula. This method of calculation, developed by the author, avoids the exceedingly laborious graphic method suggested by Maddox and quoted and amplified by Sheard.[10]

Subjects. The results of a quantitative study of 500 individuals, by methods developed in the study of the foregoing considerations, are here presented. All of the *S*s were between the ages of 10–40 yr.—not less than 10 yr. in order to insure reasonable accuracy in the subjective determinations, and not over 40 yr. due to the desirability of avoiding the lessened accommodative amplitudes which usually become apparent after that age. All had, also, single binocular vision, visual acuity of 6/6 with the proper correcting lenses, and, in cases where visual defects were present, wore the proper correcting lenses. An arbitrary division into 4 age-groups was made in order to afford, as far as possible, a basis of comparison between relatively older and relatively younger groups. The majority of the *S*s were college and high school students, but, as will be seen from the tabulations, a sufficient number of older persons was included in order that the results might not represent the conditions prevailing among the younger individuals only. The *S*s were selected from among a considerable number of persons who had reported to the author for routine examinations for visual efficiency after having previously been examined for visual defects.

All the *S*s were entirely uninformed as far as the problem was concerned, considering the procedure simply as part of the routine procedure for the determination of visual efficiency. The results have been considered collectively so that there was no need to identify the *S*s in any way except by numbering them successively from 1 to 500.

In selecting *S*s, care was taken to eliminate from consideration the few cases in which there was a manifest condition of eye-strain due to uncorrected ametropia, muscular anomalies such as convergent squint or hyperphoria, and where the astigmatic correction was not consistent or varied during the investigative procedure.

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[9] F. W. Weymouth, P. R. Brust, and F. H. Gobar, Ocular muscle balance at the reading distance and certain related factors. *Amer. J. Physiol. Opt.*, 6, 1925, 189.

[10] Sheard, *Dynamic Ocular Tests*, 1917, 60.

Apparatus. The apparatus consisted of a standard DeZeng phorometer, equipped with a near point target; an ordinary ophthalmic trial case; a test cabinet for visual acuity judgments and fixation; a self-luminous dynamic skiascope, and a 10-cm. rule graduated in mm. The phorometer was supported by a suitable floorstand and contained, before each eye, 3 cells for the reception of the trial case lenses and a rotary prism calibrated to read from 0–30 prism diopters. The rotary prism is attached in such a manner as to make possible the placing of the prism base-apex line in any given direction. In addition there is a white near-point card, measuring 13 cm. square and with a 4 mm. black E printed in its center, held upon a rod at a distance of 1/3 m.

The ophthalmic trial case used for the phorometer cells is composed of convex and concave cylindrical lenses, and is so arranged that any given lense power may be obtained by one or a combination of two or more lenses. It is calibrated in the familiar lense dioptric system.

The test cabinet used was the 20th Century Cabinet produced, like all the other items of apparatus, by the American Optical Company. Used at a distance of 6 m., for which it is calibrated, its fixation-letters are calculated to subtend a 5′ angle at their designated distances. The increasing size of the letters gives successively 6/6, 6/7.5, 6/10, 6/12, 6/15, 6/20, 6/30, and 6/60, in terms of visual acuity. The numerator represents the fixation-distance while the denominator expresses the smallest letters seen clearly at that distance. Thus 6/12 visual acuity means that *S* cannot see clearly letters smaller than the 12 m. group at a distance of 6 m.

The last of the series, the 6/60 object, consists of the one letter, E, and was the stimulus used for fixation in determining the presence and amount of esophoria or exophoria for distance. The smaller letters were used for the judgment of visual acuity with 6/6 considered as the optimum amount.

For the skiametric procedure, the skiascope used was the Tait dynamic instrument, developed by the author, and described fully in a previous paper.[11]

Procedure. The procedure was identical with every *S* regardless of age. The name and age of *S* were first noted and then the inter-pupillary distance was measured by the parallax method with the millimeter rule. This distance, ascertained when each eye was fixing straight ahead, is obviously equal to the distance between the centers of rotation of the two eyes which is the basis for the total convergence calculation. Dynamic skiametry was the next procedure. With sufficient convex lense power before each eye to make *S* artificially myopic for the fixation distance, his attention was directed to the skiascope target on the side opposite to that of the eye upon which *E* was working. The fixation-target to which *S*'s attention was directed, was previously illuminated by the chart illuminating device which is part of the instrument. *S* was then given the following directions:

> Look constantly at the illuminated target on the instrument even if you do not see it clearly. Tell me if at any time you see two targets.

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[11] E. F. Tait, A quantitative system of dynamic skiametry, *Amer. J. Optom.*, 6, 1929, 669-693.

By tilting the skiascope and observing the fundus reflex, the movement of the margin of the reflex was then noted and if it was in the direction opposite to the movement of the light upon the face, the convex lense value was reduced until the movement was extremely rapid, showing that the conjugate focus to *S*'s retina was approximately coincident with the position of the principal nodal point of *E*'s eye. If the initial movement coincided with the facial illumination, higher convex lenses were applied until the movement was in the opposite direction. As long as the movement of the reflex was rather slow, the lense values were successively reduced in each eye, shifting *S*'s attention to the right and the left targets successively, but as soon as the movement became rather rapid the attention of *S* was directed to the illuminated chart to his left and the right eye was then carried to the point of extremely rapid movement of the reflex. Then the skiascope illumination was shifted to the chart on the other side and the left eye corrected in the same way and to the same extent. Lack of fixation by both eyes was readily noticeable by reason of the rapid changes in the reflex or by *S*'s report of diplopia. The values thus found were recorded as the dynamic skiametric results. Shifts in the rate and type of the reflex movements were readily observable with lense differences of 0.25 diopter so that this figure would represent the maximum experimental error.

A careful subjective judgment procedure, with fixation at 6 m. and utilizing the same principle of supplying excess convex lense power and then reducing, was the next operation. One eye at a time was occluded and the highest plus lense with which 6/6 or better vision was attainable was noted as the subjective finding. In the vast majority of *S*s the astigmatism, if present, required for correction the same cylindrical value in the subjective procedure as in the dynamic, and only such cases are here presented, as those having variable cylindrical values introduced a consideration foreign to the purpose of this paper, that of compensatory lenticular astigmatism.

For the determination of the esophoria or exophoria at 6 m., a six-prism diopter prism, with the subjective lense correction in place, was inserted base down in front of the right eye and *S*'s attention directed to the 6/60 letter E in the test cabinet. *S* was then questioned as follows.

> You now see two Es off in the distance. Is the lower E directly under the upper one, or is it to the right or to the left?

The rotary prism was then swung into place in front of the left eye with its base-apex line in the proper direction, and *S* given the following instructions.

> Tell me just when the lower E is directly under the upper one.

If the rotary prism, when vertical coincidence was secured, is base out, esophoria was present; if base in, exophoria, and this and the amount were noted.

The same procedure was then followed with fixation at 1/3 m. on the near-point fixation-target and the esophoria or exophoria for near determined. During this process sufficient time, usually 1–2 min., was permitted for the lower E to settle into position as it usually oscillated somewhat while the prism value was slowly changing.

Discussion of method. It may readily be seen that the dynamic determination corrected not only *S*'s refractive error but that between the lenses and his accommodation the added positive refractive power for the near-point fixation was supplied. If the distance correction as found by the subjective method is then compared with the dynamic skiametric findings, the amount of accommodation used during the dynamic procedure may be calculated readily by subtracting from 3 diopters the difference between the two.

That the stimulus for this accommodation is not due to the lack of a clear retinal image with less accommodation is apparent when the details of the dynamic method are considered. Sufficient convex lense power to relieve accommodation totally was present during the early phases of the operation, but was not utilized by the eye. It can be shown by inhibition with base in prisms that this residual accommodation is due to the convergent stimulus and that it, when present, is an indication of the efficiency of the convergence-accommodation reflex.

In the study of convergence it has been necessary to utilize the classification of ocular muscle anomalies, suggested by Stevens,[12] wherein the terms esophoria and exophoria serve to classify the possibilities in horizontal heterophoria. Esophoria is considered as indicating a tendency, while the fusion faculty is functioning to neutralize it and prevent actual movement, of the visual axes of the two eyes to converge with respect to each other. Exophoria, on the other hand, indicates a tendency toward divergence of the visual axes under the same conditions. Both esophoria and exophoria are necessarily relative to the distance of fixation and to the influence of accommodation. In the determination of either of these conditions the first requisite is to suspend the action of the fusion faculty and thus destroy its power to innervate the extra ocular muscles in the interest of single binocular vision. This procedure, accomplished by the use of one of the many dissociation methods, in the present instance by the use of a six-prism diopter prism base down in front of the right eye, permits the visual axis of each eye to assume a definite position determined by the extent and distribution of the reciprocal tonic innervation to the extra ocular muscles; provided that the fixation is at distance and any refractive defects are corrected by proper glasses. Where accommodation is in use, as in near fixation, more or less convergence effect stimulated by the accommodative action is usually added to the basic tonic innervational distribution, thus effecting a difference in the angular relations of the visual axes from those obtaining at distance. The effect of this accommodation can be measured without difficulty by comparing the results of the distance convergence determination with those of the near. By reason of the slight judgment error and the lack of accurate calibration of the rotary prisms to less than one prism diopter, there was an inherent maximum error of that amount in measuring esophoria or exophoria. Weymouth, Brust, and Gobar's table was therefore used only to the nearest full prism diopter value when calculating the total convergence from parallelism to the visual axes to fixation at 1/3 m. from the corneae.[13] Following are the total convergence values for pupillary distances of from 55–70 mm.

Pupillary Distance (in mm.)	Total Convergence (in prism diopters)
55–58	16
59–61	17
62–65	18
66–70	19

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[12] G. T. Stevens, *Motor Apparatus of the Eyes,* 1906, 215.
[13] Weymouth, Brust, and Gobar, *op. cit.,* 190.

Convergent Accommodation (In diopters)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
2.00										1		2	1			1						
1.75									1		1	4	2	1		2						
1.50					1		2	2	1	2	4	9	20	13	17	11	7	6	7	1	2	2
1.25			1		1		1	1	3	8	13	9	11	4	11	10	9	3	2	1		
1.00				1	1	2	5	12	12	5	18	10	9	5	6	4	5					
.75				1	1	8	16	8	11	10	7	7	2	3		1		2		1		1
.50	1	1	3	1	8	7	4	6	10	3	4	3	4		1		1					
.25	1	2	2	3	5	3	2	4	2		2			1								
0	11	6	2	3	2		1	1														

Accommodative Convergence
(In prism diopters)

Fig. 1. Distribution of 500 Ss, Ages 10–40 Yr.

Convergent Accommodation (In diopters)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
2.00																						
1.75												1		1								
1.50					1		1		1	1		2	9	3	4	2	3	2	4		1	
1.25									1	2	5	3	3	1		4	3					
1.00						2	2		4	1	2		3	1								
.75				1			2			1		2						1				
.50			2	2																		
.25			1					2														
0	2																					

Accommodative Convergence
(In prism diopters)

Fig. 2. Distribution of 89 Ss, Ages 10–17 Yr.

CONVERGENT ACCOMMODATION (In diopters)																						
2.00												1				1						1
1.75												1	1							2		
1.50							1				1	2	3	3	8	5	2			1	1	1
1.25			1		1			1		2	4	5	4	1	6	3	4	1	2			
1.00							1	7	3	3	5	3	5	1	3	2	2					
.75					1	6	11	3	5	3	3	2	1	1								1
.50	1	1			3	3	1	2	8	1		2	2									
.25	1	1	1		2		1	2	1		1											
0	5	3	1	1	2		1	1														
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21

ACCOMMODATIVE CONVERGENCE
(In prism diopters)

FIG. 3. DISTRIBUTION OF 190 Ss, AGES 18–25 YR.

CONVERGENT ACCOMMODATION (In diopters)	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
2.00												1	1									
1.75												2				1						1
1.50								1		1	1	2	1	3	4	1		2	2			1
1.25									1	3	1	1		2	1	1		1				
1.00					1		1	2	3		3	1		2			3					
.75						1	2	1	5	3	2		1							1		
.50					1	1	2	1	2		1											
.25					2	2	1							1								
0	1	1																				

ACCOMMODATIVE CONVERGENCE
(In prism diopters)

FIG. 4. DISTRIBUTION OF 84 Ss, AGES 26–30 YR.

CONVERGENT ACCOMMODATION
(In diopters)

	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
2.00										1												
1.75									1		1		1			1					1	
1.50								1			2	3	7	4	1	3	2	2	1			
1.25							1		1	1	3		4		4	2	2	1		1		
1.00				1			1	3	2	1	8	6	1	1	3	2						
.75						1	1	4	1	3	2	3		2		1		1				
.50			1	1	2	3	1	3		2	3	1	2		1		1					
.25		1		3	1	1			1		1											
0	3	2	1	2																		

ACCOMMODATIVE CONVERGENCE
(In prism diopters)

FIG. 5. DISTRIBUTION OF 137 Ss, AGES 31–40 YR.

For purposes of calculation esophoria was considered as a plus quantity and exophoria as minus. The accommodative-convergence was then calculated by substituting in the following formulae:

$$(1)\ N_e - D_e = P_e$$
$$(2)\ C_t + P_e = C_a$$

Where:

N_e = Near point esophoria or exophoria,
D_e = Distance esophoria or exophoria,
P_e = Physiological exophoria or esophoria,
C_t = Total convergence from parallelism to one-third meter, and
C_a = Accommodative convergence.

RESULTS

The distribution of the 500 cases with respect to their accommodative convergence and convergent accommodation value will be found in Fig. 1. In this whole series the greatest amount of convergent accommodation encountered was 2 diopters and the greatest amount of accommodative convergence was 21 prism diopters with 5 *S*s each. On the other hand, there were 11 *Ss* with neither accommodative convergence nor convergent accommodation. The other *S*s were distributed with noticeable regularity between these two extremes. A superficial inspection of the table will show that convergent accommodation is not only present in most cases but that there is a very positive correlation between it and the accommodative convergence. The actual coefficient of correlation, by the Product-Moment Method,[14] is +0.73 ± .014.

The distributions of the 4 age-groups will be found in Figs. 2-5. Fig. 2 gives the results and scatter of the pre-college group, ages 10-17 yr.; Fig. 3, of the college group, ages 18-25 yr.; Fig. 4, of the group between the college and the pre-presbyopes, ages 26-30 yr.; and Fig. 5, of the pre-presbyopic group with diminished amplitudes of accommodation, ages 31-40 yr.

It will be noted that age has apparently no modifying effect upon the presence of either accommodative convergence or convergent accommodation as the general character of the distribution is similar in all 4 age-groups, the atypical as well as the typical cases being represented in each. In considering the atypical aspects of the distribution, it should be noted that there are relatively few cases in which there is accommodative convergence without convergent accommodation or convergent accommodation without accommodative convergence. A noteworthy characteristic of the convergent accommodation distribution is that, while there are 107 *S*s in the 1.50 diopter group, there are only 15 *S*s in the 1.75 diopter group, 6 in the 2.00 diopter group, and none higher. Between 0 and 1.50 diopters, however, is a steadily increasing number for every 0.25 diopter increase. This would give a badly skewed curve if plotted according to the amount of convergent accommodation.

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[14] H. E. Garrett, *Statistics in Psychology and Education,* 1926, 163-168.

The curve for the accommodative convergence would, on the other hand, show a very definite regularity if allowance were made for the inequalities introduced by the apparent predilection of the observer for even numbers in the choice of values made necessary by the lack of fractional sub-divisions on the rotary prisms.

Discussion. In view of the fairly high positive correlation obtained between the accommodative convergence and the convergent accommodation determinations, it is apparent that Helmholtz's view is supported and amplified in so far as that is possible with the relatively limited number of *S*s studied. If we consider that the laws of association, as deduced from learning experiments, are valid in subliminal conscious processes as well as in the conscious (and there is no good reason for not doing so) the results of this investigation may be taken as indicating that there is usually a reciprocal relationship existing between accommodation and convergence, the source and type of the stimulus depending upon whether the accommodation or the convergence is the dominant factor. In this investigation, the author first made the convergence dominant by securing binocular fixation at a near-point and eliminating accommodation as far as possible by supplying convex lense power equal to the focal length of the fixation-distance. Under these conditions some accommodation did relax and was measured by the amount of plus lense value which had to be deducted from the initial plus excess.

Dominancy was then transferred to the accommodation by permitting the eyes to use a normal amount of accommodation and at the same time eliminating the influence of the fusion faculty, thus freeing convergence. The convergence existing under such conditions must be that which is stimulated by the presence of accommodation.

Binocular fixation and accommodation for a near point, however, if the values obtained in this experiment may be taken as representative, involve more of both accommodation and convergence than the usual amounts supplied when either is the stimulus and the other the effect. Inasmuch as accommodation is the primary requirement for clear vision, it is probable that the usual dominancy is that of the accommodation, which would give more or less accommodative convergence depending upon the efficiency of the reflex. This is the more likely in that there is a very effective mechanism in the fusional convergence innervation for supplying any deficiencies in accommodative convergence. The convergent accommodative reflex probably would not, therefore, be effective under ordinary conditions, but would be immediately operative in situations where the convergence would become dominant as in the so-called 'fogging' system of subjective or objective refraction where strong convex lenses are applied at the beginning of the process.

The accommodative convergence values found in this experiment show that 8–14 prism diopters may be considered as the usual and therefore optimum range, thus agreeing very well with Sheard's determination.[15] It is also further apparent that convergent accommodation of over 2 diopters is not common, no such case being found during the present investigation. The relatively great number of *S*s with 1.50 diopters, however, suggests that this amount represents the optimum value for this reflex. That the amount of convergent accommodation is not dependent upon the amplitude of accommodation of the individual is quite apparent when the values in the 4 age-groups are considered. The average amplitude of accommodation in the pre-presbyopic group, in which the amplitude is decreasing rapidly, must be much less than in any of the other groups, but an inspection of the age-group results will show that there are proportionally as many in the upper classifications in the older as in the younger *S*s. This optimum value of 1.50 diopters for the convergent accommodation confirms quite fully the results of Bestor who found, as the result of a survey of 139 cases, 1.50 diopters as the average amount of accommodative lag with fixation at 1/3 m.[16] Although not so designated or considered by Bestor, the difference between the accommodative lag and the total near point refractive value of 3 diopters must have been due to the presence of that amount of convergent accommodation.

The existence of a reflex relationship between accommodation and convergence, reciprocal in nature, in which either can be the primary source of the stimulus and the other the response, may be tentatively established as a result of the work reported in this paper. The development of physiological optics in general and visual psychology in particular will probably demand further investigations along these lines.

SUMMARY

(1) Accommodation apparently due to a stimulus primarily derived from convergence was present in practically all of the *S*s and must therefore be considered as a normal reflex.

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[15] Sheard, *Op. cit.*, *Amer. J. Physiol. Opt.*, 1, 1920, 249.

[16] H. M. Bestor, The interpretation of dynamic skiametric findings, *Amer. J. Physiol. Opt.*, 1, 1920, 229.

(2) The amount of convergent accommodation varied from 0–2 diopters with 1.50 diopters as the probable optimum value.

(3) The accommodative convergence values are in close agreement with those found in previous experiments.

(4) The amount of convergent accommodation in the average *S* is normally quite proportional to the amount of accommodative convergence, when measured by the methods used herein, as is shown by the relatively high positive correlation between them.

(5) The view is here suggested that there is a reflex relationship between accommodation and convergence, reciprocal and associative in nature, in which either can be conceived as the primary stimulus and the other the response.

INSIGHT *VS.* TRIAL-AND-ERROR IN THE SOLUTION OF PROBLEMS

By GEORGE W. HARTMANN, Pennsylvania State College

The exact nature of the mental and neural changes which occur whenever an act is learned continues to be a matter for dispute. Undoubtedly the issue will remain controversial as long as psychologists are persuaded that the real key to behavior is to be found in the properties of the central nervous system. The ultimate substantiation of any thorough-going theory of learning is admittedly contingent upon advances in neurological research; nevertheless, despite the reciprocal benefits which these fields have enjoyed, the learning hypotheses have always suffered from inadequacies in descriptions of the way in which an organism modifies its conduct when confronted with new situations. These deficiencies are not present in reports of single studies but appear in comparing several experiments because different problems involving distinct reactions were used.

An explanation of learning in terms of lowered synaptic resistance is plausible when built upon evidence derived from simple maze-running and the memorization of a nonsense syllable series where 'gradualness' in obtaining mastery is the most important single feature; but it rings less convincingly when applied to rational puzzle-solving in the human where 'suddenness' in making the correct response is outstanding. The 'trial-and-error' explanation of traditional associationism fits the typical negatively-accelerated curve very well, but the 'insight' version of Gestalt psychology is more in conformity with those curves which reveal precipitous drops or rises.

Configurationists maintain that trial-and-error accounts are valid only in those experimental situations where a perception or comprehension of the total setting is impossible under the conditions imposed upon the learner. There, accidental or chance happenings in the strict mathematical sense are the major determinants of progress. Whatever intelligence the learner may have is hamstrung by these mechanical limitations. In cases of true learning, *i.e.* instances where a survey of all the factors in the problem is permitted, the solutions occur as soon as insight into all the relations involved is reached. This view seemingly demands that little or no improvement appear until the moment of insight is established when a sharp inflection in the curve takes place.

Those who believe that learning is essentially the formation of bonds claim that the burden of proof is upon the configurationists. They assert that trial-and-error events are frequently and definitely observable in animal behavior but that insightful conduct is rare. Moreover, insight is very likely the outcome of a longer period of mental exploration, during which trial-and-error combinations will have

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had ample opportunity to be formed, tested, and discarded; overt behavior is merely delayed until the appropriate response has been chosen.

It was for the purpose of weighing anew the evidence in favor of these different positions that the present investigation was designed and undertaken.

MATERIALS AND PROCEDURE

In planning the experiment, we assumed that it would be easier to discover the rôles of insight and trial-and-error in the higher thought processes than in simpler motor performances. Problems were, therefore, selected from the field of thought. They were all constructed so that errors would abound until the 'principle' of solution was discerned, after which no further mistakes would appear, *i.e.* 100% failure preceded the 'critical moment,' which was followed by 100% success. The character of the critical moment and its dependence upon earlier trials was the center of interest. Consequently all the test problems were given the *Os* individually so that both *O* and *E* would have ample opportunity for observation and report.

Test problems. Ten tests were devised and presented the *Os*. In addition to the specific instructions which were given with every test, the following general instructions were read to the *Os*, so as to insure the appropriate mental set, at the beginning of every experimental session.

"During this period a number of different tasks will be put before you. Each task appears in serial form; your problem is to find the principle underlying each series. These principles are of such a nature that as soon as you have discovered the right one for each set, the solution of the remaining problems is easy. Do your best and watch carefully what happens to you. Don't be afraid to express your thoughts as you work."

Test 1: (Point sequence). The test sheet (see Fig. 1) was divided into three parts: the first contained a number of black dots; the second a number of small circles; and the third a number of small equilateral triangles. The pattern of each part was apparently haphazard, but as a matter of fact it was carefully constructed so that some of the items would appear on straight lines. *E* placed this sheet before *O* and said:

"Note well everything I do. A general principle underlies the movements I make. As soon as you find out what this is, do something similar with the other two spot-groups."

E then pointed to a dot which was at one end of a straight line of dots and to the next to the last dot at the other end of the row—to "a" and "b" in Fig. 1. *O* was told to do the same with the circles and to give his reasons. If he falsely interpreted the instruction, a second illustration was given him by *E*'s pointing to "c" and "d." If the principle was still undiscovered, a third illustration was given by pointing to "e" and "f"; and so on until the solution was achieved or defeat acknowledged. In this and all of the other tests, *E* noted *O*'s activities during the problem solving and also at the end of each experiment recorded *O's* verbal reports.

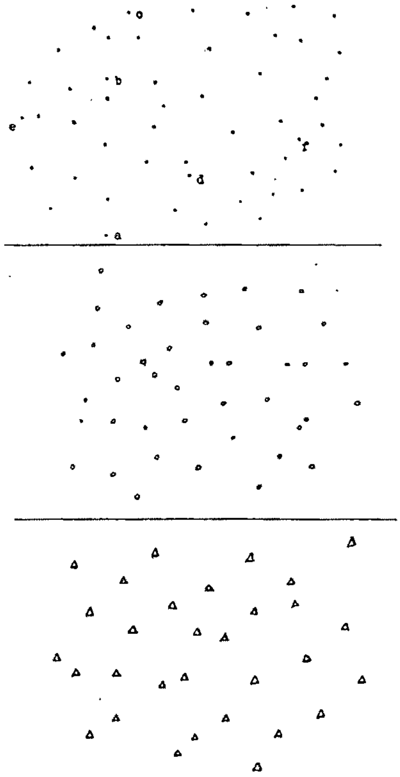


FIG. 1. POINT SEQUENCE TEST

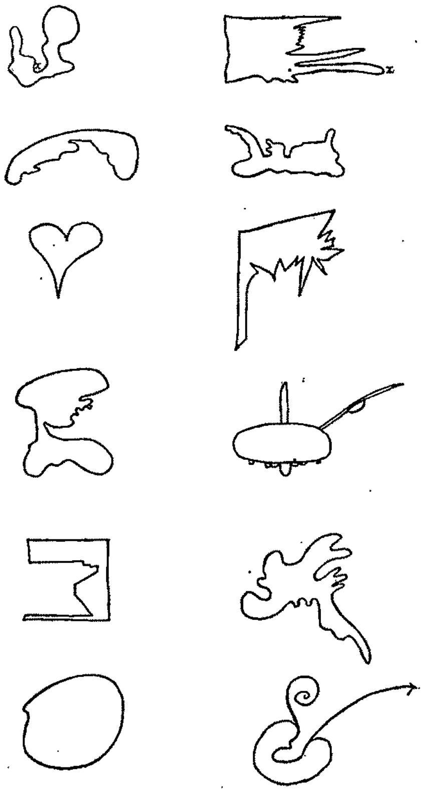


FIG. 2. CONCAVITY-CONVEXITY TEST

Test 2: (Concavity-convexity). A sheet containing a paired arrangement of rounded and angular figures (see Fig. 2) was placed before *O*. *E* then said:

"Watch me. I point here and there. There is a reason why I have done just what I did. Please do the same with the remaining figures."

As *E* spoke, he brought his pointer down at the inmost concave indentation of the left member of the pair and at the outmost convex projection of the right member—to the points marked "x" in Fig. 2. If *O* got the idea at once as evidenced by his ability to indicate the corresponding concavities and convexities in the other figures of the series, no further action occurred, but if he had followed a false clue, another illustration with the second pair was offered—and so on, until successful.

Test 3: (Sentences). A page containing the following sentences was laid before *O*.

(1) The youth loves the beautiful maiden.
(2) Sound is localized externally.
(3) Why are the students drawn to socialism?
(4) I developed the pictures myself.

(5) Solitary study of an experimental science can never be a substitute for observation.
(6) The aforesaid guaranty will be honored by all our shops.
(7) The committee wishes to favor all protestants.
(8) The inclosed bill is due upon presentation.
(9) He is the oldest user of the library.

As *O* turned to these sentences *E* remarked:

"Read carefully the first two sentences. The underlined words in both have the same relation to the whole sentence. Show me the corresponding terms in the other sentences."

When *O* made a wrong selection, *E* said, "Incorrect," when a correct selection *E* said, "Right." If no progress had been made by *O* in 5 min., *E* illustrated the problem in the next line; and so on, until the end of the list.

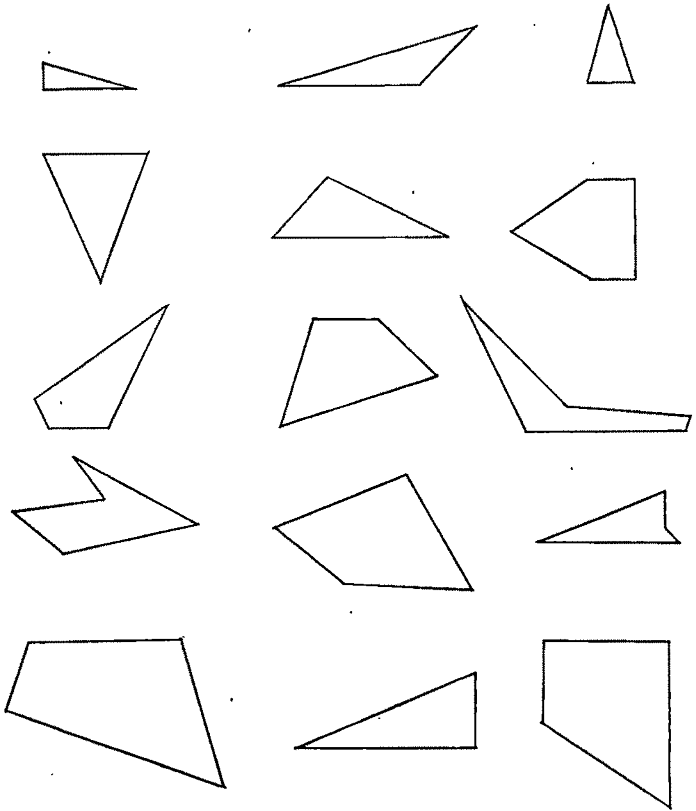


FIG. 3. ANGLE TEST

Test 4: (Angles). Each horizontal row of geometrical figures, shown in Fig. 3, was considered as a unit. *E* pointed to the most acute angle in the first figure, the most obtuse angle in the middle, and the most acute one again in the last; and then requested *O* to repeat this pattern with the others. Examples were continued until *O* 'caught on.'

Test 5: (Inverted analogies). Test 5 was a deviation from the ordinary analogies test used in many measures of intelligence.

(1) steel : hard : : burden : light
(2) stomach : middle : : head : bottom
(3) one : two : : today : ________
(4) egg : oval : : sun : ________
(5) pot : cook : : broom : ________
(6) sky : blue : : snow : ________
(7) Socrates : wise : : Hercules : ________
(8) acids : sour : : honey : ________
(9) lamp : stove : : flame : ________
(10) eat : hungry : : sleep : ________
(11) feathers : light : : diamond : ________
(12) mouth : hand : : eat : ________

Instead of filling in the normally expected analogue, *O* was first required to find it and then give its opposite. The instructions read:

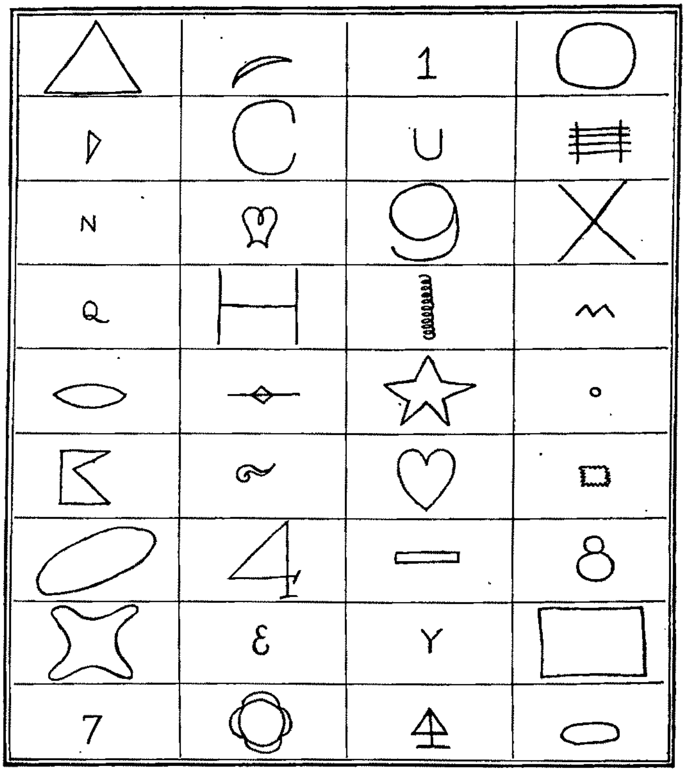


Fig. 4. Familiar Figure Test

"The underlined words in the first two model sentences are deemed correct because no other words satisfy the principle implicit here. Analyze the examples for the rule and apply to the remaining items."

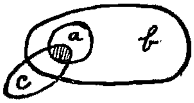
If the 'point' was not soon seen, added helps were given *O* by cumulating, one by one, the right answers.

Test 6: (Figures). The material for Test 6 is shown in Fig. 4. Four columns of familiar outlines were placed upon a sheet, each row across constituting a unit problem, and consisting of one large circular figure, a large angular one, a small circular one, and a small angular one. The position each occupied, however, was purely haphazard. *E* pointed to the smallest rounded one in the first row and said, "I call this one correct." When necessary, the same act was performed with the second row, etc.

Test 7: (Syllogisms). A sheet containing the following was given *O.*

Please observe the following:

(1) All a is b;
Some a is c;
Some b is c. (True statement)



Why this is true can be demonstrated by means of circles, as in the sample figure. In similar fashion prove the truth or falsity of the following propositions.

(2) All Löso is Skandbeg;
All Skandbeg is Heifors;
All Heifors is Löso.

(3) Many beta are gamma;
All gamma are delta;
All beta are delta.

(4) Some x's are squares;
All squares are p's.
Some x's are p's.

(5) No Nirvana is Mahatma;
No Aurangzeb is Nirvana;
No Mahatma is Aurangzeb.

(6) Iron is not gold;
Whatever is not gold, has no value;
Iron has no value.

The first one—the model—was tested for its truth or falsity by means of Eulerian circles. It was hoped that *O,* in watching the rough sketch illustrating the device, would notice that the graphic representation of the degree of overlap (or freedom therefrom) was the essence of the method. He was then asked to test pictorially the validity of the remaining conclusions. If he failed to make headway, the second problem was illustrated, and so on, until learned.

Test 8: (Objects). This situation demanded the use of definite concrete material. A rather large collection of common articles was arranged upon the laboratory table in the following order:

(1) beech, oak, and pine pegs of uniform size.
(2) iron filings, face powder, and sawdust in similar dishes.
(3) lump of clay, glass disk, and porcelain plate.
(4) steel, lead, and brass bars of uniform length.
(5) leather strip, cloth strip, and absorbent cotton.

(6) rock candy, water, and bread.
(7) shaving brush, nail brush, and toothbrush.

E then said to *O:*

"Look at this first row of objects. I call this correct (pointing to the pine block). Now look at the next row. For the same reason I consider this correct (pointing to the face powder). Now try to identify the right item in the remaining rows."

If more help were needed *E* indicated the correct object in the next row, and so on until learned.

Test 9: (Prime numbers). A sheet containing the following was given *O.*

(A)	12 and 4	12 plus 4 makes 16 12 times 4 makes 48 12 minus 4 makes 8 12 divided by 4 makes 3. I consider 3 correct.
(B)	10 and 5	10 plus 5 makes 15 10 minus 5 makes 5 10 times 5 makes 50 10 divided by 5 makes 2. Either 5 or 2 is correct.
(C)	14 and 2	(16, 12, 28, 7)
(D)	20 and 1	(21, 19, 20, 20)
(E)	15 and 3	(18, 12, 45, 5)
(F)	24 and 12	(36, 12, 288, 2)
(G)	29 and 2	(31, 27, 58, 14½)
(H)	17 and 4	(21, 13, 68, 4¼)
(I)	110 and 10	(120, 100, 1100, 11)
(J)	39 and 4	(43, 35, 136, 9¾)
(K)	31 and 8	(39, 23, 248, 3⅞)
(L)	9 and 8	(17, 1, 72, 1⅛)

E then said to *O:*

"Notice the first pair of numbers at the top of this blank, 12 and 4. You understand, of course, that I can perform the four elementary arithmetical operations upon them such as: 12 plus 4 makes 16; 12 minus 4 makes 8; 12 times 4 makes 48; and 12 divided by 4 makes 3. I arbitrarily designate 3 correct, but I have adhered to a fixed rule in doing so. If I do the same with the next pair, 10 and 5, I consider either 5 or 2 as correct. Try to discover why and identify the 'right' number among the other possibilities."

Test 10: (Rational Learning). Peterson's original and unmodified form of the test was used with the accompanying instructions:[1]

"This is a memory-reason test. The letters A, B, C, D, E, F, G, H, I, and J are numbered in a random order from 1 to 10. I shall call out the letters in their order and you are to guess numbers for each letter till you get the correct number when I shall say 'Right.' Then I shall call out the next letter, and so on. This procedure shall be continued till you get each number right at the first guess twice in succession through the series, from A to J. Then you are through. You must ask no questions, but are to use all the mental powers at your command. You will be judged by the number of errors (or wrong guesses) you make and the number of repetitions from A to J required."

E's prepared sheet contained the following code with space for recording *O*'s responses:

Letters	A	B	C	D	E	F	G	H	I	J
Numbers	9	6	2	10	8	1	5	4	7	3

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[1] J. Peterson, Experiments in rational learning, *Psychol. Rev.,* 25, 1918, 443-467.

It should be noted that this test deviates somewhat from the special thought-pattern which characterizes the nine preceding situations, but it was deliberately included to see if it would throw light upon differences in the methodology of solution, should such be found.

RESULTS

A summary of the data, obtained in the tests described above, is given in Table I. The number of repetitions or separate illustrations required by the *O*s constitutes an index of learning which is reasonably free from an interpretative bias. Consequently, the results were first organized in this form.

TABLE I

NUMBER EXAMPLES REQUIRED BEFORE THE PRINCIPLE OF SOLUTION WAS COMPREHENDED

("x" denotes failures)

O	Tests									
	1	2	3	4	5	6	7	8	9	10
A	x	x		x	6			7(?)	x	
B	1	4	x		1			4		
C	3	5	x		5			1(?)		
D		4	x	1	5	1	1		x	2
F		1	x	1	5			x	x	3
G	8			1		9			x	
H	3	1	x	1	3	4		4		
I							5			
J							6			
K							6			
L	x	6		1	3		4			
M	x	3			2		3			
N	1	3	x	3		2		3	x	
Av.	3.20	3.33	x	1.33	3.75	4.00	4.16	3.80	x	2.50

Total Average = 3.26

The different averages for each type of test and the total mean should be understood as having a low degree of precision, since the *O*s do not correspond in all situations and particularly since the values are derived only from instances in which solutions were actually obtained. If the x values had been carried far enough to procure definite figures, the means for all tests would undoubtedly have been higher. 'Instantaneous' solutions, *i.e.* those which occur after but a single illustration, are certainly not the rule. 'Sudden' solutions—those which appear after a longer interval of delay during which no apparent progress has been made—are often preceded by gross errors. Since the table does not reveal the qualitative features of the learning—upon which, in the last analysis, the theories really hinge—it will be necessary to examine in some detail the extensive protocols at this point. These excerpts show best the development of the thought processes toward the goal.

Test 1. Observer *A:* No solution was reached, even after 14 distinct operations had been presented. *A* thought of the following possibilities in order: (1) that parallel lines were being traced; (2) that two neighboring points must be in each series; (3) that a reversal of direction was the significant thing; and (4) that some geometrical figure was being formed by the movement. In common with other *O*'s loss of memory for the earlier models caused great confusion in the later stages, so that one may safely claim that if the solution is not reached within the first few trials, it is unlikely that it will be attained over a longer period.

B: Correct after one exhibition. *B* first saw a "cloud of spots" and before any action developed, he had adopted a critical attitude and asked himself, "What possible events can happen here?" The same instant that *E* pointed to definite spots, *B* saw the straight line relationship and hit simultaneously upon the principle—uniting the first and next-to-last member.

C: Correct after two models. There were no competing principles; the first suggestion which arose was the only meaningful one. "The idea was simply there. I simply saw what you demonstrated. No further problem was involved."

G: Correct at the eighth trial. *G* noticed the straight lines first, then the rhythm of movement; later, he tried incipiently, but rejected the notion of auditory tempo. After seven models, he observed that termination of the movement at the second point from the end was the only common element.

H: Correct on third demonstration. "The solution came immediately; until then my mind was an utter blank. Even when the idea was clear, I was not overconfident and thought the matter might be more complex." This record is interesting because it indicates that not all insight carries with it a conviction of its own correctness (cf. *A*'s results in Test 8).

L: Unsolved. "Either the task is meaningless or no task at all is involved. My guess is that you are just playing a joke on me to see how I would respond in the presence of utter nonsense. Frankly, I'm irritated."

Test 2. A: No solution was reached despite 30 min. of faithful work. Three distinct hypotheses appeared to observer *A:* (1) that similar angles were involved; (2) that projections of like shape were to be sought; and (3) that the correct point lay between two great bends. *A* explained that in this last he assumed a principle of equilibrium, and that if he was wrong, it was because he had been misled by the configurationist doctrine of 'good' figure or contour.

B: "At first I thought you wanted absolutely identical points, but then I assumed that only corresponding points were required. Consequently, I sought the farthest points inward in the left figure and the farthest point outward in the right one."

C: "The solution was purely accidental. I had no reason for believing it to be the correct one; I could just as well have taken some other. Subjectively, there was nothing novel about the manner in which the idea arose—merely a brief warming-up and then it was there!"

D: "Blind chance-taking was all that occurred. I should not have been surprised had my third answer been wrong. It was very much like a *guessing game* to me."

F: "The whole process was one of immediate perception. I saw at once that no conventional geometrical character was involved as the figures were too irregular to be based on any curved or linear feature. The principle was quickly verbalized as 'the most indented first and then the most projected.' "

Test 3. B: " I tried definitely to locate some internal rational meaning to the sentences. My first thought was that words which were not absolutely necessary were to be underlined; next I tried the most important terms; then I assumed fixed parts of speech were involved. After that I was blocked, and although the possibility of counting letters occurred to me, it was rejected as too external a criterion." The last statement is of interest because it shows that the necessary 'lead' had been struck; without a guiding principle, however, to employ it, it proved abortive.

D: "From the beginning I considered a mechanical possibility and developed the following hypotheses in succession: words with double letters; first words of phrases; second word from beginning or end; number of letters in different words; plus at least a half-dozen other remote possibilities which have left me." There it should be noted that as the number of conceivable principles increases (or, as in this case, even approaches infinity) the chances of solution become progressively less.

Test 4. A: No solution. The separate suggestions were: (1) the absolute magnitudes were decisive; (2) all three angles constituting 180° together; (3) the angles opposite the largest sides.

D: "I wasn't sure of the correctness of my response when I gave it. The hypothesis was held tentatively until you [*E*] said 'yes'."

F: "The idea just came—how or why I don't know. It appeared as a single event. There was a feeling of indistinct looking followed by something arising. I see in the solution a very obvious characteristic of the figures." This report reveals the difficulties in describing a genuine experience of insight. Either the process occurs too rapidly or the essential antecedents cannot be found in focal consciousness; perhaps both facts are involved.

Test 5. A: The principle was finally discerned after much groping and by modifying partial insights into a more complete one. The notion of opposition was isolated after inspection of the original examples but it lacked precision; since *A* responded with "darkness" to the stimulus word "sun." Similarly, the term "lady" was given to match "broom," because that designation is the opposite of a servant," who would normally deal with the domestic items listed! Even after the correct word "weak" was given to "Hercules," the principle was uncertainly applied. Apparently—as every experienced teacher knows—it is possible to be on the right track and yet have one's vision of the goal obscured by some inner 'cloud.'

C: "The notion did not come with a leap—instead it was as though one were going in swimming and slowly sticking one's feet into the water!" Gradual growth characterized the formation of the concept in this case. The uncertainty with which the principle had been achieved was shown in the occasional errors of application after the solution had arrived.

D: "The correct idea arose after you said 'square' in connection with the word 'sun.' The right answer here was sharply pictorial and I saw the point vividly and instantaneously."

F: "After my initial errors, I intended to give some kind of paradoxical opposition but I offered the wrong attributes or antagonists. Finally, the exact reaction appeared. Something happened within me resembling the electric light bulb which the comic papers place over the stupid fellow's head."

Test 6. G: "My most persistent error was the assumption that the number of sides was the crucial factor. The idea that size might be the criterion suddenly popped out; after a visual sweep there was added to this the principle of line quality. My numerous earlier errors were probably due to my adherence to absolute rather than relative judgments." This report is of interest in demonstrating that a solution often appears after an appropriate assembly of part or component ideas has been formed.

H: "My first hypothesis dealt with the matter of symmetry. This was quickly discarded. Finally, the complex principle came in sections: the features of bigness and littleness arose first, then roundness and squareness. Luck appears to play a big part in the searching process—one either sees the objective or not at all."

N: "Initial inspection showed that I could either proceed according to size or according to form. All at once it struck me that one could combine both criteria. Since this relation was easy to define, the rest was merely a matter of seeing whether it fitted the conditions."

Test 7. D: "This is a clever new wrinkle to me. It is plain that the conclusion is right or wrong only if the overlapping circles reflect the terms of the statements. Correctness or falsity is immediately seen by spatial position. After the first correct solution the thought process tends to be reduced to a visual reaction; the only difference being that a representative or symbolic character is presupposed."

J: "The correct procedure to be followed developed only slowly and gradually. Yet now the matter is completely clear. The isolated facts must first be placed in a larger total system; as soon as this coördination of relations has been arranged, the principle is plain."

Test 8. A: The solution in this case was ambiguous, hence the question-mark after the figure in the table. At first *A* asked, "Are all the objects symbols for shaving?" (for some reason, the shaving brush was a prominent item in his perceptual field). Then he sought for some common sensory object, such as size. At the very end, making a sudden grimace, he exclaimed, "How about *softness?*" But this idea, although correct, was rejected since the metal and wooden blocks seemed to lie beyond the range of application of this principle. One is surprised to find a solution actually at hand discarded because it does not meet a superficial test of fitness. The process of verification would seem to be an integral, although subsequent, part of the insightful process, for otherwise the insight remains imperfect (cf. *H*'s results in Test 1).

B: "Numerous principles suggested themselves—the tallest item, the one farthest to the right, the most frequently used, the dampest, etc. Then suddenly—after a survey of all the objects—came the idea of the least resistant."

H: "I classified the objects first into useful and useless items, and later tried the principle of hardness. I was 'stumped' by the metals, all evidently stout and shiny, until I reëxamined them and saw that one was lead. In trying to figure out the relation, I happened to dig my thumbnail into the lead block—and the whole thing cleared up at once."

Test 9. G: "My assumptions ran in this order: (1) the smallest number; (2) the odd number; (3) a combination of (1) and (2); (4) some geometrical series; (5) numbers which when represented graphically would form hexagons! The real answer is probably something wholly different, but the mathematical possibilities are too numerous for me to single out the right one."

Test 10. This test deserves separate consideration because its formal structure departs noticeably from the preceding ones. In this case, the *O*s sought for a principle although none other than purely arbitrary association was involved in the task presented. Nevertheless, it appears that rational, if not logical, procedures can be employed in deciphering a code, even though the code itself lacks a rational composition. The record of *D* illustrates this perfectly. He assumed at first that a reversed unit arrangement was being employed, *i.e.* that the initial letters of the alphabet were attached to the highest numbers and vice versa, but abandoned this principle as soon as the second letter failed to accord with it. To aid his memory for numbers already given, he bent down the first joint of the corresponding finger; consequently, he was able to suggest numbers only from those which clearly remained. If one did not work, he tried another. We have here a good instance of rational "tapping around" versus irrational "hopping around." It seems to the writer that the way in which the situation is attacked is itself evidence of insight into the necessary preliminary conditions. There is no reason for assuming that insight is restricted to occasions involving overt solution: it is perhaps just as much a matter of insight to control and regulate economically one's trial-and-error activities when one is convinced—in the absence of any definite set of meaningful relationships—that this is the only feasible way of reaching one's objective.

Discussion

An interpretation of the data secured in this study is difficult. The main result, however, may be phrased as follows: Learning can and does occur either by means of continual trial-and-error activity with an ultimate association of correct responses or through insight, *i.e.* through an immediate appreciation of the functions of various elements in a total situation. Certainly this is true on a *descriptive* level, even if the ultimate explanation of the fact of learning be sought in some *tertium quid* at present unknown.

One very obvious feature of the learning situations here employed is the frequency with which working hypotheses were developed and rejected before the desired insight made its appearance (see *G*'s record in Test 1, *A*'s in Test 5, and *B*'s in Test 8). In all of these cases, a number of possibilities was suggested and every one had to be tried before the correct principle of solution was discovered. The following question arises in this connection: Why do the numerous possibilities occur only in some tests and only with some *O*s? The answers obviously rest upon an analysis of the nature of intelligence. Lacking that we may hazard the opinion that success on the first trial may be merely the limiting case of a trial-and-error series (cf. *F*'s report in Test 4), in which a single unit of practice is adequate to establish mastery. Formally, it would be comparable to the fixation of a conditioned response—as in many classic neurotic symptoms—on the basis of but one experience. Insights may then be considered as the outcomes of prior trial-and-error and occasionally emerge when that trial-and-error is at a minimum.

On the other hand, the assertion of many critics that 'insight' is merely the product of past experience, or a re-arrangement of habitual responses, is probably false, since the tasks—with the possible exception of the syllogism and rational learning tests—are all as novel as any which the *O*s are apt to encounter in their lives. The solutions are genuine ones and within the rather narrow range of complexity embraced by the problems are as original and creative as any inventive idea. They are inductions or inferences from specific material and always represent an act of achievement.

Phenomenologically, our descriptions of the moment of insight tend to be very meager (see *F*'s report in Test 4). It seems that in this respect it is very much like the 'determining tendency' of the Würzburg School in being effective in behavior but unanalyzed as a conscious state. In some manner or other, our mental gears are shifted—or, to vary the figure, the kaleidoscopic particles are redistributed—but the product and not the process is alone accessible to us. The apparent suddenness with which insight occurs, at least in some cases, suggests an analogy if not a neurological kinship to the perceptual facts of figure and ground, especially in the quick alternation obtained with ambiguous outlines. Much of the language of reasoning is derived from the terms employed in perception and this relationship is more likely generic than fortuitous. Hunting the face in the tree is usually preceded by many false identifications, just as the typical principle in this study was only revealed after three or more (cf. the average values in Table 1) plausible principles had been tried. Even 'false' insights occur, *i.e.* they arise suddenly and carry emotional conviction as intense as the 'real' ones until their inadequacy is established by further testing.

Further, it may be noted that there are degrees if not kinds of insight which should be distinguished. The terminological difficulties inherent in the concept have been discussed by the writer in another place.[2] If the data suggested by Test 10 be of general validity, there appear to be two types of insight—one involving a comprehension of the internal structure or organization of a situation, and the other involving sagacity in selecting appropriate methodological approaches to the task of problem-solving. Normally the two probably operate together as a unit but where the first part is impossible because of the nature of the material the second may still exist.

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[2] G. W. Hartmann, The concept and criteria of insight, *Psychol. Rev.*, 38, 1931, 242-253.

On the animal plane, one probably also encounters two levels of insight: a broad one, characterized by an appreciation of the instrumental function of definite physical objects, and a narrow one, in which the understanding is limited to an awareness that in order to attain a particular end one must join one activity to another,—an *'Und'-verbindung* of the purest sort! A child knows that if one presses a button light appears, but only one familiar with elementary electricity realizes that this consequence is dependent upon the contact of two metal conductors, a charged wire, and the chemical nature of the filament.

The varieties of insight, as classified by Alpert in a study made with children,[3] were also found in our adult observers: (1) solution with *immediate* insight; (2) solution with *gradual* insight (see *C*'s results in Test 5); (a) *Partial* insight (see *F*'s results in Test 5); (b) *Complete* insight; and (3) solution with *sudden* insight (see *H*'s results in Test 8). In the case of immediate insight all the elements required by the complete configuration are present simultaneously; where gradual insight appears they are built up by accretion; and in sudden insight the imperfect configuration is quickly closed when a crucial element fits into its proper place, *i.e.* a sharp transition occurs as soon as some significant or useful fact has been observed. This last, of course, is the most representative and clearest case of insightful behavior, being preceded by a long or short period devoid of success. Between the solution of the problem and the conviction that it has been correctly solved falls an interval of varying length; but regardless of this delay the solution itself follows once the prerequisite conditions have been fulfilled. Too much stress should not be laid upon this factor of 'suddenness'—a relative term at best—since we are dealing with an inconvertible proposition, *i.e.* all sudden solutions are insightful but not all insightful solutions are sudden!

The typical hyperbolic learning curve differs markedly from any graphic records of progress which one could construct for the instances above. Nevertheless, this fact alone—of which much has been made—does not constitute proof that two utterly different species of learning are involved. The writer suspects that the negatively accelerated curves which one obtains from relatively simple motor performances differ as much from each other as they do from curves portraying gradual insight. The difficulty probably lies in the unit of measurement. In the case of insight, minute advances do not manifest themselves externally as soon as made, *i.e.* every added unit of time does not result in an increase in the amount done, but are first accumulated into higher units before overt behavior occurs.[4] A 'principle' is of the order of a higher unit and simply cannot function except in its entirety: it has a definite 'all-or-none' character. But this certainly does not imply that the trial-and-error activity which precedes the emergence of the insight is irrelevant to its formation.

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[3] A. Alpert, *The Solving of Problem-Situations by Pre-School Children,* 1928, 41.

It is curious to see how the classic problem of the transfer of training is re-opened, or at least re-interpreted, by the concept of insight as used by the defenders of the *Gestalt* doctrine. Once the insight into a certain kind of situation has been acquired, similar situations different in content but alike in form (configuration) can be adequately adjusted to without further training. Transposition of "structure-functions" unquestionably occurs in lower animals: why then should we hold to an exaggerated specificity of learning in man? To be sure, transfer takes place only within definite limits for if the later configuration be too foreign to the earlier a new insight will be demanded. There are limits to the generality which insights can assume; *e.g.* an animal which has mastered one "Umweg" problem is not thereby qualified to solve all such situations! The configurations presented by new tasks must not deviate too widely from the original insight if the latter is to be successful. Just because an adult knows how to solve the equation X^2 plus $MX = N$ by adding $(M/2)^2$ to both sides does not of itself enable him to determine the weight of a body through indirect balancing of the scales or to equalize the resistance in a Wheatstone bridge—even though they all share in common the property of maintaining equilibrium by adding certain quantities to both sides.

CONCLUSIONS

Trial-and-error activity when applied to problems which are capable of yielding general principles eventuates in insight as understood by the *Gestalt* psychologists. The required act is firmly learned as soon as, and only when, the insight has been achieved. Depending upon unknown conditions in the learner's organism, the insight may make its appearance immediately, gradually, or suddenly after a delay. Once acquired, the insight may be uncertainly applied to concrete situations or even forgotten, although it is normally very stable. The insight itself seems to be a configuration made up of familiar parts operating in a new setting. Trial-and-error experimentation appears to be necessary in inserting and rejecting the elements of a series of tentative organizations before the right one, *i.e.* the one which realizes the goal-satisfaction, is completed. Whether the same state of affairs holds for other types of problems must be determined by later research.

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[4] Cf. E. L. Thorndike, *Educational Psychology,* 2, 1913, 338-349.

CURVES OF MEMORIZATION AFTER DIFFERENT AMOUNTS OF PRACTICE

By JOHN A. MCGEOCH, University of Missouri

It has long been known that practice upon successive samples of a given learning material is normally accompanied by an increase in the speed of learning as measured in trials, time, or errors. In his study of the form of the curve of memorization Kjerstad obtained this result and added to it the very important conclusion that as the number of trials taken to learn successive samples decreases the form of the curve of memorization remains essentially unchanged.[1] He based this upon the finding, yielded by analysis of Vincent curves, that the percentages learned in halves, thirds or sixths of the total trials did not vary importantly between the two halves of the total number of lists learned. This general conclusion has recently been partially confirmed by White,[2] who has shown that the percentages of 12-syllable lists correctly anticipated at successive quarters of the total learning of the lists in positions 1, 7, 13, 19, and 25 of a practice series and in the corresponding positions of a succeeding continuous work series are substantially the same with the single exception of those of the list in position 1 of the practice series. The latter has lower values at the 25- and 50-percentile points than do any of the others. The importance of this generalization that the percentages learned in given fractions of the total learning period remain a constant through the diversities present in the repeated learning of similar lists is great. The present paper reports a test of this generalization with the data of two different experiments, together with a study of certain other aspects of the curve of memorization.

EXPERIMENT I

Method. The first data to be discussed are those of an experiment on the variation in retroactive inhibition with varying time-interval.[3] Six groups of 20 college students each learned, by the anticipation method, lists of 10 two-syllable adjectives exposed at a 2-sec. rate on an electrically driven drum. Each list included an eleventh adjective which served as a cue and which was not learned. Learning was to a criterion of 3 successive perfect trials. A complete record was

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[1] C. L. Kjerstad, The form of the learning curves for memory, *Psychol. Monog.*, 26, 1919, (no. 116), 1-89.

[2] G. C. White, The form of the curve of memorizing, *J. Exper. Psychol.*, 15, 1932, 184-194.

[3] This experiment was assisted by a Grant-in-Aid from the National Research Council.

taken of the correct anticipations or of the failures to anticipate at each position and during each trial.[4] A quantitative picture of the course of learning from the first trial to the criterion is thus provided.

At the beginning of the experiment, immediately following careful verbal instructions, every *S* learned two lists at a single sitting. A 5-min. interval elapsed between the completion of the first and the beginning of the second. The learning of these two lists constituted a practice sitting for the *S*s, all of whom were hitherto entirely unacquainted with learning by the anticipation method. In the main experiment each of the 6 groups learned under either a rest or a work condition and each learned a total of 5 lists to the criterion and 5 other lists to 10 repetitions each. Under the work conditions the 10 repetitions constituted interpolated learning while in the rest condition they were taken after the original list had been relearned in order to keep the amount of practice the same under both rest and work conditions. Groups A, B, and C[5] learned under the rest condition, and Groups D, E, and F under the work condition. In the intervals between learning and relearning the groups working under the rest condition did no more learning of this kind; Groups D and E learned the interpolated list immediately following the completion of the original list to the criterion; and Group F had the interpolated list just prior to the relearning of the original. In all conditions the 5 intervals used were 20 min., 1, 24, 48, and 144 hr. The total period covered by the experiment was thus about 2 weeks.

The 5 conditions were gone through in an order which counterbalanced the factors of both practice and list for the conditions. When the conditions were ordered in serial position from first to fifth, regardless of interval, the lists were so arranged that half of the *S*s took them in one order and the other half reversed the order of each successive pair. Under the practice conditions all *S*s learned the same lists in the same order. All of the lists used are of homogeneous material and are virtually equivalent in difficulty and there is small reason to infer that the factor of the particular list has had any disturbing influence upon the conclusions.

One *E* was in charge of Groups A and E, another was in charge of Groups C and D, while the two other groups each had a different *E*. The 4 *E*s, in addition to a training course in experimental psychology, had been given careful training by the writer in the use of the anticipation method and in the precautions necessary to the experimental conditions employed, and their work was under careful supervision throughout. Conditions of lighting and of the general environment were kept constant and vigorous effort was made to maintain as rigid a constancy of conditions as is possible when human beings are the experimental *S*s.

Treatment of data. In combining his individual curves Kjerstad employed the Vincent technique[6] whereby amounts learned in successive equal fractions of the total learning time are combined, and the same method has been used with the present data. The memorization curves for every *S* and for every list were first drawn by plotting trials to the criterion of 3 perfect in succession on the abscissa and number of correct anticipations on the ordinate. The data for every curve were plotted on a proportional scale such that each had an abscissa 200 mm. in length. This was aided by tables showing the exact divisions of the abscissa for each number of trials from 10 to 100.[7] When all curves are plotted on abscissas of equal length, it is a simple matter to divide them into any desired fraction. The values for equal fractions of the total number of trials may then be added and a composite Vincent curve drawn from the pooled data.

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[4] These records were taken on special forms devised by Dr. Arthur W. Melton of Yale University and printed by the Printing Office of the Yale Press.

[5] Group C, working under the rest condition, did not learn the five balance or post-relearning lists to 10 repetitions each, but otherwise the conditions were exactly the same as for the two other groups working under these conditions. The Vincent Curve values show no sign of having been affected by this difference and Group C has, therefore, been included without separate treatment.

[6] S. B. Vincent, The function of the vibrissae in the behavior of the white rat, *Behav. Monog.*, 1, 1912, 1-81.

This method reduces to a common base a large amount of highly variable data and makes possible a general interpretation which would be impossible if one dealt only with the individual curves. It has the disadvantage that it masks the individual characteristics of the data and provides, in the composite curve, a result some distance removed from the raw individual phenomena. All measures of central tendency do this, however, and the Vincent procedure yields a distortion which is no greater than that of any measure of central tendency. If, moreover, one is always careful to compare the features of the composite curve with those of the individual curves, there is slight danger of drawing conclusions which are only artifacts of averaging.

Results. Any examination of Kjerstad's conclusion that the form of the curve of memorization remains constant as the number of trials taken to learn decreases must first make certain that the decrease in trials has actually occurred in the data to be examined. More complete data upon the changes accompanying practice at successive lists of this material under the three work conditions and one rest condition (Group A) are to be published separately. A brief summary of the relevant facts will suffice for the present purpose. The mean number of trials taken by the 80 *S*s of Groups A, D, E, and F to learn the first practice list to a criterion of 3 perfect trials is 35.4, while the mean number required to learn the last of the 7 major lists is 11.5. This is a percentage decrease of 67.5, and the corresponding percentages for Group B and C are 59 and 62. These decreases in the numbers of trials required to learn are clearly large enough to give an opportunity for changes in the form of the curve of memorization to appear if such changes are correlated with the effects of practice. The data make possible, therefore, a test of Kjerstad's conclusion.

Kjerstad divided his curves into halves, thirds, and sixths and failed to find with any of these divisions any uniform suggestion of a change of form with practice. The curves yielded by the present experiment will be presented first in halves, for the purpose of direct comparison with Kjerstad's. The percentages learned by each group in the first half of the learning period are shown for the first practice list and for the final list, together with sigmas of the means, in Table I. Only the data for the first halves are given, since they give an adequate picture of the facts and since the data for the second halves can, if desired, be obtained subtractively.

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[7] I am greatly indebted to Dr. Arthur W. Melton of Yale University who prepared these tables and who very kindly provided me with copies.

TABLE I

Percentages Learned in the First Halves of the Learning Period

Lists	Rest Groups			Work Groups		
	A	B	C	D	E	F
Practice	54.7±4.9	48.7±4.3	60.9±4.5	59.2±5.3	64.8±4.2	62.7±3.2
Final	77.0±2.4	76.5±2.3	75.2±3.1	72.8±4.5	75.4±3.7	78.3±2.8

The 6 groups show without exception a clear increase with practice in the amount learned during the first half of the learning period, and, hence, a definite shift toward a more pronounced negative acceleration of the curve of memorization. These data fail, therefore, to verify Kjerstad's conclusion that the curve is a constant in form regardless of the stage of practice. The average change for the 6 groups amounts to 17.3%, which although small in comparison with the 59% to 67% decrease in trials required to learn, represents a considerable change in the form of the curve. In view of the consistency of the data, it is scarcely necessary to present the sigmas of the differences in order to validate the conclusion. These sigmas may readily be found, if desired, from the data given. Not all differences are three times sigma (those for Groups C, D, and E are not), but the chances are 97 in 100 that the least reliable difference (Group E) is greater than zero, and this together with the complete consistency of the 6 groups permits confident acceptance of the conclusion that practice affects the form of the curve.

It will be noticed that the percentages of the practice list learned in the first half of the period vary considerably among the 6 groups and that the corresponding percentages of the final list vary much less. Moreover, the sigmas of the means of the final lists are considerably smaller. A second effect of practice is, thus, to render the Vincent curve-values of the individual groups less variable and the several means more nearly the same in magnitude.

This analysis of the curves in terms of halves is at best a rough procedure and can give only a general picture of the actual situation. Divisions into thirds would be only slightly better and a finer analysis than even sixths can provide is desirable. In order to arrive in greater detail at the influence of practice upon the form of the curve, the individual curves have been divided into tenths and a composite curve drawn for each group to show the cumulative percentages of the total list learned in successive tenths of the total learning period. The curves of the 120 *S*s have then been pooled to give a single curve at each of the 10 points in the course of practice; and the data for such a composite curve are presented in Table II. To present the data of the individual groups would necessitate cumbersome tables and would add little to the briefer presentation of Table II which gives, in addition to the mean values of the practice and the final lists, the mean deviation of the 6 groups from the mean of all. A comparison of the two columns of means shows that the final list has at every possible tenth a higher mean than has the practice list. The differences are largest during the first half of the learning period and decline thereafter. All save that at Position 9 are statistically reliable. The curves of the 6 separate groups agree almost perfectly with these characteristics of the composite curve. At only 3 points out of 54 is a value for the final list lower than the corresponding one for the practice list and then by an insignificant amount and at the 8th or 9th division of the curve. All groups agree in showing the largest differences during the first half of the learning period. It must be concluded that the amount of practice which has intervened between the first and the last lists has definitely increased the initial spurt and sharpness of the negative acceleration of the memorization curve and has uniformly affected the magnitude of the values at each succeeding tenth. The influence of practice is greatest upon the first half of the curve, *i.e.* upon the amount learned during the first half of the learning period.

TABLE II

Vincent Curve-Values for First Practice and Final Lists
(N is 120 at each tenth)

Tenth	Mean	σM	V	Group A.D. (6 Groups)	Mean	σM	V	Group A.D. (6 Groups)
1	17.0	1.15	74.4	2.8	30.6	1.47	52.6	4.6
2	32.0	1.57	53.7	4.6	47.3	1.43	32.9	5.6
3	41.9	1.86	48.4	8.1	59.7	1.74	31.8	3.9
4	54.0	1.80	36.4	4.2	68.1	1.54	22.1	3.0
5	58.3	1.89	32.4	4.5	75.9	1.43	18.5	1.4
6	67.9	1.56	25.0	2.7	78.6	1.45	20.1	2.0
7	72.6	1.57	23.6	2.1	83.0	1.21	19.0	1.9
8	74.9	1.78	25.7	3.5	83.6	1.28	16.7	2.7
9	81.2	1.47	19.8	3.6	83.6	1.20	15.6	2.2
10	100.0	0	0	0	100.0	0	0	0

The sigmas of the means are, even in the early part of the learning period, relatively small and these mean values may be considered to have a high degree of stability. The relative variability of each list at each tenth decreases almost uniformly from a high point at the initial tenth to a low one at the last tenth, the 9th, at which variability is possible. At every point the variability of the final list is less than that of the first practice list. Practice has uniformly and in considerable amounts decreased the variability of the Vincent curve-values.

The largest single decrease in the number of trials required for mastery occurs from the first to the second practice list and it is necessary to inquire whether there has been a comparable change in the values of the curve. These are, at successive tenths of the learning of Practice List B: 21.0; 36.6; 52.8; 63.4; 70.5; 75.6; 78.9; 84.3; 82.3; and 100.0. A comparison of these with the figures of Table II will show that the curve for List B is higher at each tenth than is that for List A and that the gains made by the former are greatest over tenths 3 to 8. The curve for the final list is higher, except at the 8th tenth, than is the curve for List B. The shift with practice is, thus, relatively rapid at first but extends beyond the initial practice list.

The relative variability of the means of the 6 groups from the mean of all is nowhere large and decreases irregularly as learning proceeds. Computation of the relative inter-group variability (100 A.D./M) would show that at every point the variability of the final list is smaller than that of the practice list and that at many points the difference is considerable. The conclusion reached from examination of the percentages learned in the first halves of the learning period that the effect of practice is to reduce the inter-group variability of mean Vincent curve-values or to render the mean values of different samplings more stable is, therefore, reënforced.

The problem of the form of the curves of acquisition during the very early stages of the process has been for some time both active and unsettled,[8] and it will be significant to inquire what form these curves have near the point of origin. Division into tenths has yielded a composite curve with a general negative acceleration. In the case of the practice lists, however, which require a mean of about 35 trials for mastery, each tenth includes 3.5 trials, on the average, and thus the form of the curve in the first few trials is obscured. The initial form may be seen more clearly if a composite is constructed from more minute divisions of each individual curve. Such a composite curve has been drawn in fortieths. Inasmuch as we are primarily concerned here with the early portion of the curve and as the general form of its later portion can be seen from Table II, the composite will be reproduced for the first ten fortieths only (Fig. 1). The early part of the curve of the practice list is virtually linear, while the corresponding portion of the final list has an early initial spurt and subsequent negative acceleration. The

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[8] Cf. E. Culler, Nature of the learning curve, *Psychol. Bull.*, 25, 1928, 143-144.

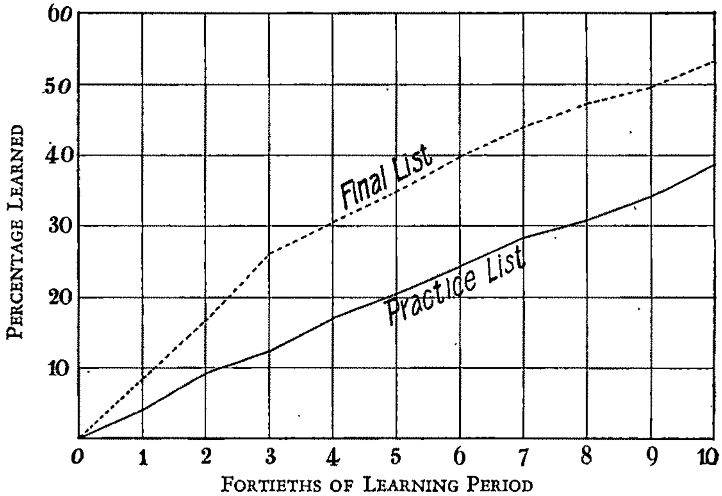

FIG. 1. VINCENT CURVES FOR THE FIRST 10 FORTIETHS OF THE TOTAL LEARNING PERIOD
(Adjectives)

conclusions already drawn regarding the influence of practice upon the form of the curve are reënforced by the data of this figure.

At this point another feature of Table II and of the curves of its constituent groups should be mentioned. Both practice and final lists show in every group the clear end-spurt which Peterson[9] and White have reported.[10] The magnitude of the spurt is only slightly reduced by practice.

EXPERIMENT II

Method. In this experiment 12 college students, none of whom was familiar with either the anticipation method or with any experimental work on memorizing, learned lists of 8 syllables from the Glaze 0% group[11] under the same conditions as those employed in Experiment I. On each of 3 practice days one list was learned to a criterion of 3 successive perfect trials. This was followed by the main experiment in which they learned, either partially or completely, under conditions described elsewhere,[12] 22 lists of syllables of varying associative value. On the day following the completion of this experiment, they learned another list of 0% associative value. The order in which these 0% lists were learned was counterbalanced to give all lists equal average position. The data have been treated in the same way as were those of Experiment I.

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[9] G. M. Peterson, Negative acceleration with material of varying difficulty, *J. Exper. Psychol.*, 11, 1928, 40-44.

[10] White, *op. cit.*

[11] J. A. Glaze, The association value of non-sense syllables, *Ped. Sem.*, 35, 1928, 255-267.

[12] J. A. McGeoch, The influence of degree of interpolated learning upon retroactive inhibition, this JOURNAL, 44, 1932, 695-708.

Results. The percentages learned in the first half of the learning period at the four positions in order are: 55.3; 60.0; 66.5; and 67.9. This regular increase with practice supports the conclusions of Experiment I.[13] This increase is seen more clearly, however, when the composite curves are drawn to represent the amounts learned at successive tenths of the learning period. The numerical values of these curves (Table III) show a clear tendency to increase in magnitude, especially in the first half of the learning period, during the three practice days. There is, also, a discernible but less regular tendency for the values in the second half of the period to increase. After the intervening practice at 22 mixed lists, the amount learned in the initial tenth is larger, as is that learned by the end of every tenth except the second. The large amount of practice has been followed by no great changes in the form of the curve, but there is an unmistakable shift toward a larger initial spurt with the resulting augmentation of the later portion of the curve.[14]

TABLE III

CUMULATIVE PERCENTAGES OF PRACTICE AND FINAL LISTS LEARNED IN SUCCESSIVE TENTHS OF THE TOTAL LEARNING PERIOD

Tenth	Position of List			
	Practice			Final
	1	2	3	1
1	13.6	15.5	25.6	30.7
2	29.6	39.6	44.3	43.2
3	41.2	35.6	51.0	53.6
4	43.7	52.1	53.6	66.0
5	55.3	60.0	65.2	67.8
6	75.6	70.0	70.2	77.2
7	69.8	64.0	74.0	85.0
8	72.5	78.3	80.1	81.2
9	83.6	79.3	86.5	87.0
10	100.0	100.0	100.0	100.0

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[13] The mean numbers of trials required to learn these lists to a criterion of three perfect trials in succession reduced from 60.8 at the first practice position to 22.4 at the position immediately following the main experiment. This large reduction gives ample opportunity for changes in the curve of memorization to occur if practice is accompanied by such changes.

[14] To apply statistical criteria to the means and differences of this experiment is, in view of the small number of *Ss*, scarcely fruitful. The uniformity of the shift in the Vincent curve-values is, however, sufficient guarantee of the trustworthiness of the general tendency. On the face of the results there is an inversion at the 6th tenth in the first two practice lists, none in the third, and one at the 7th tenth in the final list. Whether a plateau phenomenon, which shifts toward the end of the list or disappears entirely with practice, is indicated is hard to say with any certainty.

It should be noticed that in all of these curves the end-spurt phenomenon clearly appears, and that this end-spurt is slightly reduced by practice.

A comparison of the values of the two experiments shows that they are, in the practice position, roughly the same at corresponding tenths and that, likewise, there is a rough equivalence in the final list. If anything, the adjectives show a slightly greater initial spurt than do the syllables and, with approximately half the amount of practice in terms of lists learned, as much gain.

The form of the curve in the early part of the total learning period has been examined by the method employed in Experiment I, and Fig. 2 shows the first 10 fortieths of the Vincent curves for the first practice and the final list. The curve for the former has a slight initial positive acceleration and would, if completely drawn, be S-shaped. Judged by the number of trials required to learn, this list of syllables was much more difficult than was the first list of adjectives in the preceding experiment and the greater tendency toward S-ness under these conditions makes it appear that, with naïve *S*s and with increasing difficulty of material, the curve may be expected definitely to assume this form.[15] Practice, however, produces a change to an initial spurt and negative acceleration, as the curve for the final list demonstrates.

DISCUSSION

Both experiments have shown a change with practice in the amounts learned at the end of successive tenths of the total learning period and that, particularly, increasingly large percentages are learned in the first few tenths of the period as practice continues. This differs sharply from Kjerstad's conclusion that the percentages learned in corresponding halves, thirds, and sixths of the learning period are unaffected by practice. It is, however, questionable whether the present experiments should be construed as contradicting Kjerstad's in any fundamental sense, on account of certain differences in experimental method and in treatment of data.

There are several marked differences between the materials and procedures of Kjerstad's study and those of the present experiments. With one exception he employed paired associates with a test in writing after each exposure and a 3-min. intermission between the test and the succeeding exposure. The single exception consisted of serial nonsense syllables, but these were presented at a 3-sec. rate and a written reproduction was attempted after each complete presentation. The differences which are obvious between the methods of the two experiments have doubtless been responsible for some of the differences in the results. Certainly the longer exposure interval and the written reproduction would operate to hasten the attainment of a practice level, but in the absence of specific knowledge regarding the character of this influence it is hardly worth while to do more than call attention to its presence. It should be noted, also, that for the purposes of any precise analysis of the curve Kjerstad's methods of measuring progress are rather rough. About half of his *S*s, moreover, were graduate students in psychology whose knowledge of the subject, training in experimental work, and probable experience at memorizing may have brought them to a somewhat advanced stage of practice before the beginning of the experiment.

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[15] Cf. the conclusions reached by Culler, *op. cit.*

An important consideration in accounting for the apparently contradictory results lies in the number of lists represented in Kjerstad's comparisons. He does not compare the curves obtained at the beginning of practice with those drawn at some later stage. His relatively few *S*s each learned large numbers of lists of each material and in studying the effect of practice upon Vincent curve-values he compared the average percentages reproduced at halves, thirds, or sixths of the total period during the learning of the first half of the lists learned with those reproduced correspondingly during the learning of the second half of the lists. This comparison of the pooled values of fairly large numbers of lists necessarily obscures any change which

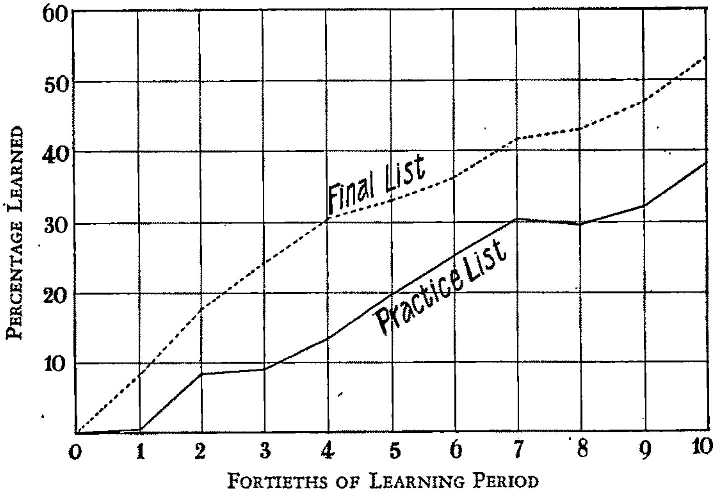

Fig. 2. Vincent Curves for the First 10 Fortieths of the Total Learning Period
(Nonsense syllables)

may have occurred during the learning of the first few lists since these enter into the composite which includes more than the first two or three lists. The present results make it seem probable that practice effects diminish rapidly with continued learning and it is a reasonable inference that his averaged results masked these early changes. In this paper the results which have been pooled were all from an equivalent stage of practice and comparisons between the first list learned and one learned after intervening practice are designed to reveal changes which Kjerstad's method would conceal. Shifts as large as those reported here, however, had they occurred in his results, should have operated to reduce somewhat the means for the first halves of his lists. That such a reduction is not visible may mean that the factor under discussion is less potent to account for differences than has been suggested or it may be due to differences in experimental method and in *Ss*. It is at least a reasonable hypothesis that the latter is the case.

The present results are also, on the surface, at variance with those of White, but the contradiction is more apparent than real. His results show smaller percentages during the first half of the period of the first list learned in the practice series. The next sample taken is from the 7th position and here the values at the 25- and 50-percentile points are higher. Samplings after further practice show no additional gains. These results do not mean that there is no shift in Vincent curve-points with practice, but rather that the shift is over by the time the 7th list has been reached, at least in so far as an analysis by quartiles will reveal it. This finding that the curve is unchanged by continued practice after the first 7 lists accounts for the fact that the final list in Experiment II has values which are only slightly larger than those of the third practice list.

The question at once arises whether the results of the present experiment imply any modification in the interpretation of Kjerstad's other conclusions. It will be best to confine discussion of this question to the single case of the influence of the length of the list upon the form of the curve of memorization, since Kjerstad's conclusion that the latter is independent of the former has been confirmed by Robinson and Heron[16] and by Robinson and Darrow.[17] The lists employed in the present experiments have been confined in length to 10 adjectives and 8 nonsense syllables. The percentages learned in successive tenths of the learning period are, however, much the same for the two at the initial and final stages of practice, although the values for the second and third practice lists deviate somewhat from these percentages. While no direct conclusions can be drawn from these small differences in length of list regarding the influence of this factor, there is nothing in the results to indicate that the generalization stated above is in any way contradicted. It is, however, a probable inference that one modifying clause should be added to the effect that the generalization holds within or at any given stage of practice. The addition of this clause assumes that the curves for lists of different lengths would show comparable changes with practice, but this assumption seems readily justifiable. The Kjerstad-Robinson law, as this generalization may be called, would then read: the form of the Vincent curve of memorization at any given stage or condition of practice is independent of the length of the list.[18]

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[16] E. S. Robinson and W. T. Heron, Result of variations in length of memorized material, *J. Exper. Psychol.*, 5, 1922, 428-448.

[17] E. S. Robinson and C. W. Darrow, Effect of length of list upon memory for numbers, this JOURNAL, 35, 1924, 235-243.

It is worth emphasizing that the 6 equal samplings of Experiment I yield results which show a relatively high degree of uniformity. This finding of relative constancy in results from small populations is one effective answer to the criticism that conclusions from laboratory work done with small numbers of *S*s cannot be generalized. No one, presumably, has supposed that such results could be generalized for all possible *S*s, but only that they were valid for all *S*s of the same kind and under the same conditions, an assumption which is strongly supported by these data.

CONCLUSIONS

(1) Practice at memorizing lists of adjectives and of nonsense syllables by the anticipation method and under the other conditions of these experiments is consistently accompanied by changes in the percentages learned in successive fractions of the total learning period. The more important of these changes are stated in the following generalizations. (a) Practice increases by considerable amounts the percentages learned in the first half of the total learning period and particularly in the early tenths. The slope and extent of the initial spurt and the sharpness of the subsequent inflection are, thus, increased. The curve for nonsense syllables is shifted with practice from a slight initial positive acceleration to an initial spurt and succeeding negative acceleration. (b) Practice also decreases the variability of the percentages learned in successive fractions of the total period and renders more uniform the means of different samplings of *S*s.

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[18] In the experiments by Kjerstad, Robinson, Heron and Darrow the factor of length of list was studied with pooled data in which all stages of practice were averaged and in which, therefore, there was no opportunity for the effect of practice to become apparent. Under these conditions the law is a generalized one for a composite of all stages of practice involved in the experiment.

(2) It is suggested, in the light of these results, that a clause should be added to the Kjerstad-Robinson law which would modify it to read thus: *the form of the Vincent curve of memorization at any given stage or condition of practice is independent of the length of the list.*

(3) The curve of memorization for lists of nonsense syllables of 0% associative value shows, at the first practice position, an initial positive acceleration and the total curve is S-shaped. With practice, as has already been stated, early negative acceleration supervenes, but the final spurt phenomenon, or final positive acceleration remains. The corresponding curve for adjectives has an early linearity. These facts support the conclusion that with unpracticed *S*s and difficult materials the curve of memorization will tend to be S-shaped.

A PSYCHOPHYSICAL STUDY OF VISUAL GESTALTEN

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There has been relatively little psychophysical work done from the point of view of Gestalt psychology. The configurationists approach the problems of relational judgments quite differently from the psychophysicians.[1] The application of the methods of psychophysics to the problems of Gestalt may prove difficult, but there appears to be no *a priori* reason why an attempt should not be made.

The present paper marks an attempt to apply the constant method to one of the outstanding problems of Gestalt psychology, *i.e.* to the study of visual *Gestalten.*

The particular problem that we have undertaken is to discover whether a change in the configurational relationship would noticeably affect the thresholds for the judgment of the separation of two visually presented bars. In one case the judgment was to be made within a spontaneous dominant configuration and in the other it was to be made between two such spontaneous dominant configurations which apparently were in opposition to the particular configuration required for the judgment. The configurational membership was determined by the color of the bars, red and blue being used in two arrangements.

METHOD

Apparatus. Fourteen comparison-cards and one standard card were successively presented by means of a Whipple Disk Tachistoscope for an exposure-time of 75σ. This duration was adequate for a clear impression but too brief for exploratory eye movements.[2] Except for the first few hours the complete experiment was conducted in a dark room.

The 14 Comparison-Cards were divided into two series (A and B) of 7 cards each. The Standard Card was common for the two series. The cards were made of heavy white cardboard, 180 mm. wide and 100 mm. high, on which were pasted vertical bars 4 mm. wide and 50 mm. high.

The Standard Card contained two black parallel bars with a separation of 9 mm. The comparison-cards contained four parallel vertical bars, the separation between the two outer pairs of bars remained constant at 9 mm., while the separation between the two middle bars varied. The various separations of the middle bars were the same for Series A and B: 7½, 8, 8½, 9, 9½, 10, and 10½ mm.

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* Accepted for publication April 6, 1932. This study was suggested and directed by Professor S. W. Fernberger.

[1] H. Helson, The psychology of gestalt, this JOURNAL, 37, 1926, 37.

[2] G. M. Whipple, *Manual of Mental and Physical Tests,* 1910, 227.

Series A and B differed only in the pattern formed by the color of the bars. Series A contained two red and two blue bars, the red to the left and the blue to the right. Series B contained three red bars and one blue bar, the blue being to the extreme right. The judgments required were of the relative separation of the two central bars. In Series A the judgments were between the inner bars of the two color pairs while in Series B the judgments were between the two center bars of the red group. In Series A the distances to be judged lay between two spontaneous configurations, while in Series B they lay within a spontaneous configuration formed by the color of the bars.

Procedure. *O* sat in a comfortable chair 7 ft. in front of the tachistoscope. The chair was of a height to bring his eyes on a level with the exposure-field. In order to escape the undesired effects of fatigue, *O* was permitted to rest whenever he desired. Whether he had availed himself of this privilege or not, a 5-min. rest period was given him at about the middle of every experimental hour.

The cards of the two series (A and B) were intermixed, and presented haphazardly. In order to minimize the influence of the serial arrangement, 10 different haphazard orders were employed during the course of the study. But one time order was used in presenting the cards. The standard was always presented first, and then the variable. The comparisons were made as rapidly as possible. After some practice *E* was able to give the *O*s 14 comparisons on an average, in a 5-min. work period —approximately 1 every 20 sec.

The *O*s were permitted to write down their casual introspections at the end of every experimental hour.

Observers. The *O*s,[3] 5 in number, where Dr. Stanley F. Cooper (*C*), Dr. John. F. Humes (*H*), Dr. Francis W. Irwin (*I*), members of the Departmental Staff, and Mr. Wendell R. Carlson (*K*), and Mr. Charles C. Limburg (*L*), graduate students. All possessed normal or normally adjusted vision. Three of them (*C*, *H*, and *I*) were acquainted with tachistoscopic procedure while for 2 (*K* and *L*) it was new. All were unacquainted with the details of the experiment except those given in the instructions and apparent in the procedure. Each served for a period of about an hour, on 2 days a week, for 15 weeks, extending from February to June, 1931.

Every *O* gave a total of 3,500 judgments: 250 on each of the 14 comparison-cards —25 for each of the 10 haphazard orders.

Instructions. The following instructions were read at the beginning of each experimental session.

> I am going to show you a number of cards. First I shall show you a standard card of two bars, then a comparison-card of four bars, then another standard card and another comparison-card and so on. I want you in each case to pay careful attention to the standard card and then give me your judgment for the following comparison-card. The standard card may vary as well as 'the comparison-cards. I want you to judge whether the central bars of the comparison-card are the same distance apart as the bars of the standard or whether the distance apart is greater or less. After each comparison-card you will give me your judgment, merely saying the words 'less,' 'equal,' or 'greater.' Be sure in each case to pay careful attention to the standard card so that you may give a true comparison judgment. Before presenting a standard card I will say the words 'standard' and 'now' with a short pause between. Before presenting a comparison-card I will say the words 'ready' and 'now' with a short

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[3] I wish to express my thanks to the *O*s for their assistance and coöperation.

pause between. I want your judgments on the comparison-cards to be immediate. Just say 'less,' 'equal,' or 'greater.'

RESULTS

For every *O*, 25 judgments were obtained on the 14 comparison stimuli for each of the 10 haphazard orders of presentation. Consecutive fractions were paired for purposes of calculation. From these 5 combined fractions, measures were computed separately for Series A and B in accordance with the $\Phi(\gamma)$ hypothesis making use of Urban's tables.[4]

These computed measures are shown in Tables I to X, all of which are similar. There are 2 tables for every *O*. The odd numbered tables show results for Series A, the even for Series B. In the first column of each of the 10 tables is shown the combined consecutive fractions for which the meas-

TABLE I

C's RESULTS IN SERIES A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.83	1.19	8.46	9.41	0.95	9.02
3 & 4	0.91	1.02	8.45	9.55	1.11	9.03
5 & 6	1.07	1.17	8.17	9.30	1.13	8.76
7 & 8	0.82	1.09	8.16	9.33	1.17	8.83
9 & 10	1.20	1.18	8.28	9.27	0.99	8.77
Av.	0.97	1.13	8.30	9.37	1.07	8.88

TABLE II

C's RESULTS IN SERIES B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.80	0.89	8.59	9.45	0.86	9.04
3 & 4	0.81	1.15	8.46	9.43	0.97	9.03
5 & 6	0.88	1.13	8.18	9.26	1.09	8.79
7 & 8	1.34	1.18	8.07	9.30	1.23	8.65
9 & 10	1.02	1.14	8.23	9.36	1.13	8.83
Av.	0.97	1.10	8.31	9.36	1.06	8.87

TABLE III

H's RESULTS IN SERIES A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.57	0.55	8.66	9.23	0.56	8.94
3 & 4	0.71	0.69	8.76	9.02	0.26	8.89
5 & 6	0.65	0.62	8.93	9.15	0.22	9.04
7 & 8	0.52	0.52	8.89	9.25	0.36	9.07
9 & 10	0.77	0.78	8.92	9.07	0.15	8.99
Av.	0.64	0.63	8.83	9.14	0.31	8.99

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[4] See S. W. Fernberger, A simplification of the practice of the method of constant stimuli, this JOURNAL 25, 1914, 121-130. Also G. J. Rich, A checking table for the method of constant stimuli, *ibid.*, 29, 1918, 120-121.

TABLE IV

H's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.50	0.54	8.54	9.17	0.63	8.86
3 & 4	0.67	0.63	8.64	8.94	0.31	8.78
5 & 6	0.60	0.59	8.76	8.98	0.22	8.87
7 & 8	0.53	0.50	8.63	9.01	0.39	8.81
9 & 10	0.68	0.75	8.91	9.13	0.23	9.03
Av.	0.60	0.60	8.70	9.05	0.36	8.87

TABLE V

I's Results in Series A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.25	1.28	8.56	9.36	0.80	8.97
3 & 4	1.46	0.88	8.56	9.15	0.59	8.78
5 & 6	1.87	1.19	8.51	9.36	0.85	8.84
7 & 8	1.53	0.91	8.59	9.42	0.84	8.90
9 & 10	1.87	1.47	8.69	9.46	0.77	9.03
Av.	1.60	1.15	8.58	9.35	0.77	8.90

TABLE VI

I's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.11	1.30	8.51	9.18	0.67	8.87
3 & 4	1.44	1.17	8.47	9.10	0.62	8.75
5 & 6	2.14	1.31	8.43	9.33	0.90	8.77
7 & 8	1.06	1.13	8.61	9.43	0.82	9.03
9 & 10	1.39	1.56	8.72	9.55	0.83	9.16
Av.	1.43	1.29	8.55	9.32	0.77	8.92

TABLE VII

K's Results in Series A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.19	1.20	8.53	9.56	1.03	9.05
3 & 4	1.60	1.05	8.64	9.80	1.16	9.10
5 & 6	1.29	1.09	8.70	10.02	1.31	9.30
7 & 8	1.08	1.31	8.81	10.16	1.35	9.55
9 & 10	1.35	1.43	8.70	9.87	1.17	9.30
Av.	1.30	1.22	8.68	9.88	1.20	9.26

TABLE VIII

K's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	0.94	1.82	8.45	9.39	0.94	9.07
3 & 4	1.35	1.60	8.45	9.63	1.18	9.09
5 & 6	1.69	1.54	8.56	9.86	1.29	9.18
7 & 8	1.22	1.91	8.55	9.92	1.37	9.39
9 & 10	1.30	1.13	8.68	9.99	1.31	9.29
Av.	1.30	1.60	8.54	9.76	1.22	9.20

TABLE IX

L's Results in Series A

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.17	0.94	8.27	9.83	1.56	8.97
3 & 4	1.25	1.15	8.33	9.88	1.54	9.07
5 & 6	0.84	1.11	8.26	10.17	1.90	9.34
7 & 8	0.90	1.08	8.04	10.14	2.10	9.18
9 & 10	1.31	0.86	8.07	9.90	1.83	8.80
Av.	1.09	1.03	8.19	9.98	1.79	9.07

TABLE X

L's Results in Series B

Order	h_1	h_2	S_1	S_2	I.U.	P.S.E.
1 & 2	1.01	1.06	8.25	9.67	1.43	8.98
3 & 4	1.27	1.15	8.23	9.61	1.38	8.88
5 & 6	1.34	1.20	8.10	9.77	1.67	8.89
7 & 8	1.05	1.16	7.92	9.88	1.96	8.95
9 & 10	1.33	1.07	8.17	9.84	1.68	8.91
Av.	1.20	1.13	8.13	9.75	1.62	8.92

ures were calculated. In the second and third columns are shown the Coefficients of Precision for the 'Less' and 'Greater' judgments, h_1 and h_2 respectively. In the fourth and fifth columns are shown the thresholds for the 'Less' and 'Greater' judgments, S_1 and S_2 respectively. The sixth column shows the values of the Interval of Uncertainty, and the seventh column shows the Point of Subjective Equality.[5] The average for each of the measures is shown in the bottom row of the table.

In Figs. 1 to 5 are shown straight line graphs of the judgments of every *O* for the entire experiment obtained by combining the judgments for all 10 fractions. The curves for the 'Less,' 'Equal,' and 'Greater' judgments are readily distinguishable. It will be noted that the curves are relatively of the normal $\Phi(\gamma)$ type. The values for Series A are connected by continuous lines, while those for Series B are connected by broken lines. It will be noted that there is little difference between the results of the two series. The curves for *C, H,* and *I* practically coincide in the two series, and those for *K* and *L* are very like.

The Intervals of Uncertainty are the most crucial values for the purposes of our experiment as they are the chief measures of the accuracy of judgment. Let us consider them in more detail. Table XI gives these values with their Probable Errors for all the *O*s for each series, together with their dif-

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[5] For an explanation of these measures see S. W. Fernberger, On the relations of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.,* 14, 1913, (no. 61), 29-49.

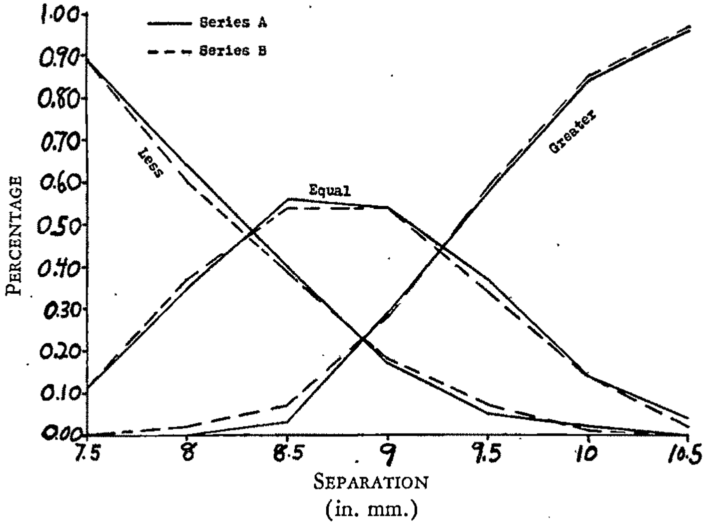

FIG. 1. CURVES SHOWING *C*'S RESULTS

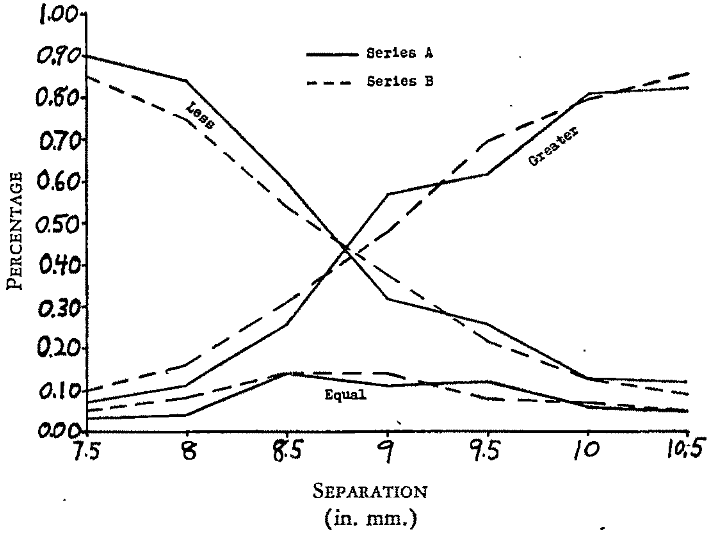

FIG. 2. CURVES SHOWING *H*'S RESULTS

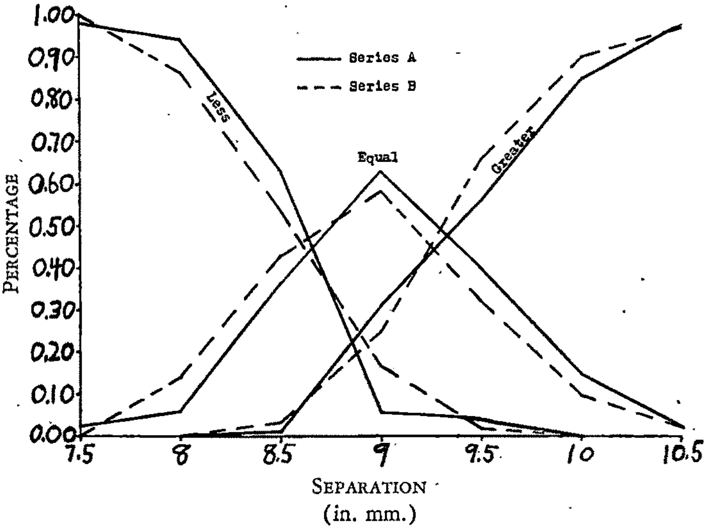

FIG. 3. CURVES SHOWING *I*'S RESULTS

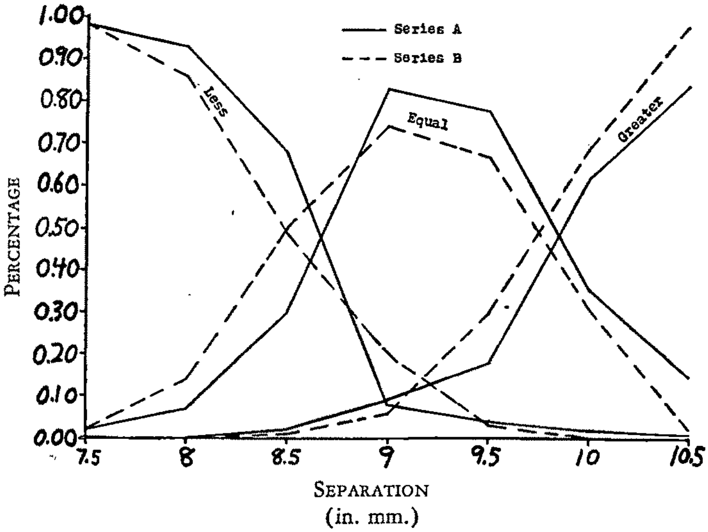

FIG. 4. CURVES SHOWING *K*'S RESULTS

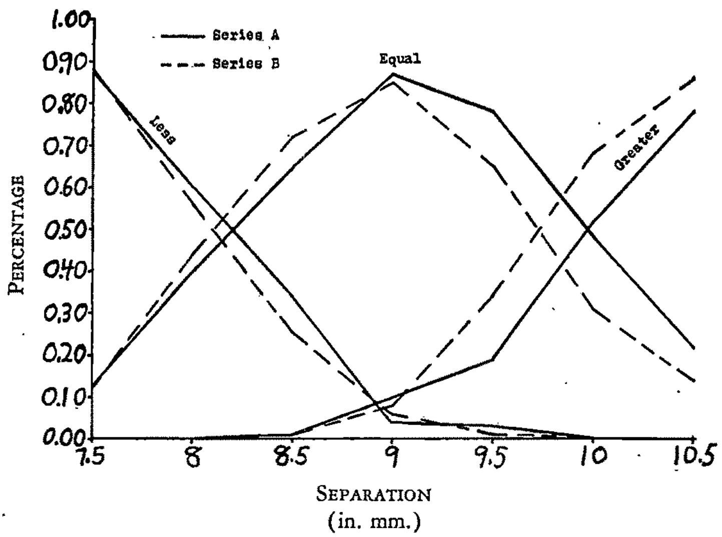

FIG. 5. CURVES SHOWING *L*'S RESULTS

ference and an index of the significance of that difference.[6] One of the *O*'s, (*I*), shows no difference between the intervals of uncertainty, and two of them (*C* and *L*) show a difference in one direction, and two (*H* and *K*) show a difference in the opposite direction. The differences are all small and the indexes of reliability are likewise small ranging from 0.1031 for *C* to 0.7485 for *L*. These values, even *L*'s the largest, are too low to indicate any real significance. Such difference as we found must be regarded, therefore, as being due to chance.

Not only do the size and direction of the average interval of uncertainty vary among the different *O*s, but they also vary in the results of the in-

TABLE XI

COMPARISON OF THE AVERAGE INTERVALS OF UNCERTAINTY FOR EVERY *O*

	Interval of Uncertainty					
	Series A		Series B			Significance of
O	Av.	P.E.	Av.	P.E.	Diff. (A–B)	Differences
K	1.20	.0384	1.22	.0513	−0.02	.1667
C	1.07	.0283	1.06	.0432	+0.01	.1031
H	0.31	.0479	0.36	.0506	−0.05	.3699
I	0.77	—	0.77	—	0.00	—
L	1.79	.0715	1.62	.0701	+0.17	.7485

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[6] For the determination of this index see S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 141-142.

dividual *O*s. As shown in Tables I-X, where the data are fractionated for every *O* into 5 equal parts, the intervals of certainty for a given *O* are sometimes larger for Series A and sometimes for Series B. For *L* alone do the differences between comparable fractions all lie in the same direction, *i.e.* toward Series A. *H* is the next most consistent. In 4 of the 5 fractionations, Series B is the larger, and in the fifth fraction the two series are equal. The data of the other 3 *O*s, however, show considerable fluctuation. The intervals of uncertainty are larger in Series A for 3, 2, and 2 of the 5 fractionations for *C*, *I*, and *K* respectively.

From a consideration of all these facts it is evident that any conclusion as to a definite and statistically significant difference between the Intervals of Uncertainty for the two configurations would be unwarranted. This means that the two configurational arrangements in Series A and B exerted no differential effect on the ability to judge differences in the two cases under our experimental conditions.

The Points of Subjective Equality for Series A and B as shown in Tables I to X show no uniformity in their relation to actual equality. There appears to be no consistent time error, nor is the approximation to actual equality consistently closer for one series than for the other.

From a consideration of the size of the Coefficients of Precision for Series A and B, as indicated in Tables I to X, we again find no consistent difference. Also, there is no uniform tendency for the Coefficients of Precision to become larger or for the Intervals of Uncertainty to become smaller in the later fractions as compared with the earlier fractions. This would be expected if improvement due to progressive practice took place in the accuracy of judgments.

Our findings in this experiment have indicated no differential effect for the two configurations used in Series A and B. Moreover, there does not seem to have been any added effort required to judge one series as compared with the other. At least the occasional recorded introspections do not reveal such a condition. The one configurational arrangement seems to have given as accurate results and presented no more difficulty of any sort than the other.

These findings are not in accordance with the results expected at the outset of the experiment from other studies of the influence of configurations upon thresholds. We had expected that the configurational nature of Series B would facilitate judgments and that of Series A would tend to impair the accuracy of judgments. Series B, as we have pointed out, involved judgments within a dominant spontaneous configuration, while Series A involved judgments between two dominant spontaneous configurations. The organization of the two central bars required for judgments under the conditions of this experiment should be favored by the former configuration and directly opposed by the latter configuration.

CONCLUSIONS

From this experiment we are led to conclude one of three things: (1) either configurational membership exerts no influence upon the thresholds for the judgment of the separation of two objects visually presented; (2) or the conditions of our experiment were inadequate to detect such influence actually present; (3) or our interpretation of the nature of our stimuli was incorrect due to the inadequacy of color alone to materially effect configurational organization.

SUMMARY

(1) Thresholds were obtained by the method of constant stimuli for the judgment of the separation of bars in two different visual Gestalten, one of which demanded judgments within a dominant spontaneous configuration and the other of which demanded judgments between two dominant spontaneous configurations. The configurations were determined by the color of the component bars, red and blue being used in two different arrangements.

(2) A treatment of the judgments of the two configurational arrangements gave similar results. No differential effect was apparent for the two configurational arrangements for the Intervals of Uncertainty, the Points of Subjective Equality, or the Coefficients of Precision. The results for the two configurational arrangements showed no consistent or significant differences.

A METHOD OF MEASURING THE EFFECT OF PRIMACY OF REPORT IN THE RANGE OF ATTENTION EXPERIMENT

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The traditional 'range of attention' experiment consists of the flashing before the eyes of the *S*, by means of a tachistoscope, a card bearing a number of letters of the alphabet, colors, dots, forms, or lines. The flash consumes a fraction of a second, usually 100 σ. Immediately afterward *S* writes out or expresses orally the contents, in proper order, of what he believes he perceived during the flash. This act of writing or speaking or reproducing psychologists have termed the *S*'s 'report.' In order to make such a 'report,' *S* represents to himself, to his mind, by visual imagery or in some other manner, the letters or other contents prior to or during the act of writing or speaking. The latter, this act of mental representation to make possible the 'report,' psychologists have called the act of 'recall.' During the flash *S* perceived many of the exposed contents, some much more clearly than others. The different degrees of clearness in the perceptual act, have been described as an exhibition of 'attention.' For brevity's sake the 'seeing' of the contents during the brief exposure is called 'perceptual attention.' The purpose of the experiment is to measure, by measuring the number of the contents of 'recall' and 'report,' the 'range' or number of contents which were perceived with such a high degree of clearness that their 'report' was certain. This experiment involves at least three variables: (1) the act of *perceptual 'attention,'* causing certain contents to stand out dominantly over others during the brief exposure; (2) the act of *'recalling,'* through imagery and otherwise, what one 'remembers' of the contents which were flashed a moment previously; and (3) the act of *'reporting,'* in writing or spoken words, what one is 'recalling' or has just 'recalled.' The variable processes of 'recall' and 'report' are known to function with a high frequency from left to right in a majority of *S*s. Consequently if the results of 'reports' are plotted in a graphic form, say for the content positions from left to right (1 to 9, for cards carrying 9 contents arranged in a horizontal sequence) a high 'recall' or 'report' efficiency characterizes the leftmost position (position no. 1). From there on toward the right up to the 6th and 7th positions, a rapid decline of effectiveness is to be observed. The final or rightmost content will show an increased effectiveness: the curve in that region will rise somewhat instead of remaining parallel with the base line or declining further. Such a graph of the effectiveness of the 'recalls' or 'reports' for the various letter-positions from left to right, is very similar to the typical learning curve.

But several questions obtrude immediately. Is this decline due to the perceptual and 'attention' process during the exposure? Is it due to the priority or primacy of 'recall?' Finally, is it due to primacy of both 'recall' and 'report?' Psychologists have for many years doubted the efficacy of this experiment to reveal the 'range of attention.' It certainly does bring out some of the effects

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* Accepted for publication March 15, 1933.

of the range of 'memory.' Can the latter be conceived as a co-variable of the 'range of perceptual attention?' The answer has been in many quarters,[1] a vigorous "no." Indeed, psychologists for many decades have considered particularly baffling this problem of distinguishing in 'range of attention' scores between the effects of 'perceptual attention' during exposures and the effects of 'recalling' and 'reporting' what was perceived. In a recent paper the senior author stated a belief that the problem had never been solved successfully.[2] The hope was there expressed that soon a solution would be found which would enable us properly to evaluate the effects of the primacy of 'report' in this type of experiment. Believing that an extended investigation on 225 *S*s undertaken last year and being continued now has given at least a partial solution to the problem, we wish to present the technique and many of its results for criticism or re-application by other investigators.

PRELIMINARY EXPERIMENT

Our first effort to solve the problem developed from the following line of reasoning. If during tachistoscopic exposure perceptual 'attention' is distributed from left to right, having its maximum clearness near the first letter-position, and if the usual type of 'recall' has its own order of effectiveness, as determined by primacy from left-to-right, why cannot the two sequences of clearness or the two orders of effectiveness of 'perception' and 'recall,' be caused to oppose each other? If perception proceeds from left to right, why should not an *S* be forced to 'recall' in an order from right to left, that is, in an order quite the opposite to the order of 'recall' to which most *S*s are habituated? If such reverse order of 'recall' be possible, and if the effect of 'recall'-order is considerable, why would not the effectiveness of the 'perceptual'-order of the clearness of exposure cancel out or balance the effectiveness of a reverse order of 'recall,' thus to render the letter-position curve a straight line, parallel to the base line? If one or the other factor is the stronger, why will not the effect of the stronger cause this curve to vary its direction of declination, especially in those cases where the *fixation* of perception can be made to conflict with the order of 'recall' stipulated, as in left-right perceptions with right-left 'recalls' and in right-left perceptions accompanied by left-right 'recalls?' And, if it be possible to cause *S*s to modify the habitual order of 'recall,' to produce right-left reproductions, why cannot a 'recall' starting at the center character and proceeding thence in alternate fashion towards the left and the right be also possible of attainment?

Plans were formulated to test out these proposals. The preliminary experiment was tried on 25 sophomore college students. Home-made, gravity tachistoscopes were employed.[3] The laboratory's traditional exposure cards served as exposure materials.[4] These were printed in 18-pt. Caslon bold capitals, 4 to 10 letters in nonsense arrangement in the center of each card. Specially mimeographed recording blanks were supplied the *S*s. After each exposure, *E* presented a blank indicated by an arrow where the 'recall' should start and the order in which it must progress. Oral instructions stipulated before each exposure of 100 σ that fixation must be held upon the center of the field. The order of the 'recall' was withheld until after the exposure had been effected. The two new orders of 'recall,' added to the usual left to right (L-R) order, were right to left (R-L), and center-alternate (C-A).

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[1] See especially the many papers by K. M. Dallenbach and his students, many of which have been reviewed in *Psychol. Bull.*, 23, 1926, 1-18; 25, 1928, 493-512; 27, 1930, 497-513.

[2] H. R. Crosland, Letter-position effects in the range of attention experiment as affected by the number of letters in each exposure, *J. Exper. Psychol.*, 14, 1931, 477-507. See especially 501-503, and 507.

[3] These were described in *J. Exper. Psychol.*, 14, 1931, 479, 481.

[4] *Loc. cit.*

This preliminary experiment proved to be a flat failure. In a short time we discovered that instructing an *S* to 'recall' in a given order was no guarantee that he could carry out the instructions. A distinction between order of 'recall' and order of 'report' became at once necessary. The students testified that they could not resist, check, or transform their habituated left-to-right orders of 'recall;' that greatest difficulty attended their efforts to make order of 'report' differ from order of 'recall;' and that by the time a R-L or C-A 'report' was under way they had forgotten the 'recalled' letters. Indeed when these unusual 'reports' had been accomplished, after a fashion, the 'range' scores so returned were for the various letter-positions not appreciably larger than zero. In other words, the carrying through of the instructions to 'report' in the R-L and C-A orders caused so much forgetting of the letters as 'recalled' that the earned 'range' scores were worthless for the purposes of the experiment.

The failure of the preliminary experiment was disheartening. Though difficult to understand, it nevertheless taught two lessons: 'report' might be controlled, but 'recall' would continue to be refractory, and if the technique was to succeed, it could do so only after the *S*s had submitted to large amounts of practice in the odd, R-L and C-A orders of 'report.'

MAIN EXPERIMENT

Technique. An experiment to afford preliminary training to 120 *S*s and after that to test the influences of the three orders of 'report,' was immediately begun. Its formulation included plans to control additional variables which we had not hitherto commanded.

(1) Fixation. The first and foremost of these was fixation of the *S*'s eyes during an exposure. Ten expedients were designed to control, or to measure the effects of, fixation.

(1) The form of print. Fifty-four cards, each 114 mm. by 94 mm. in the vertical and horizontal dimensions respectively, were printed in 14 pt. lower-case Caslon bold type. The 54 were divided into two series, a practice and a main series, of 27 cards each. Each card carried 9 letters in nonsense arrangement. The letters were printed exactly 40 mm. from the bottom of the card. The entire 54 were divided into 3 sets, depending upon the location of the 9 letters across the card from left to right. Eighteen of the cards carried letters placed in the center, that is, with the fifth or middlemost letter placed in the center of the card (between left and right). These are called 'Center-Prints.' Another 18 carried 'Left-Prints,' whose rightmost or 9th characters lay just at the center of the card. The remaining 18 carried 'Right-Prints;' their leftmost or 1st characters were located at center. One purpose of the variable method of print was to procure different fixations, when *S* was required always to fixate the center of the field and the center of the card. Another purpose was to encourage central fixation, for without it *S* would frequently 'see' nothing but white cardboard during an exposure.

(2) Random or chance presentations of 27 cards carrying the various kinds of print.

(3) Two hundred preliminary trials, to habituate central fixation and to habituate the two unusual orders of 'report' (R-L and C-A)—100 trials each.

(4) The 3 orders of 'report,' of L-R, R-L, and C-A—in the main exposures proper.

(5) A random order of these 3 kinds of 'report.'

(6) A chance or random sequence of combinations of *print* of exposure and order of *'report.'*

(7) Introspections as to where, during exposure, fixation was located.

(8) Preëxposure fixation of a hair-line cross (x) in the center of the field.

(9) Typed instructions, reiterated before every exposure, urging the necessity of central fixation; in fact, no exposure was made until *S* indicated that he had fixation adjusted as instructed.

(10) An exposure period of only 100σ.

(2) Handedness. The second variable controlled was that of handedness.[5] Sixty of the 120 *S*s were right-handed; the rest were left-handed.

(3) Eyedness. Eyedness was under control: 60 *S*s were right-eyed and 60 left-eyed.

(4) Handedness and eyedness. Handedness and eyedness were also controlled: 30 *S*s were right-handed and right-eyed; 30 were right-handed and left-eyed; 30 were left-handed and left-eyed; and the remaining 30 were left-handed and right-eyed.

(5) Sex. Each of the above groups of 30 *S*s contained 15 men and 15 women; thus sex was controlled.

So, omitting reference to these collateral devices introduced merely for purposes of control, one observes that the main experiment had two purposes: (1) the affording to each *S* of 100 trials for each of the two unusual orders of 'report' in *right-left* and *center-alternate* directions; and (2) the measurement of the comparative effects of 3 types of 'report,' L-R, R-L, and C-A, under each condition of print (fixation).

In both the trial and main-exposure series a specially mimeographed recording blank was employed. It carried (after a 'report' had been recorded) records concerning measurements of handedness, eyedness, sex, etc. Nine blanks were used for the entering by *E* of the letters as objectively correct. Nine other blanks, just under these, were filled in by *S*, just after an exposure. An arrow indicated where he was to begin his report and the direction it was to take. *E* also gave oral instructions as to order of 'report' just after an exposure ended. *S* did not see this arrow, or know what order of 'report' to expect, until after the exposure had ended. *E* stood at the elbow of his writing arm to assure that he would obey instructions. In case introspections or the discovery of white cardboard by him indicated a failure to fixate the center, the record was discarded and the exposure made over again at some other time, in another sequence.

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[5] Handedness was measured by a series of 5 questions taken from Downey's questionnaire on handedness. Three or more answers in the same direction of conformity constituted the standard of classification; and while absolute conformity between the 5 answers was not required, still in 9 cases out of 10 a ratio of 5 to 0 was obtained. Eyedness was measured in four ways: the two-finger, homonymous, heteronymous double-images test; a modification of this; Miles's manuscope; and a variation of the latter.

Introspective data were recorded in the form of answers to the following questions: Where was your fixation directed just prior to the moment when the exposure occurred? At the moment the exposure began where was your fixation directed? Did fixation move or alter its position after the exposure occurred, that is, during the duration of the exposure itself? If so, how many times did it so alter or change? If it altered, from what point to what point or points did it move, so far as you can now recall? Did any of the letters come to your mind, during the writing, in a different order than the one you were required to follow in writing? If the 'recall' order differed from that of the 'writing,' please state in detail the order of the recall.

Did any letters occur to you, during the writing itself, which you could *not* recognize as having been seen during the exposure? If so, what in detail were such letters?

The exposures were made through means of a Dodge mirror-tachistoscope.[6] The time of the exposure was controlled to 100 σ by a modification of the gravity chronoscope.[7]

Not only did this experiment succeed where the preliminary investigation had failed, but its success is exceeded by that of the study now in progress. The 25 preliminary *Ss* either could not 'report' in the two unusual forms, or the scores of such 'reports' approached the magnitude of zero. The 120 *Ss* of the main experiment could not only make, after 200 preliminary trials, sizable scores under R-L and C-A 'reports,' but the R-L 'reports' were as competent as the L-R 'reports,' a fact amply demonstrated by the data to follow below. The 100 or more *Ss* in the current investigation do not require any preliminary training. They make R-L and C-A 'reports' just as easily as they make L-R 'reports.' The success of the main experiment as compared with the preliminary, is due undoubtedly to the thorough control of fixation and to the chance sequences in which 'reports' and 'prints' accompany one another and succeed themselves. *S* cannot anticipate the kind of print (fixation) nor the order of 'report.' Hence his largest chance of obtaining a high score will depend upon fixation of the center of the exposure field. Moreover he has been aided in this type of fixation by the 200 preliminary trials. The success of the current project over the main one of last year lies, we believe, in the fact that the present *Ss* are all defective readers. The nature of this defectiveness does not, however, concern us here.

Results. The above assertions of success will be more convincing when the obtained results are studied. Below are presented 6 graphs (Figs. 1-6) and 7 tables. The former relate to differences between orders of 'report' under each type of print (fixation) and to differences between prints (fixations) under each order of 'report.' The tables relate to the sizes and significances of differences, as between prints, in one case, and between 'reports,' in the other case, where in each instance the 3 samples of the other variable have been pooled; and to the general effectiveness of the L-R, R-L, and C-A types of 'report' where all letter-positions have themselves been pooled.

Scrutiny of Figs. 2 and 3 should serve to convince the most skeptical reader that absolutely no differences appeared as between the three orders of report: right-left, left-right, and center-alternate. Not only do the curves of the effectiveness of report begin at approximately the same high points in each of these

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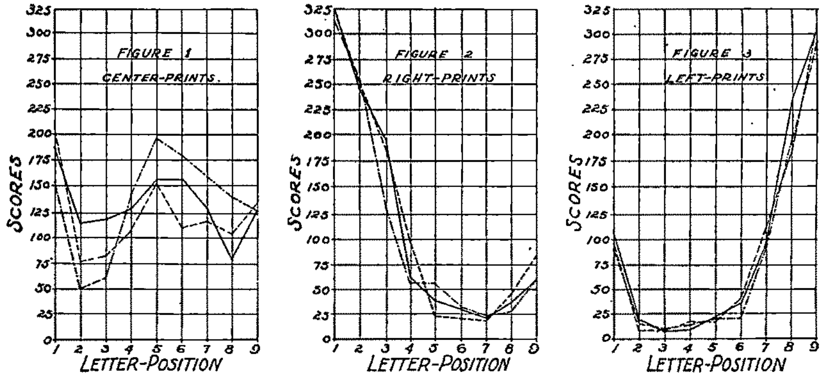
[6] *J. Appl. Psychol.,* 12, 1928, 82-85.

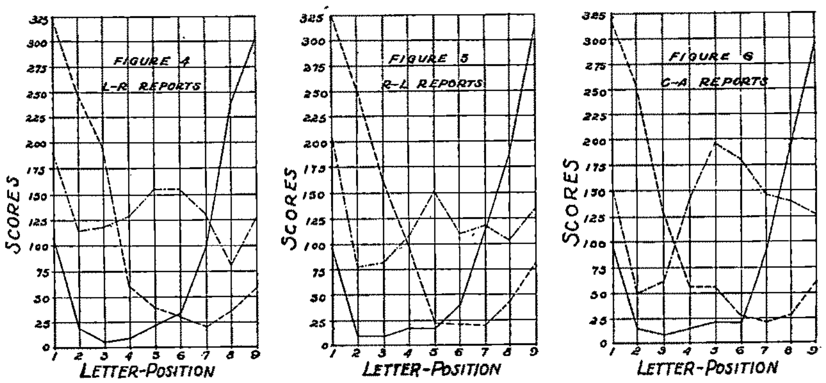
[7] Cf. *J. Exper. Psychol.,* 9, 1926, 162-168.



Figs. 1-3. Showing, for Each Condition of Print, the Scores for Every Letter-Position

Center-Print = center-fixation
Right-Print = left-fixation
Left-Print = right-fixation

———— = L-R report
– – – – = R-L report
–.–.–.– = C-A report



Figs. 4-6. Showing, for Each Order of Report, the Scores for Every Letter-Position

L-R = left to right report
R-L = right to left report
C-A = center-alternate report

———— = Left-Print
– – – – = Right-Print
–.–.–.– = Center-Print

figures, but the declination is practically of the same form and equal in amount. This is so true that constructing the drawings was very difficult; at many points the 3 curves in each of these figures actually superimpose themselves. The meaning of the approximate identicalness of the 3 'report' curves in each of Figs. 2 and 3 is to the effect that R-L and C-A reports are quite as competent as L-R reports. Moreover, all 3 give exactly the same results, curves of the same

TABLE I

Differences Between the Mean Scores for 'Left-Right' and 'Right-Left' Reports for Each Letter-Position; the Standard Errors of These Differences, with the Significance of Each Difference
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	5.23	5.19	0.04	2.33	2.81	.2	0.2	58
2	2.83	3.17	0.34	1.46	1.55	.142	2.4	99
3	2.11	2.72	0.61	1.46	1.38	.149	4.1	100
4	1.84	1.64	0.20	1.24	1.24	.119	1.7	96
5	1.59	1.83	0.24	1.23	1.41	.133	1.8	96
6	1.53	1.84	0.31	1.33	1.51	.144	2.2	99
7	2.11	2.17	0.06	1.70	1.63	.141	0.4	65
8	2.75	2.94	0.19	1.71	1.44	.155	1.2	88
9	4.38	4.06	0.32	1.83	1.81	.170	1.9	97
Mean	2.71	2.84	0.247				1.77	89

form, same magnitudes, and with the same degrees of declination. Finally, the factor of almost infinitely great importance is the form of fixation as this is determined by the style of print. When one compares Fig. 2 with Fig. 3, one finds curves almost the perfect opposites of one another in the direction and form of declination. The curves of Fig. 3 have their highest scores at position 9; those of Fig. 2 at position 1. Position 1 in Fig. 3 exhibits slightly higher scores than position 9 in Fig. 2. In Fig. 2, position 1 gives slightly higher scores than position 9 in Fig. 3. Positions 5, 4, 3, and 2 in Fig. 3 show slightly lower scores than positions 5, 6, 7, and 8 in Fig. 2. The conclusions from Figs. 2 and 3 deserve emphatic reiteration: (1) R-L and C-A reports can be and are made competently. (2) R-L, L-R, and C-A reports are exactly alike, in nearly every respect, in each figure; and (3) the form of print, the fixation of the eyes, or of 'attention,' is by all odds the chief determiner of the score and of which

TABLE II

Differences Between the Mean Scores for 'Left-Right' and 'Center-Alternate' Reports for Each Letter-Position; the Standard Errors of These Differences, with the Significance of Each Difference
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	4.81	5.19	0.38	2.00	2.81	.1257	3.02	100
2	2.66	3.17	0.51	1.27	1.55	.1536	3.32	100
3	1.70	2.72	1.02	1.35	1.38	.1357	7.52	100
4	1.80	1.64	0.16	1.21	1.24	.1277	1.25	89
5	2.28	1.83	0.45	1.40	1.41	.1175	3.83	100
6	1.88	1.84	0.04	1.38	1.51	.127	0.31	62
7	2.15	2.17	0.02	1.66	1.63	.1296	0.15	56
8	3.11	2.94	0.17	1.60	1.44	.1356	1.25	89
9	3.99	4.06	0.07	1.69	1.81	.1281	0.55	71
Mean	2.70	2.84	0.313				2.36	85

TABLE III

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'RIGHT-LEFT' AND 'CENTER-ALTERNATE' REPORTS FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	*D/σ* Diff.	Percent Chances
1	4.81	5.23	0.42	2.00	2.33	.14	3.0	100
2	2.66	2.83	0.17	1.27	1.46	.1304	1.3	90
3	1.70	2.11	0.41	1.35	1.46	.121	3.4	100
4	1.80	1.84	0.04	1.21	1.24	.132	0.3	62
5	2.28	1.59	0.69	1.40	1.23	.122	5.7	100
6	1.88	1.53	0.35	1.38	1.33	.127	2.8	100
7	2.15	2.11	0.04	1.66	1.70	.141	0.3	62
8	3.11	2.75	0.36	1.60	1.71	.168	2.1	98
9	3.99	4.38	0.39	1.69	1.83	.149	2.6	99
Mean	2.70	2.71	0.32				2.36	90

position-scores will be high and which low. Form of print (fixation) renders every order of report into a form consistent with itself. Priority of 'report,' or of 'recall,' or of both 'recall' and 'report' is wholly absent or its effects are negligibly small. The ruling factor is that of fixation.

With respect, however, to Fig. 1, there are slight differences to be noted as between the orders of report. The L-R, R-L, and C-A curves, under the condition of central fixation, do not superpose themselves, although they are greatly similar in form. This similarity is particularly evident in letter-positions 1, 2, and 3. Some similarity also exists at position 5 (the point of fixation), and again at positions 6 and 7. All three curves differ from those which the senior author has published previously,[8] curves which were believed to have been obtained under conditions of central fixation. (Two explanations probably account for the dis-

TABLE IV

DIFFERENCES BETWEEN THE MEAN SCORES FOR 'LEFT' AND 'RIGHT' PRINTS FOR EACH LETTER-POSITION; THE STANDARD ERRORS OF THESE DIFFERENCES, WITH THE SIGNIFICANCE OF EACH DIFFERENCE
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	*D/σ* Diff.	Percent Chances
1	8.07	2.51	5.56	1.72	2.52	.2553	21.78	100
2	6.30	0.35	5.95	2.29	0.856	.2135	27.87	100
3	4.00	0.19	3.81	2.19	0.959	.2042	18.66	100
4	1.47	0.28	1.19	1.63	0.645	.1487	8.00	100
5	0.99	0.49	0.50	1.36	0.894	.1319	3.79	100
6	0.68	0.78	0.10	1.03	1.29	.1393	0.718	76
7	0.53	2.52	1.99	1.11	2.32	.2138	9.31	100
8	0.88	5.13	4.25	1.40	2.31	.2377	17.88	100
9	1.64	7.57	5.93	2.04	1.67	.2454	24.16	100
Mean	2.73	2.20	3.24				14.69	97

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[8] *J. Appl. Psychol.*, 12, 1928, 82-124; *Psychol. Bull.*, 26, 1929, 375-377; *J. Exper. Psychol.*, 14, 1931, 477-507.

TABLE V

Differences between the Mean Scores for 'Left' and 'Center Prints' for Each Letter-Position; the Standard Errors of These Differences, with the Significance of Each Difference
(N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	2.51	1.59	0.94	2.52	3.21	.2107	4.37	100
2	0.35	2.02	1.67	0.856	1.87	.1559	10.77	100
3	0.19	2.19	2.00	0.959	1.90	.1780	11.24	100
4	0.28	3.13	2.85	0.645	1.87	.1794	15.89	100
5	0.49	4.20	3.71	0.894	2.37	.2128	17.43	100
6	0.78	3.73	2.95	1.29	2.38	.2209	13.35	100
7	2.52	3.33	0.81	2.32	2.43	.2474	3.27	100
8	5.13	2.69	2.44	2.31	2.46	.3095	7.88	100
9	7.57	3.23	4.34	1.67	2.69	.2864	15.15	100
Mean	2.20	2.90	2.41				11.04	100

TABLE VI

Differences between the Mean Scores for 'Right' and 'Center Prints' for Each Letter-Position; the Standard Errors of These Differences, with the Significance of Each Difference
N = 120)
(Maximum Score = 9)

Position	M_1	M_2	D.	σ_1	σ_2	σ Diff.	D/σ Diff.	Percent Chances
1	8.07	1.59	6.48	1.72	3.21	.2678	24.20	100
2	6.30	2.02	4.28	2.29	1.87	.2694	15.89	100
3	4.00	2.19	1.81	2.19	1.90	.2462	7.35	100
4	1.47	3.13	1.66	1.63	1.87	.2069	8.02	100
5	0.99	4.20	3.21	1.36	2.37	.2339	13.72	100
6	0.68	3.73	3.05	1.03	2.38	.2089	14.60	100
7	0.53	3.33	2.80	1.11	2.43	.2234	12.53	100
8	0.88	2.69	1.81	1.40	2.46	.199	9.10	100
9	1.64	3.23	1.59	2.04	2.69	.2114	7.52	100
Mean	2.73	2.90	2.96				12.55	100

TABLE VII

Mean Scores, of All Nine Letter-Positions, for All Ss (120), for Each Type of Print (Fixation), Arranged by Orders of Report
(Maximum Score = 3)

Order	Left-Print	Center-Print	Right-Print	Mean
L–R	.766	1.114	.932	.937
R–L	.733	1.012	.952	.899
C–A	.698	1.106	.879	.894
Mean	.732	1.077	.921	

cordant results: failure in the early experiments to control fixation, and the fact that practically all early *Ss* were both right-eyed and right-handed.) The older curves declined rapidly from left to right, with no further decline at positions 6 and 7 and with rises for positions 8 and 9. The present curves resemble them only for positions 1 and 2; thereafter all similarity ceases. In fact the high peaks at position 5, and the general greater efficiency of reports of positions 6, 7, 8, and 9, combine with the facts of Figs. 2 and 3 to demonstrate the imperativeness of fixation as the chief factor determining the sizes of scores. One may say that the earlier discovered L to R tendency is somewhat represented at two points in the curves of Fig. 1: from position 1 to position 4, and from 5 or 6 to position 9. Since only a small if any difference exists as between the three orders of report, the earlier-described L to R tendency of 'attention' is still evident in the results. It has been interrupted merely by a stronger factor; namely, that of fixation. In this case it has suffered adversely in competition with central-fixation. It appears also that reports of characters to the right of center, in positions 6, 7, 8, and 9, have been more competent than for positions 1, 2, 3, and 4. The total scores for these positions (all reports pooled) are 1556 and 1428 respectively. This again may be considered as an indication of the presence, though in weakened form, of the 'attentional' and perceptual L to R tendency. The L-R order of report is throughout Fig. 1 slightly superior to the R-L order. If we omit reference to position 1, the total scores for the remaining 8 positions are, for L-R, 1013; for R-L, 887. Similarly omitting the first 5 positions, the scores for positions 6 to 9 are, L-R, 498; for R-L, 468. Observing Table VII, one discovers that under central fixation (center-print), the entire set of 9 positions gives the following average scores: L-R, 1.114; R-L, 1.012; and C-A, 1.106. The latter quantities mean that while L-R and C-A reports excelled R-L reports the differences between them are insignificantly small, even under conditions of central fixation. Hence except for the two exhibitions in Fig. 1, of the L to R perceptual 'attention' tendency, the results in these curves conform closely with those contained in Figs. 2 and 3.

Turning our attention now to Figs. 4, 5, and 6, where the curves above described have been regrouped, we discover at once and unmistakably that the prime factor determining the sizes of position scores is the form of print or fixation. In Fig. 4 the 3 L-R report-curves are almost wholly different from one another. There are only two similarities between them: the initial drop between positions 1 and 2, and the final rise, between positions 8 and 9. Again it is clear that L-R reports were not only not stereotyped in the L to R form but were so fluid as to permit the factor of fixation to cause them to resemble greatly the R-L and C-A reports. Similarly, in Fig. 5, one notes that not only were R-L reports possible but with minor exceptions they resemble very closely the L-R curves of Fig. 4. They were also practically as efficient as the L-R reports. Lack of stereotypy is to be discovered in them; they altered their courses with apparent ease to conform to the condition of fixation (print). Finally the curves of Fig. 6 are very similar to the curves of Figs. 4 and 5. Center-alternate reports conformed so closely to the condition of print or fixation that their curves assumed the forms of the curves of both the L-R and R-L reports. Their largest failure to conform lay in positions 4, 5, and 6, where one observes a fairly high peak in the curve for center-print. In other words, the order of report in this instance exerted a very small influence when, and only when, it was aided by the factor of fixation. In conclusion it must be reiterated that the order of report is a very weak factor in determining the sizes of position scores. If any factor of 'recall' order was also present, its effects certainly were lost in the effects accruing from the form of fixation; and where it conflicted with the R-L and C-A report orders, it most assuredly was too weak to transform the latter types of report into a form consistent with its own order of effectiveness. The results strongly suggest that the factor of priority or primacy of 'recall,' or of 'report,' or of both 'recall' and 'report,' has been grossly exaggerated in importance. The main factors influencing 'report' efficiency and distributing the sizes of scores among the letter-positions appear to have been inherent in the act of fixation and the perception and 'attention' accompanying such fixation.

There remain to be discussed the contents of Tables I to VII. The first three of these, Tables I, II, and III, show the sizes of differences between the L-R, R-L, and C-A reports for each letter-position, and the significance of each such difference. It should be borne in mind that these tables contain composite results, all forms of print (fixation) pooled together in the mean scores. Hence, should differences between prints prove to be large and significant beyond question, such differences will contaminate those differences apparently accruing from the form of report, to render them larger than they really are and to augment their significance. With this caution, we may proceed to observe the contents of these tables. The mean difference, for the 9 letter-positions, between L-R and R-L reports is only 0.247 (of a maximum score of 9); and the mean significance of the 9 differences is represented by only 89 chances in 100, a number of chances quite under a number which would make the significance certain. This means that many chance factors are present to aid in causing the 9 differences between L-R and R-L reports here shown. When it is recalled that these differences are contaminated by print-differences, one must conclude that such differences are even farther removed from being real differences, and that the number of chance factors is larger than here shown. In only one letter-position (position 3) did a difference occur whose magnitude was large enough to suggest a significance of unquestioned certitude. Likewise, in Table II the mean difference for the 9 letter-positions, between L-R and C-A reports, was only 0.313; with a mean significance of only 85 chances in 100. Considering these facts in the light of the above-mentioned contamination, these differences probably are determined in a large degree by chance factors. Table III shows a mean difference between R-L and C-A reports for the 9 letter-positions of only 0.32, with a mean significance indicated by 90 chances in 100, the largest obtaining in these three tables. In four instances, letter-positions 1, 3, 5, and 6, the significance of the differences appears to be certain, but the aforesaid contamination is probably a contributing factor in the sizes of these differences. Chance factors have not been wholly eliminated from the differences as between R-L and C-A reports as shown in Table III.

Turning to Tables IV, V, and VI, we discover a wholly different state of affairs: the differences between forms of print, that is, between the scores resulting from three types of fixation, are with one exception enormously greater than the differences between orders or report. The exception is for letter-position

6, Table IV, in the differences between 'Left' and 'Right Prints.' Every other difference between forms of 'Print' is so large as to be expected in 100 chances in 100. In fact the ratios of the difference to σ of the difference, range (with the above exception) from 3.27 to 27.87. Inasmuch as a ratio of 3.00 determines perfect certitude of significance, the excess amounts in these ratios above 3.00, are not reflected in additional chances: there is no higher percentage than 100 in 100. If we were to compare the mean ratios of difference to σ difference, of Tables I, II, and III as against Tables IV, V, and VI, we should find 2.16, and 12.76 respectively. This large difference between ratios finds no fair exemplification in degree of significance in favor of the differences between 'prints.' However, the sizes of the differences between 'prints' may be compared with the sizes of the differences between orders of report. The mean difference between reports is 0.293, while the mean difference between 'prints' is 2.87. That is, the latter is 9.6 times as large as the former; and the former differences are contaminated by the latter. We must conclude, therefore, that 'print' or 'fixation' is almost an infinitely more important factor than order of 'report'; that few, and small, if any, differences in sizes of scores result from different types of report.

Scrutiny of Table VII should allow us to generalize concerning differences as between 'prints' and as between 'reports' when all 9 letter-positions have been averaged together. Very little, if any, difference is to be found between the types of 'report' under the condition of 'center-prints.' All 'reports' under this condition were more excellent than those under 'right' and 'left-prints.' Under 'center-print,' L-R reports led, followed in order by C-A, and lastly by R-L reports. The poorest 'reports' occurred under 'left-prints.' Under 'right-prints,' the C-A report was the most inefficient. Under 'left-print,' the L-R report excelled the R-L report. Under 'right-print' the reverse was true: R-L reports excelled the L-R reports. Throughout the entire experiment, where all 'prints' are pooled, the L-R reports are somewhat better than the other two forms; and practically a tie exists between R-L and C-A reports. The only evidences in the whole experiment of the existence of a somewhat stereotyped and fixed or perseverative L to R tendency of 'recall,' or 'report,' or both, are this slight superiority of L-R reports over the other two types when all 'prints' and all letter-positions are pooled together, and the slight inferiority of all reports of 'left-prints.' It is not impossible, however, that the excessive left-eyedness of the 60 left-eyed *Ss*, in contrast with the weaker right-eyedness of the 60 right-eyed *Ss*, weighted very heavily these reports of 'center-' and 'right-prints,' so that the L-R reports seem more incompetent (especially under 'right-prints') than the mere order of report can itself explain. It is a fact that the left-eyed *Ss* were much more at a disadvantage under conditions of 'left-print' than were the right-eyed *Ss* under conditions of 'right-print.' Conversely, left-eyed *Ss* possessed a greater advantage in 'right-prints' than that exhibited by right-eyed *Ss* in 'left-prints.' This is, however, another story, and need not engage us here.

GENERAL SUMMARY

(1) Two experiments, a preliminary and a main investigation, have been devoted to comparing right-left and center-alternate 'reports' with the usual left-right 'reports.' The former involved 25 *Ss*; the latter applied to 120 *Ss*. The 25

*S*s were unselected; the 120 were selected in balanced groups on the basis of sex, handedness, and eyedness.

(2) The preliminary experiment contained expedients of questioned effectiveness to control fixation of the *S*s' eyes. The main experiment employed 10 expedients to control fixation, which are believed to have proved their efficiency. The preliminary experiment failed; the main undertaking has, we believe, succeeded in a gratifying fashion.

(3) Among the 10 the chief expedients were two: (a) three variable types of card, presented in random and chance sequences, were (1) 'center-prints,' to obtain the effects of central fixation; (2) 'left-prints' to effect right-field fixations; and (3) 'right-prints,' to accomplish left-field fixations; and (b) three variable 'orders of report:' left-right (L-R), center-alternate (C-A), and right-left (R-L). Three additional expedients were the random sequences of 'prints,' random sequences of 'orders of report,' and a random sequence of combinations of 'prints' and 'orders of report.'

(4) Not only were R-L and C-A reports found possible of accomplishment, but they turned out to be approximately as efficient as the usual L-R orders of report.

(5) If there is a preferential and hard-to-break left-to-right tendency of 'recall,' or of 'report,' or of both, it failed to show itself in this investigation. Not only was 'recall' as an independent factor absent, or was translated into the 'report' influence, but the latter, the 'report' tendency, was itself lacking or was very weak.

(6) The prime condition to determine what letters will be 'reported,' in high frequency and accurately, was discovered to be the style of print, the type of fixation induced by the conditions listed under (3) above. All other factors, such as a tendency of L-R recall, proved to be merely incidental if present at all. Two much more important conditions than priority of recall or report have been discovered to be the eyedness and the handedness of the *S*s, topics concerning which subsequent papers will attempt to shed some light.

COLOR PHENOMENA FROM ADJACENT RETINAL AREAS FOR DIFFERENT TEMPORAL PATTERNS OF INTERMITTENT WHITE LIGHT

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In a previous paper[1] the writer has described certain color phenomena attending the intermittent exposure of a small bright white area. The effects of varying the brightness of the area, the frequency and duration of exposures, and the brightness of the surrounding area were investigated, and the results were shown to be compatible with Troland's suggestion that the transmission of color impressions from the retina to the cortex might depend upon the modulation of the optic nerve current.[2] In this paper a more complicated stimulus-situation is considered; namely, the stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light.[3]

Obviously stimulus-patterns of this kind could be multiplied without number, but we may confine our attention to a few examples which illustrate the general underlying principles. This paper aims to show how the factors involved in a complex stimulus-situation can be analyzed, and how the facts can be accounted for on Troland's modulation theory.

If a given area of the retina, β, is stimulated intermittently and the surrounding part of the retina is also stimulated intermittently, that part of the field of vision corresponding to β may be seen colored and this color may arise in either of two ways: (1) the color may be induced upon β by intermittent stimulation of the surrounding area, a phenomenon related to that of color-contrast; (2) or it may depend upon the intermittent stimulation of β itself. Four criteria for the determination of the mode of production of the color are suggested by the following questions: (1) What is the effect of stimulating β steadily instead of intermittently? (2) What is the effect of stimulating the surrounding area steadily instead of intermittently? (3) What is the effect of leaving the surrounding area unstimulated? (4) What are the effects of varying the phase relations of the two sets of stimuli?

The color may be regarded as color-contrast induced upon β by intermittent stimulation of the surrounding area, (1) if the color can be similarly produced when β is stimulated steadily instead of intermittently, and (2) if the color can be produced only when the surrounding area is intermittently stimulated.

The color may be attributed to the intermittent stimulation of β, if the color cannot be produced when β is steadily stimulated instead of intermittently; and

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[1] G. A. Fry, Modulation of the optic nerve-current as a basis for color-vision, this JOURNAL, 45, 1933, 488-492.

[2] L. T. Troland, The enigma of color vision, *Amer. J. Physiol. Opt.*, 2, 1921, 43.

[3] The present paper, published by the author as National Research Fellow in the Biological Sciences, is from the Department of Ophthalmology, Oscar Johnson Institute, Washington University, St. Louis, Mo.

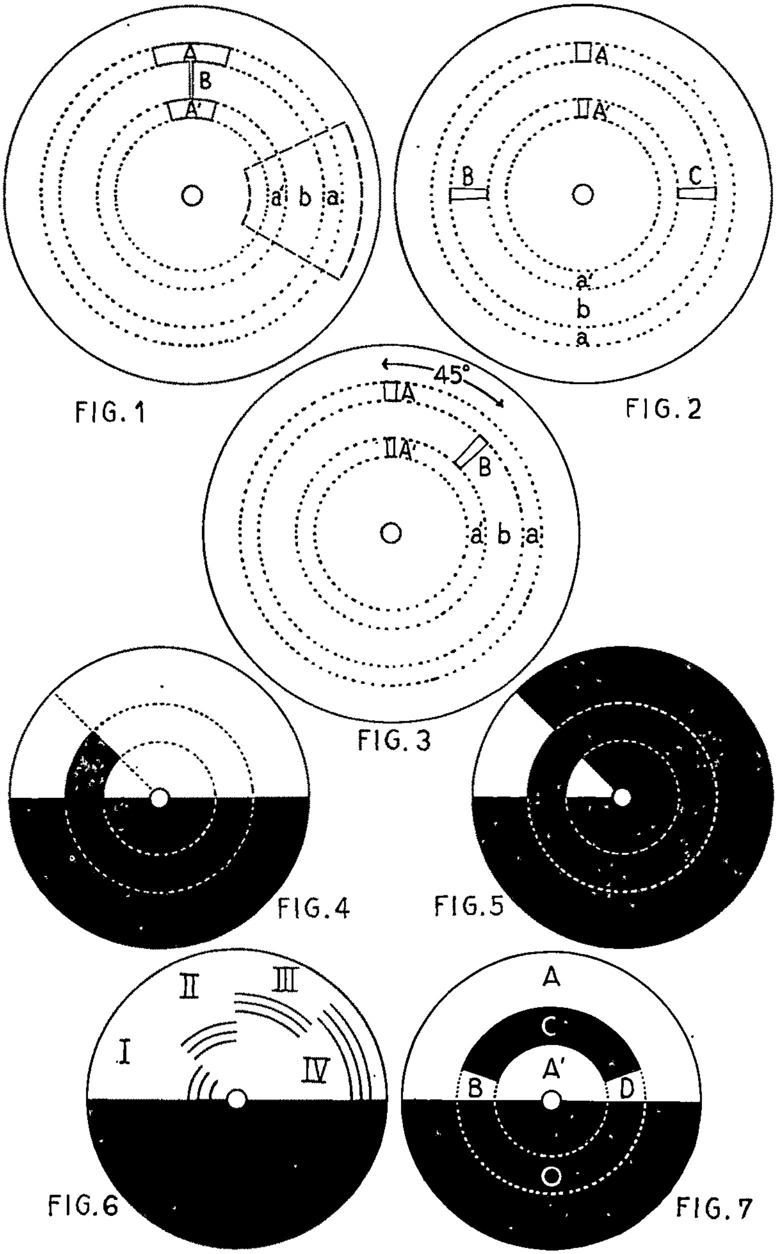

FIG. 1 FIG. 2 FIG. 3 FIG. 4 FIG. 5 FIG. 6 FIG. 7

Figs. 1-7. Showing Details in the Construction of the Various Disks

TABLE I

Analysis of the Phenomena Obtained with the Disk in Fig. 1

Character of Stimulation of β	Character of Stimulation of α and α'	Color of *b*	Color of *a* and *a′*
Intermittent	Intermittent	Green	Pink
	Steady*	Green	White
	Absent	White	Black
Intermittent	Intermittent	Green	Pink
Absent		Black	Pink

* If A and A′ are each divided into ten 3°-slits and these are evenly distributed around *a* and *a′*, the frequency of stimulation for these zones is so rapid that it is equivalent to steady stimulation.

it need not be regarded as color-contrast induced by intermittent stimulation of the surrounding area, if the color can be produced when the surrounding area is either steadily stimulated or not stimulated at all. There are cases in which the color can be produced when the surrounding area is steadily stimulated, but cannot be produced if the surrounding area is left unstimulated.

The significance of the effects obtained by varying the phase relations of the two sets of stimuli can be understood better after a few concrete examples have been presented.

Let us illustrate the general principles stated above by applying them to a few concrete examples. The disks in Figs. 1, 2, and 3 are made of black cardboard and are 10 in. diam. In each case zone *b* is 1 in. wide; zones *a* and *a′* are each ½ in. wide; and zone *a′* is 2 in. from the center of the disk. A, A′, B, and C represent open slits. In the case of Fig. 1, A and A′ are 30° wide and B is 1° wide. In the case of Fig. 2, A, A′, B, and C are each 5° wide. In the case of Fig. 3, A, A′, and B are each 5° wide and the leading edges of A and A′ are 45° from the leading edge of B. These disks were rotated before a bright surface, which consisted in a ground glass illuminated from behind by two 250-watt lamps. The brightness of this surface was 402.7 c. per sq. ft. and its size and position with respect to the disks are indicated by the dash line in Fig. 1. The experiments were performed in a dark room. The observations were made at a distance of 12 ft.

For the sake of convenience the symbols, α, β, and α', will be used to refer to the three zones of the retina corresponding to the three zones of the disk, *a, b,* and *a′* respectively.

When the disk in Fig. 1 is rotated 10 times per sec. before the bright surface, zone *b* is seen green and zones *a* and *a′* pink. An analysis of this phenomenon was made in accordance with the criteria mentioned above and the results are presented in Table I. The intermittent stimulation of α and α' aids in the production of the green color of *b,* for the green cannot be produced when α and α' are left unstimulated. However, the green need not be regarded as color-contrast induced upon β by intermittent stimulation of α and α', for the green can be produced just as well when α and α' are steadily stimulated instead of intermittently. Neither is the pink of zones *a* and *a′* color-contrast induced by intermittent stimulation of β, for the pink is produced even when β is left unstimulated.

Fig. 8 illustrates a possible conception of what takes place in the three zones of the retina corresponding to the three zones of the disk. α and α′ depress activity of β[4] during the intervals between successive stimulations and thus help to modulate it. It may be supposed further that this modulation subserves the production of the sensation green. At least this inference is in accordance with Troland's suggestion that the transmission of color impressions from the retina to the cortex may depend upon the modulation of the currents in the various optic nerve-fibers. The activity of α and α′ seems to be capable of being modulated by its own intermittent stimulation without the aid of β.

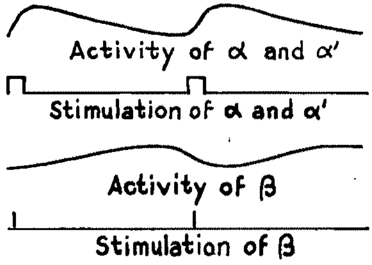

FIG. 8

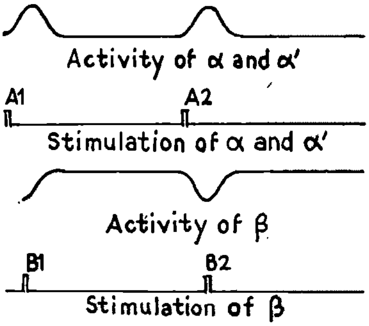

FIG. 9

FIGS. 8-9. ILLUSTRATION OF THE RETINAL PROCESSES

It must be supposed that varying the phase-relations of the two sets of modulation-waves in no way interferes with the wave-pattern of either set, because varying the phase of B with respect to A and A′ has no effect upon the colors.

In the case of the disk in Fig. 2 color-contrast undoubtedly comes into play. When the disk is rotated 10 times per sec. before the bright surface, zones *a* and *a′* are seen blue and zone *b* yellow. An analysis of this disk, similar to the analysis of the disk in Fig. 1, results in the data of Table II. The yellow of zone *b* must be regarded as color-contrast induced by intermittent stimulation of α and α′ for it cannot arise in any other way. The yellow can be produced when β is steadily stimulated so that it cannot be attributed to the intermittence of stimulation of β. If α and α′ are left unstimulated, *b* is not seen yellow but a flickering white. If α and α′ are steadily stimulated *b* is seen as light blue instead of yellow. In other words, *b* is seen as yellow only when α and α′ are intermittently stimulated.

Zone *b,* in turn, aids in the production of the blue of *a* and *a′*; the blue cannot be produced if β is left unstimulated, for under these conditions only a flickering white sensation is obtained. The blue is produced, however, when β is

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[4] It is not necessary to assume that this depressive effect has its seat entirely in the retina. Interaction between the various retino-cortical pathways might take place not only at the retina but also at the basal ganglia and the cortex.

TABLE II

ANALYSIS OF THE PHENOMENA OBTAINED WITH THE DISK IN FIG. 2

Character of Stimulation of β	Character of Stimulation of α and α'	Color of *b*	Color of *a* and *a′*
Intermittent	Intermittent	Yellow	Blue
	Steady†	Bluish white	Yellow
	Absent	White	Black
Intermittent	Intermittent	Yellow	Blue
Steady*		Yellow	Blue
Absent		Black	White

* B and C were divided each into five 1° slits and these were evenly distributed around zone *b*. The frequency of stimulation for this zone was then so rapid that it was equivalent to steady stimulation.

† A and A′ were divided each into ten 1° slits and these were distributed evenly around zones *a* and *a′*, thus making the stimualtion of these zones equivalent to steady stimulation.

steadily stimulated as well as when β is intermittently stimulated. The blue is produced only when α and α' are intermittently stimulated and is not produced if α and α' are steadily stimulated.

A plausible account of what takes place in the three zones of the retina corresponding to the three zones of the disk is approximately as follows. The activity of zone β depresses the activity of α and α' during the intervals between successive stimulations and thus helps to modulate it. However the activity of α and α' does not have in return a similar effect upon the activity of β, for β acts as if it were steadily stimulated and shows no tendency to set up a rhythmic modulation of its own. The modulation which it acquires, and which subserves the yellow color-sensation, must be induced upon it by the rhythmic activity of α and α'. It might be supposed that the activity of β tends to wax and wane reciprocally with that of α and α', being depressed when the activity of α and α' waxes and enhanced when it wanes.

If the intermittent stimulation of zone β is equivalent to steady stimulation, we have an explanation of the fact that varying the phase of A and A′ with respect to B and C has no effect upon the colors.

The disk in Fig. 3 presents an entirely different situation. When it is rotated anticlockwise before the bright surface, zone *b* appears yellowish white and zones *a* and *a′* purplish blue. In spite of the fact that all the slits are the same size, the purplish blue appears much darker than the yellowish white. When the direction of rotation is reversed, the colors appear reversed.

Fig. 9 illustrates a possible conception of what takes place in the three zones of the retina corresponding to the three zones of the disk. Let us suppose that the disk is rotating anticlockwise with B following close on to A and A′. During the long periods from B1 to A2, and from B2 to A3 the activity of β depresses the activity of α and α', and during the short periods from A2 to B2, and from A3 to B3, α and α' depress β.[5] Thus the activity of β waxes and wanes reciprocally with that of α and α', and it might be supposed that the difference in color depends upon the difference in the shapes of the modulation-waves.

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[5] The fact that *a* and *a′* appear darker than *b* shows that β depresses α and α' more than they depress β.

When adjacent areas of the retina are stimulated with different temporal patterns of intermittent white light, peculiar color phenomena are apt to arise at the juncture of the two areas. Thus when the disk in Fig. 4 is rotated anticlockwise 4 times per sec., illuminated with a 250-watt lamp placed 2 ft. away and observed at a distance of 2 ft., the band engendered by the moving arc appears to have a vivid green core and red borders. When the disk is rotated in the opposite direction, the band is seen to have dark blue borders and a purplish core and on each side of the band is a halo of very light blue. The fact that the colors at the border change with the direction of rotation indicates that peculiar physiological processes come into play.

Bidwell[6] pointed out that, when a white object is suddenly exposed in a dark field of vision, it is seen to have red borders for a moment. The essential conditions for the observation of Bidwell's red borders are provided by the disk in Fig. 5. When such a disk is rotated 4 times per sec., illuminated with a 250-watt lamp placed 2 ft. away and observed at a distance of 2 ft., the dark band is seen bordered internally on each side with red. These red borders are very narrow and much more difficult to observe than in the case of the disk in Fig. 4.

Bidwell's red borders owe their existence at least in part to chromatic aberration. Blue rays of light entering the eye come to a focus sooner than the red rays. Hence the retinal blur circle produced by a point source of white light must have either a blue core and a red border, or a red core and a blue border, depending upon whether the blue or the red rays come to a focus on the retina. Let us assume that at a distance of 2 ft. clearest vision is obtained when the blue rays come to a focus on the retina and the blur circles thus have blue cores and red borders. Accordingly the immediate outside of a white object should be seen red and the immediate inside blue. These colors are not seen when one gazes steadily at an object, but when a white object is intermittently exposed, as in the case of the disk in Fig. 5, red borders are actually seen. It is impossible to say why scattered red light is more effective under these conditions, but certain facts indicate that the actual stimulation of the retina with red light plays just as important a rôle as the intermittence of stimulation.

In the first place, the borders appear blue instead of red if a convex lens is placed in front of the eye so that the blue rays cannot come to a focus on the retina. The borders also appear blue if the observer stands 12 ft. away, provided A and A′ are very bright. It may be supposed that at this distance the blue rays cannot come to a focus. In the second place, if a prism is held before the eye, the border on one side appears blue and on the other red. Finally, it must be noted that Bidwell himself observed a suddenly exposed white object through ten different colored glasses and was able to see a red border only when red light was transmitted.

There is a fundamental difference between the disk in Fig. 4 and the disk in Fig. 5. If a prism is held before the eye when the disk in Fig. 4 is at rest, the border on one side of the black arc appears red and the border on the other blue; but when the disk is rotated anticlockwise, the blue border is apparently transformed into a purplish red one. However, in the case of the disk in Fig. 5, the blue border remains blue. The transformation of the blue into a purplish red in the case of the disk in Fig. 4 evinces the play of some peculiar physiological process.

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[6] S. Bidwell, On subjective colour phenomena attending sudden changes of illumination, *Proc. Roy. Soc.*, 60, 1896, 368-377.

Benham's disk, which is illustrated in Fig. 6, is of interest in this paper inasmuch as it involves the stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light. When such a disk is rotated anticlockwise about 7 times per sec., and is illuminated with diffused daylight, the four groups of circles, engendered by the moving arcs, appear colored as follows: I, red; II, yellow; III, green; IV, blue. When the direction of rotation is reversed, the colors appear reversed.

All the previous investigators[7] seem to have arrived at the fundamental conclusion that the essential factor in determining the color of a given group of lines on Benham's disk is the time required for the disk to turn through the angular distance from the leading edge of the white sector to the given group of lines. The superficiality of this analysis may be demonstrated by the following experiment:

When a disk 8 in. in diam. like the one in Fig. 7 is rotated 8 times per sec., illuminated with a 250-watt lamp placed 1 ft. from the disk and at an angle of 45°, and observed at a distance of 12 ft., a flickering green band is seen. According to the conclusions of previous investigators the greenness of the band depends upon the position of the black arc, C, with respect to the leading edges of A and A′. Accordingly shifting the position of C ought to change the color. But if, as C is shifted, B, D, and O are shifted along with it without changing their sizes, the band still appears green no matter what position C assumes with respect to A and A′.

Instead of shifting B, D, and O along with C previous investigators kept the size and position of O constant and changed the relative sizes of B and D. In this way they obtained a difference in color and attributed it to the change in position of C, thus ignoring the change in the relative sizes of B and D as a possible determining factor.

When an analysis of Benham's disk is made in accordance with the principles outlined in this paper, the following conclusions may be reached: (1) The red of the first group of lines is a border phenomenon related to that obtained with the disk in Fig. 4 (rotated anticlockwise). (2) The yellow of the second group of lines is a color contrast effect induced by intermittent stimulation of the surrounding area and is similar to the yellow obtained with the disk in Fig. 2. (3) The green of the third group of lines is produced by intermittent stimulation and is analogous to the green obtained with the disk in Fig. 7. (4) The blue of the fourth group of lines is allied to what is obtained with the disk in Fig. 4 (rotated clockwise) but it has not been determined to what extent it is a core phenomenon and to what extent a border phenomenon.

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[7] An account of the various investigations of Benham's disk may be found in an article by H. Piéron, Le mécanisme d'apparition des couleurs subjectives de Fechner-Benham *L'année psychol.* 23, 1922, 1-49.

SUMMARY

(1) The color phenomena attending stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light have been investigated. The factors involved in a complex stimulus-situation of this kind can be analyzed, and a few examples have been presented to illustrate the general underlying principles. The facts have also been shown to be compatible with Troland's suggestion that the transmission of color impressions from the retina to the cortex may depend upon the modulation of the optic nerve-current.

(2) When adjacent areas of the retina are stimulated with different temporal patterns of intermittent white light, color phenomena are apt to arise at the juncture of the two areas. These border phenomena are difficult to understand because they depend upon complex stimulus-conditions which involve chromatic aberration. Other puzzling physiological processes appear also to come into play.

(3) An attempt has been made to show wherein previous investigators have failed to discover the principles underlying Benham's disk, a complicated stimulus-pattern which involves the stimulation of adjacent areas of the retina with different temporal patterns of intermittent white light.

THE RESPONSE OF WARM SPOTS UNDER SUCCESSIVE STIMULATION

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Careful investigators of the temperature senses have, in order to avoid the effects of fatigue and 'tuning,' repeated the stimulations of a given spot on the skin only at long temporal intervals. Such a procedure not only prolongs the duration of the experiments, but gives rise to technical difficulties[1] that are almost as grievous as the effects it is supposed to avoid.

To determine whether the long delays between successive stimulations are necessary for accurate results with cold spots, Drury and Dallenbach repeatedly stimulated a number of such spots at intervals of 5, 15, 20 sec. and 1, 3, 5, 20 min.[2] They found that "tradition . . . has needlessly erred upon the side of safety;"[3] that stimulations may safely be made at 20-min. intervals, and almost as safely—provided only a few repetitions are made—at intervals of 1, 3, and 5 min.; for "it is not the rate as much as the frequency of stimulation that conditions the response of the cold spots."[4]

Since it would be unwarranted to assume that the results obtained with cold spots would hold for warm spots, the present study was undertaken. We seek here to do for the warm spots what Drury and Dallenbach have done for the cold.

METHOD

Apparatus and procedure. A procedure similar to that employed in the study of cold spots was used in the present investigation. An area, 5 x 10 cm., was selected on the dorsal side of *O*'s right upper forearm. The area was shaved at the beginning of the experiment so as to avoid distractions which might result from chance stimulation of the hairs. During stimulation, *O*'s arm was held firmly and comfortably in position by means of a plaster of Paris arm-rest cast especially for him.

A Cornell thermo-aesthesiometer[5] was brought to the desired temperature by means of a stream of warm water forced through it by the pressure of the University mains. The stream, before coming to the aesthesiometer, passed through a copper coil immersed in a bath of hot water. The desired temperature of the stimulus-point was obtained by varying the water's rate of flow through the apparatus; it then remained constant within the limits of 42°—43° C.[6]

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[1] For a discussion of these see K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 409-413.

[2] M. B. Drury and K. M. Dallenbach, The response of cold spots under successive stimulation, *ibid.*, 43, 1931, 490.

[3] *Op. cit.*, 496.

[4] *Ibid.*

[5] Dallenbach, Some new apparatus, *ibid.*, 34, 1923, 92 ff.

[6] The temperature of the stimulus-point was calculated as follows. The stream of warm water was divided by means of a Y-tube after it had passed through the bath. One branch led to the aesthesiometer; the other, passing through an equal

At the beginning of every experimental period 10 warm sports were located and marked with ink. After a 20-min. rest, the spots were each restimulated 11 times over at one of the following time-intervals: 5, 15, or 30 sec., or 1, 3, or 5 min. The shorter intervals were used first, and only one time-interval was used during an experimental period.

Pressure of stimulation was kept constant by means of the spiral spring and gauge on the aesthesiometer, and rate by means of a metronome set to beat seconds. For all intervals except the 15-sec., stimulation followed a 6-fold count. *E*, the junior author, counted "one, two, three, four, five, six," silently in rhythm with the metronome and applied the stimulator at "one," removed it at "two," held it off the skin during the counts of "three, four, five, six"—while *O* gave his report[7]—and then applied it again on the count of "one," and so on until the completion of the series. With the 5-sec. interval, the spots were taken singly and stimulated in rhythm with this count 11 times over. Rests were permitted, when requested by *O*, between series with different spots. With the 30-sec. interval, the spots were taken in groups of 5, and stimulated in rotation and in rhythm with the 6-fold count. A rest was given *O* between the two groups of 5. With the longer intervals of 1, 3, and 5 min., all 10 of the marked spots were stimulated in rotation and in rhythm with the 6-fold count. The spots were also taken singly with the 15-sec. interval, but the stimulation was changed to the rhythm of a 15-fold count.

In all the series, excepting those with the 30-sec. and 1-min. intervals,[8] unmarked spots were stimulated, without *O*'s knowledge, in rhythm with the marked spots either before or after the series. Since the unmarked spots were chosen haphazardly, some were sensitive to warmth and some were not. Our object in introducing them was two-fold: we did not wish *O* to become habituated in his report, and we thought, by stimulating warm spots that had not before been stimulated, to provide him with a ready means of gauging his intensive judgments.

Since the first restimulation of every spot was made approximately 20 min. after its localization, the first responses—60 in all, *i.e.* 10 for every one of the 6 different time-intervals—were treated as results of a 20-min. stimulation-interval.[9] The 10 remaining restimulations gave a total of 100 responses for every one of the 6 different temporal intervals—600 responses for every *O*.

The experiment was performed during July, 1932. The temperature of the laboratory varied between 29°–34° C.

Observers. The *O*s were M. P. Crawford (*C*) and M. Koch (*K*), graduate students in psychology, and the senior author (*D*). *C* and *K* came to the experiment

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length of rubber tubing, to a thermometer. Since the two branches were of equal length, and delivered equal amounts of water, the temperature of one branch was assumed to be the same as that of the other. A more precise determination was unnecessary as our principal object was to keep the temperature relatively constant and below the limen for heat.

[7] These reports were recorded by a second *E*. The sequence of the stimulations was too rapid to permit a single *E* doing this work. Our thanks are due Mr. T. A. Jackson and Mr. J. Foley for serving as the second *E*.

[8] The sequence of the stimulations with these intervals was continuous, as soon as one rotation was made another was begun, consequently no checks could be introduced.

[9] This time is approximate for the longer stimulation-intervals, but for the shorter, particularly the 5- and 15-sec. intervals, the time varied from 20 to 40 min.

untrained in the observation of warmth; preliminary practice, however, familiarized them with the quality of warmth and its intensive gradations.

Instructions. Instructions, similar to those used by Drury and Dallenbach,[10] were read to the *O*s at the beginning of every experimental hour. They were as follows:

> "You will be stimulated with a warm point. Report when you experience warmth, and then describe the intensity of your experience in terms of the following four point scale: intense, moderate, mild, and weak."

RESULTS

The general results of the study are given in Table I, which shows for every *O* the number of times the warm spots responded at every time-interval. For all the *O*s and all the intervals there was a high percentage of responses: from 87–100% for *C*, 88–100% for *D*, and 82–100% for *K*. There is, however, no agreement among the *O*s on the most or least favored interval. The highest percentage—if we omit from present consideration the 20-min. interval where conditions differed from those in the other intervals—was obtained at the intervals of 3 and 5 min. for *C*, of 1 min. for *D*, and of 3 min. for *K*; the lowest percentage, at intervals of 5 sec. and 1 min. for *C*, of 15 sec. for *D*, and of 5 sec. for *K*.

TABLE I

SHOWING, FOR EVERY INTERVAL, THE NUMBER OF TIMES WARMTH WAS REPORTED AND THE INTENSITY-LEVEL OF THE REPORTS

O	Interval	Intensity level				Total
		Intense	Moderate	Mild	Weak	
C	5 sec	14	42	29	2	87
	15 sec	13	47	22	11	93
	30 sec	2	26	38	25	91
	1 min	6	32	26	23	87
	3 min	12	48	23	15	98
	5 min	13	45	24	16	98
	20 min*	23	55	17	5	100
D	5 sec	30	38	15	10	93
	15 sec	6	27	42	13	88
	30 sec	24	35	26	11	96
	1 min	18	39	38	5	100
	3 min	9	32	37	20	98
	5 min	9	28	43	19	99
	20 min*	42	33	23	2	100
K	5 sec	9	38	20	15	82
	15 sec	24	45	18	11	98
	30 sec	27	39	22	11	99
	1 min	40	34	11	6	91
	3 min	44	36	11	9	100
	5 min.	28	37	17	15	97
	20 min*	25	48	17	10	100

* But 60 stimulations were given at this interval. The values here are expressed in ratios of 100 so that they would be comparable to those at the other intervals.

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[10] *Op. cit.,* 491.

Table II groups the data into two classes of intervals: 'long' (1, 3, and 5 min.), and 'short' (5, 15, and 30 sec.). As is shown in this table, more reports of warmth were returned by all the *O*s at the 'long' intervals than at the 'short;' 283 to 271 by *C*, 297 to 277 by *D*, and 288 to 279 by *K*. The averages for the contrasted intervals, the differences, and the reliability of those differences are given in Table III. The results are statistically reliable for only one (*D*) of our *O*s. *D*'s difference in favor of the 'long' intervals is 6.67 and his D/σ diff. is 4.07; *i.e.* 99.9+ times in 100, the difference would lie in the same direction. The differences for all the *O*s, however, lie in the same direction and are consequently, when considered together, highly reliable. The conclusion, therefore, seems warranted that the longer temporal intervals are more favorable than the shorter for the stimulation of warm spots. This conclusion is also borne out by the results of the 20-min. interval (see Table I), as all the *O*s report warmth in 100% of these stimulations.

TABLE II

SHOWING, FOR THE CONTRASTED INTERVALS, THE NUMBER OF TIMES WARMTH WAS REPORTED AND THE INTENSITY-LEVEL OF THE REPORTS

(The 5-,15-,and 30-sec. intervals are classified as short; the 1-,3-,and 5-min. intervals as long.)

O	Interval	Intensity level				Total
		Intense	Moderate	Mild	Weak	
C	long	31	125	73	54	283
	short	29	115	89	38	271
D	long	36	99	118	44	297
	short	60	100	83	34	277
K	long	112	107	39	30	288
	short	60	122	60	37	279

TABLE III

SHOWING, FOR THE CONTRASTED INTERVALS, THE AVERAGE NUMBER OF TIMES WARMTH WAS REPORTED, AND THE SIGNIFICANCE OF THE DIFFERENCES BETWEEN THE AVERAGES

O	Long		Short		D	σ diff.	D/σ diff.
	Av.	σ	Av.	σ			
C	94.33	1.33	90.33	1.71	4.00	2.17	1.84
D	99.00	0.18	92.33	1.54	6.67	1.64	4.07
K	96.00	1.13	93.00	1.47	3.00	1.86	1.61

Not only are more reports of warmth returned with the longer intervals, but higher intensities are also in general reported at those intervals. More 'intense' and 'moderate' reports are returned by all the *O*s after the 20-min. interval than with any other (see Table I), and a similar relationship appears in the reports of *C* and *K* after the other intervals (see Table II). Higher intensities occur more frequently after the 'long' than after the 'short' intervals, and the lower intensities more frequently after the 'short' than after the 'long.' *D*, on the contrary, and also in opposition to his own results with the 20-min. intervals, reports higher intensities more frequently after the 'short' intervals and lower intensities more frequently after the 'long.'

Both of these facts may be readily explained, though the explanations, as do the results, stand in opposition. The fact that the longer intervals give higher intensities may be explained upon the basis of fatigue and recovery: stimulation reduces sensitivity by way of fatigue; recovery, however, sets in immediately and progresses with time; consequently, the longer the interval between successive stimulations, the greater the recovery and the greater the intensity of the subsequent response. The fact that the shorter intervals give higher intensities may be equally well explained, on the other hand, by 'tuning:' stimulation 'tunes' the spot and makes it respond more intensely; the effect of 'tuning' wears off with time; consequently, the shorter the interval between successive stimulations, the greater the effect of 'tuning' and the greater the intensity of the subsequent response. Not much is known about the effects of 'tuning,' and our results contribute little; they set a problem rather than offer anything toward its solution.

The effect of repetition is shown in Table IV, which gives for the 'long' and the 'short' intervals the number of times the spots failed to respond to every one of the 11 stimulations. The 'failures' increase for *C* and *D* with frequency of stimulation, but for *K* they remain practically constant. The cases, however, are few in number, so that this relation does not become clear until the 'failures' of the first stimulations are set against those of the later. Omitting the first stimulation, as it was made after a 20-min. rest period, the total number of 'failures' for the next 5 stimulations (*i.e.* stimulations 2–6) was 16, 9, and 18 for *C, D,* and *K* respectively, and for the last 5 stimulations (*i.e.* stimulations 7–11) 30, 17, and 15 respectively. That is, almost twice as many stimulations failed to arouse warmth in the case of *C* and *D* during the last half of the experiment as during the first half. *K,* on the contrary, reported more failures during the first half, 18 as opposed to 15. *C*'s and *D*'s results may be explained by fatigue, and *K*'s results by 'tuning.' But here again results as well as explanations are conflicting, and, adding to the perplexity, *D* and *K* have changed places, so that we are denied the refuge of "individual differences."

TABLE IV

SHOWING, FOR THE CONTRASTED INTERVALS, THE NUMBER OF TIMES WARMTH WAS NOT REPORTED AT EVERY ONE OF THE 11 SUCCESSIVE STIMULATIONS

O	Interval	Stimulations											Total
		1	2	3	4	5	6	7	8	9	10	11	
C	long	0	0	0	5	0	1	2	1	2	2	4	17
	short	0	2	0	2	2	4	4	4	4	2	5	29
D	long	0	0	0	0	0	0	0	0	0	1	2	3
	short	0	1	3	1	2	2	1	2	3	3	5	23
K	long	0	1	1	1	1	1	3	1	0	3	0	12
	short	0	3	1	5	2	2	1	3	2	0	2	21

Table V classifies the 'failures' for each of the contrasted intervals upon the basis of their early or late occurrence. With the 'long' intervals, *C* 'failed' in 6 of the early stimulations and in 11 of the later, *D* in 0 and 3, and *K* in 5 and 7. *C* and *D* exhibit the same relationship—but with smaller differences—in the 'short' intervals; but *K* shows a reversal, with 13 'failures' in the early stimulations as against 8 in the later. Whatever may be the ultimate explanation of these results, they are uniform

TABLE V

Showing, for the Contrasted Intervals, the Number of Times Warmth Was Not Reported during the Early and the Late Stimulations

Interval	Stimulation	Observer		
		C	*D*	*K*
long	2–6	6	0	5
	7–11	11	3	7
short	2–6	10	9	13
	7–11	19	14	8

for the *O*s in the 'long' intervals and they indicate once more that the 'long' intervals are preferable to the 'short.'

The effect of repetition is further shown in the different intensities reported during the early and the late stimulations. In order that these differences may be set in sharp contrast, only the intensive reports during the first and last 3 stimulations at all the time-intervals are given in Table VI. This table shows that more 'intense' and 'moderate' warmths and fewer 'mild' and 'weak' ones were reported by all the *O*s during stimulations 1–3 than during 9–11. Grouping the intensive extremes, we find that *C* reported the upper two levels during the first and last 3 stimulations 120 and 98 times respectively, *D* 128 and 59, and *K* 133 and 120. The two lower intensive levels, similarly, were reported 58 and 83 times by *C*, 48 and 107 times by *D*, and 41 and 53 times by *K*. The greater effectiveness of the first three stimulations over the last three is also shown by the 'failures.' Fewer 'failures' were reported by all the *O*s during stimulations 1–3 than during 9–11. These data indicate that the number of stimulations that a spot has undergone determines to a great extent the nature of the subsequent responses. Since this point obtrudes even when all the temporal intervals are combined, we are of the opinion that, within the limits of our experiment, frequency of stimulation is of greater weight than rate.

TABLE VI

Showing the Analysis of the Reports of the First 3 and Last 3 Stimulations

O	Stim. No.	Intensity				No Rep.	Stim. No.	Intensity				No Rep.
		I	Mo	Mi	W			I	Mo	Mi	W	
C	1	14	33	10	3	0	9	7	20	18	9	6
	2	8	31	13	6	2	10	6	17	19	14	4
	3	5	29	15	11	0	11	5	23	14	9	9
	Total	27	93	38	20	2	Total	18	60	51	32	19
D	1	25	20	14	1	0	9	9	14	26	8	3
	2	20	24	12	3	1	10	5	12	24	15	4
	3	16	23	16	2	3	11	3	16	21	13	7
	Total	61	67	42	6	4	Total	17	42	71	36	14
K	1	15	29	10	6	0	9	20	18	10	10	2
	2	19	22	7	8	4	10	16	27	9	5	3
	3	12	36	6	4	2	11	14	25	11	18	2
	Total	46	87	23	18	6	Total	50	70	30	23	7

In order to determine the consistency of the responses of the various spots and the correlation of the later responses with the first response, every one of the warm spots was characterized, on the basis of the first report, as intense, moderate, mild, or weak.[11] The responses during the subsequent stimulations showed an intensive decline, but the number of responses and the extent of the intensive decline was closely correlated with the intensity of the first response. The greater the intensity of the first response, the greater were the intensities subsequently reported; and, contrariwise, the weaker the intensity of the first response, the weaker were the intensities reported later. This correlation is also confirmed by the 'failures.' Fewer 'intense' spots, for all the *O*s, failed to respond to restimulation than 'moderate,' fewer 'moderate' than 'mild,' and fewer 'mild' than 'weak.' These results indicate that the 'spot' as well as rate and frequency of stimulation conditions the response. If only those spots reported as 'intense' upon the first stimulation are selected for experimental purposes, less attention and care need be given to the rate of stimulation.

SUMMARY AND CONCLUSIONS

The greatest percentage of reports of warmth was obtained from all the *O*s after a 20-min. interval (Tables I and II). There was no agreement as to the other intervals (5, 15, 30 sec., or 1, 3, or 5 min.). More reports, however, were returned by all the *O*s with the 3 long than with the 3 short intervals. Though the differences between the 'long' and 'short' intervals were small—being statistically reliable for but one *O*—they were in the same direction for all the *O*s.

Intensity of response parallels frequency (Tables I and II). Higher intensities were more frequently reported after the 20-min. interval than after any other, but beyond that there was again no agreement among the *O*s. Two of these reported more high intensities after the 'long' intervals, and more low intensities after the 'short.' One *O*, on the contrary, reported more high intensities after the 'short' intervals.

Both frequency and intensity of response correlate inversely with frequency of stimulation (Tables IV–VI). For every interval and every *O*, the greater the number of stimulations the fewer relatively the number of reports and the lower relatively the level of intensity reported. The differences among the intervals in frequency and intensity of response is smallest during the early stimulations and greatest during the late ones (Tables V and VI). These results suggest that frequency of stimulation is of greater significance in determining both frequency and intensity of response than the rate of stimulation, *i.e.* the time-interval between successive stimulations.

There is a high correlation between the intensity of the first response of a spot and the intensity of the later responses. Spots characterized at their first stimulation as 'intense' later give fewer 'failures,' more responses of high intensity, and fewer of low intensity than spots first characterized as 'moderate,' and similarly for the 'moderate' in comparison with the 'mild,' and the 'mild' in comparison with the 'weak.' Frequency and intensity of response seem to depend more upon the specific spot stimulated than upon either rate or frequency of stimulation.

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[11] Upon this basis *C* had 14 intense, 33 moderate, 10 mild, and 3 weak spots; *D*, 24 intense, 20 moderate, 15 mild, and 1 weak; and *K*, 17 intense, 29 moderate, 10 mild, and 4 weak.

Upon the basis of these results we feel justified in concluding that the response of a warm spot in a series of stimulations depends upon the kind of spot selected and upon the frequency and rate of stimulation. Unless the experiment is directed upon intensive differences, intense spots should be selected for stimulation as they are the most persistent and consistent in their response. If such spots are selected and if they are to be restimulated only a few times, the rate is relatively unimportant; but if the spots are unselected, or if the number of restimulations is to be large, the rate becomes increasingly important.

Intervals of 20 min. are adequate for all spots and for any number of restimulations. Shorter intervals, however, may be safely used if intense spots are selected or if only a few restimulations are to be made. If either of these conditions is satisfied, intervals of from 1 to 5 min. are sufficiently long for reliable and consistent results; if both conditions are satisfied, intervals of from 15 to 30 sec. may safely be used. The slightly greater consistency in favor of the longer intervals is more than made up by the higher accuracy in locating the same spot after a briefer interval.

The results of this study agree closely with those obtained by Drury and Dallenbach for cold spots. Temporal conditions favorable for the arousal of cold are also favorable for the arousal of warmth. Whatever difference, however, exists between the results of the two studies lies in favor of the warm spots. A greater percentage of warm spots responded upon successive stimulation than of cold spots, and the responses of the warm spots were more evenly distributed over the intensive range.[12] These results stand in opposition to general opinion, but they are in line with Dallenbach's findings in an earlier study of the temperature spots. He found "that the warm spots were more easily and more definitely identified than the cold,"[13] and concluded that "it is only an observer who is naïve in the introspection of the qualities of the temperature spots, who describes his warm experiences as 'indefinite,' 'vague,' 'illy defined,' 'less insistent,' 'more difficult to localize than the cold'."[14] With this statement, the present investigation is in substantial agreement.

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[12] A comparison of the data of the two studies reveals the following relations: 'intense' responses, 328 warm against 123 cold; 'moderate,' 668 warm against 610 cold; 'mild,' 462 warm against 674 cold; and 'weak,' 237 warm against 47 cold.

[13] K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 423 f.

[14] *Ibid.*, 424.

THE MEANINGFULNESS OF 320 SELECTED NONSENSE SYLLABLES

By CLARK L. HULL, Yale University

Most experimental studies of learning and memory in which nonsense syllables are employed are quantitative in their nature. This fact makes the relative ease of learning the individual nonsense syllables a matter of prime importance. So far as the writer is aware, no direct evidence as to the relative memorization difficulty of nonsense syllables has been published.[1] It is through its indirect bearing on this important question that the relative meaningfulness of the various nonsense syllables possesses significance, the *a priori* expectation being that the more meaningful syllables would be more easily memorized.

It requires but a glance through an ordinary set of nonsense syllables to make it clear that many such syllables do not conform strictly to the implied criterion of being without meaning. This result of casual observation has been substantiated experimentally by Glaze, who determined the association value of over two thousand nonsense syllables.[2] Glaze presented his syllables by means of a tachistoscope with a maximum exposure interval of 3 sec. At the time of presentation the experimenter also spelled each syllable. The 16 *S*s were asked to report anything the syllable made them think of. While, under these conditions, a considerable number of syllables were found to have zero association values, *i.e.* to be in some sense strictly nonsense, a surprisingly large number of other syllables were found to have 100% association value. An inspection of this latter list caused the writer to wonder whether Glaze's findings might not be in part the result of the particular technique employed in the investigation.

In order to test this hypothesis, a sample of 320 syllables was chosen for special investigation largely on the grounds that they appeared to be easy to pronounce and that 256 of them appeared to be relatively without meaning. In testing these syllables for meaningfulness it was desired to secure the evidence under conditions as closely as possible resembling those of actual learning. Accordingly, the material was presented visually by means of the writer's automatic memory apparatus, as in a regular memory experiment. The syllables were printed on light buff paper in black capital letters by a regular printing press. The letters were .25 in. high (18 pt). The window of the apparatus was 1 in. wide and 1.1 in. high. The syllables as they appeared at the window were well lighted by a desk lamp, so shaded that the direct rays from the lamp did not enter the *S*'s eyes. They were exposed in series of 16 syllables, each

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* Accepted for publication June 17, 1933. Miss Ruth Hays acted as laboratory technician throughout this investigation.

[1] It would not be difficult to set up an experiment which would yield such results, though its execution would be laborious. The major constant errors besetting such an experiment would be the progressive effects of practice on the *S*'s capacity to learn, and the greater difficulty of memorizing syllables which fall near the middle of rote series. These respective difficulties could be avoided by systematically rotating the series from *S* to *S*, and by systematically rotating the syllables within each series.

[2] J. A. Glaze, The association value of non-sense syllables, *Ped. Sem.*, 35, 1928, 255-269.

syllable being in clear view for 2 sec. The 20 series were rotated among the *S*s in such a way that each series appeared equally often in every stage of practice.

In order further to simulate the conditions of a memory experiment, each series of 16 syllables was exposed 3 times in succession, and the *S* was instructed to try to memorize them. The following instructions were presented in typewritten form:

> This is an experiment in learning a list of nonsense syllables, and not a psychological test. We are interested in certain complex relationships of the learning process common to all people, and not at all concerned with your personal reactions.
>
> Shortly after the apparatus starts you will see a 3-letter syllable in the window. You are to pronounce this syllable and those that follow it as you see them. After you have seen the list once, you are to endeavor to anticipate the syllables; in other words, as you see one syllable you are to pronounce the syllable that will follow it *before* it appears. Also, as each syllable appears, if it happens to make you think of anything in particular, tell me as briefly as possible what it is. Please do not *try* particularly to think of associations for the various syllables, but if you do think of one, be *sure* to tell me of it.

In a few cases where the *S*s seemed to be disregarding the instructions, the relevant portions were called to their attention orally.

Twenty male university students were employed as *S*s. These were selected to the extent that graduate students majoring in foreign languages were avoided. None had ever taken part in a memory experiment before. They were paid by the hour for their services.

Accurate record was made by the *E* of all reactions made by *S* on each of the 3 presentations of each syllable. It was found, in general, that the reports of meaningfulness increased with successive presentations. If a syllable was reacted to as meaningful at the first presentation, it would usually be reacted to at the two remaining ones, though there were frequent exceptions to this rule. In general there was an increase in meaningfulness at each successive presentation; for the 320 syllables as a whole, the first presentation of the syllables evoked 1040 reactions, the second 1391, and the third 1492. Usually, also, if a man gave a particular reaction at one presentation of a syllable, he would give the same reaction at a subsequent presentation, though two of the *S*s frequently gave different reactions to the same syllable. Glaze's finding that individuals differ enormously from one another as to the particular meaning which a syllable has for them, or whether it has any meaning at all was also confirmed.

In scoring the syllables for meaningfulness, it was assumed that a syllable which was reacted to on all three occasions was more meaningful than one reacted to twice, and one reacted to twice was more meaningful than one reacted to only once. Accordingly, each reaction made by an *S* to a given syllable was counted like every other, the maximum score a given syllable could receive being 60; *i.e.* 3 positive reactions by each of 20 *S*s. The percentage of meaningfulness was computed on this basis. Thus if a given syllable received a total of 24 reactions, its percentage of meaningfulness by this procedure would be 40. The percentage indices of meaningfulness for the 320 syllables are shown in Table I.

One answer to the primary question of the present investigation is found by comparing the distribution of percentage indices as shown in Table I with the values for the corresponding syllables as they appear in Glaze's report. Except for 11 syllables not found in Glaze's report and one (KIV) which appears in his table in two places, the two distributions are shown graphically in Fig. 1. There it may be seen that the present technique yields, upon the whole, much smaller values. The mean value by

TABLE I

Alphabetical List of 320 Nonsense Syllables with the Percentage of Exposures at Which a Group of 20 *Ss* Reported Meaning

BAF	62	HAB	62	LAR	42	PEM	28	VED	23	YIL	12
BAP	33	HAJ	0	LEB	37	PIB	12	VEF	0	YIM	17
BAV	48	HAN	43	LEM	35	PIJ	3	VEK	20	YIN	18
BAW	57	HAP	73	LIJ	17	PIM	12	VEN	28	YIS	63
BEJ	5	HEB	33	LOY	50	PIV	28	VEP	5	YOB	10
BEP	10	HEF	40	LOZ	38	POB	2	VES	42	YOD	18
BEV	50	HIB	28	LUB	45	POF	15	VIB	8	YOJ	0
BEW	20	HIF	13	LUJ	12	POV	28	VIF	12	YOL	8
BIP	18	HIJ	3			POY	25	VIP	5	YOM	32
BIV	25	HUJ	0	MAF	0	PUD	48	VOD	23	YOS	33
BIW	2	HUV	12	MAV	27	PUM	52	VOF	0	YUF	2
BOF	12	HUX	57	MEB	17	PUV	17	VOK	8	YUK	8
BOJ	17			MEF	20	PUY	12	VOL	53	YUN	20
BOP	25	JAT	12	MEJ	13			VOM	18	YUT	5
BOV	45	JEB	8	MEP	8	RAF	37	VUD	12	YUX	7
BUP	12	JEK	20	MEV	3	REB	48	VUK	0		
BUV	12	JEX	8	MEZ	27	REJ	45	VUM	8	ZAH	8
BUW	5	JEZ	25	MIB	7	REZ	37	VUP	7	ZAK	13
		JID	3	MIP	12	RIK	28	VUR	13	ZAL	7
DAF	47	JIH	3	MIV	0	RIX	13	VUS	22	ZAM	17
DAJ	18	JIK	15	MOF	17	ROF	32	VUT	15	ZAN	15
DAK	52	JIS	12	MOJ	7	ROJ	25			ZAW	30
DAP	62	JIX	25	MOY	12	RUP	47	WAB	18	ZAX	22
DAX	12	JIY	7	MUB	15	RUV	15	WEF	12	ZEB	20
DEJ	7	JOD	15	MUJ	5	RUY	15	WEM	15	ZED	18
DEV	48	JOF	27	MUP	15			WES	55	ZEN	33
DEZ	35	JOM	17	MUX	20	TAH	10	WIB	5	ZER	12
DIB	18	JOP	20	MUY	18	TAJ	40	WIM	55	ZET	13
DIJ	7	JUB	22			TAV	43	WOB	7	ZEX	32
DOB	23	JUF	12	NAD	12	TAY	42	WOF	2	ZIB	3
DOY	18	JUP	25	NAF	3	TAZ	23	WOJ	5	ZID	7
DUJ	7	JUR	62	NAJ	12	TEB	12	WOL	35	ZIH	5
DUR	45			NAR	48	TEF	17	WOM	48	ZIK	5
DUT	50	KEB	12	NAS	37	TEP	20	WOX	20	ZIL	10
DUY	15	KEF	2	NAX	20	TEV	3	WOY	13	ZIR	15
		KEJ	0	NEF	27	TIB	28	WUB	17	ZIT	20
		KEM	32	NEJ	15	TIJ	7	WUK	17	ZIV	17
FAJ	8	KES	20	NEM	23	TIV	5	WUM	22	ZIX	27
FAP	5	KET	52	NEP	43	TOB	35	WUP	8	ZOD	30
FAV	37	KEX	7	NEV	53	TOJ	15	WUT	18	ZOF	8
FAW	28	KIB	22	NIJ	7	TOV	7	WUX	18	ZOH	13
FEK	15	KIF	10	NIM	17	TUD	15			ZOJ	0
FEP	0	KIH	5	NIV	18	TUJ	2			ZOL	18
FEX	28	KIJ	12	NOF	10	TUP	27	YAD	15	ZOM	15
FID	43	KIP	48	NOJ	13	TUS	42	YAF	12	ZOP	7
FIK	33	KIR	17	NOY	47			YAN	27	ZOR	18
FIM	32	KIV	18	NUP	30	VAB	2	YAR	33	ZOT	20
FIP	12	KIY	22	NUV	7	VAD	12	YAS	40	ZOX	33
FOD	42	KIZ	15	NUY	13	VAH	10	YAT	40	ZOY	5
FOJ	7	KOH	33			VAM	30	YAX	15	ZUD	3
FOS	43	KOJ	5	PAB	28	VAK	5	YED	17	ZUK	5
FOT	30	KOS	38	PAF	15	VAP	52	YEK	8	ZUL	27
FOV	5	KUJ	5	PAM	28	VAR	47	YEM	13	ZUN	25
FOW	38	KUN	13	PEB	42	VAW	3	YEV	7	ZUR	20
FUB	12			PEF	17	VAX	10	YIB	7	ZUT	12
FUP	2	LAJ	7	PEJ	2	VAY	17	YIK	7	ZUX	3

Glaze's method is 42.7, whereas that by the present method is 21.3, or almost exactly half as great. The σ of Glaze's distribution amounts to 25.7, whereas that by the present method amounts to 15.1. The difference between the means of the two sets of indices is 24.6 times its probable error.

A further question arises as to whether the values for syllables high in one series will also be high in the other, and vice versa. The product moments correlation coefficient between the scores obtained by the respective methods was found to be 0.48. On the surface this indicates a remarkable lack of agreement between the results by the two methods. In order to determine whether this value was seriously attenuated by inadequacies of measurement so far as the present technique is concerned,

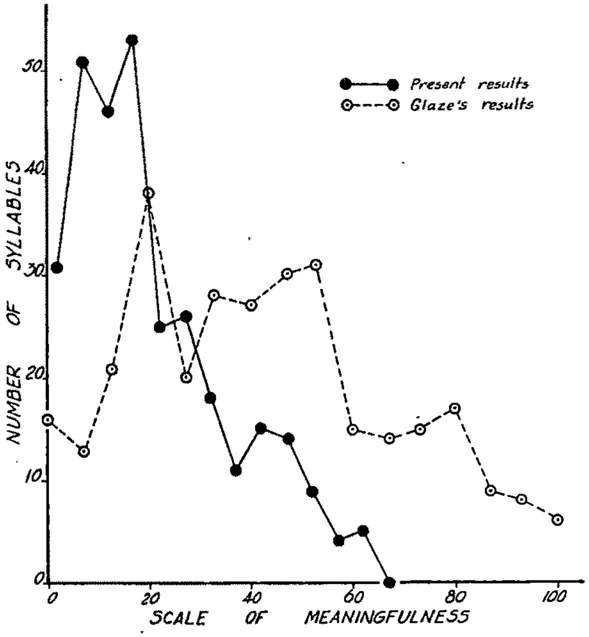

FIG. 1. DISTRIBUTATION OF 308 NONSENSE SYLLABLES WITH RESPECT TO MEANINGFULNESS AS DETERMINED BY GLAZE'S AND OUR TECHNIQUE

Note the relatively low degree of meaningfulness by the present procedure.

independent percentage indices were computed for each syllable on the basis of the results obtained from the first 10 *S*s run and those obtained from the second 10. The correlation between these amounted to 0.64. When the Spearman-Brown formula is applied to this value, it appears that the scores from the entire 20 *S*s would probably correlate with scores obtainable from 20 other *S*s similarly chosen, to the extent of 0.78. If it be assumed as a first approximation that the proportionate reliability of Glaze's results would be about the same as that of the present study, the reliability coefficient of the results from his 16 *S*s would be in the neighborhood of 0.74. Correcting the obtained correlation between the two sets of indices (0.48) for attenuation on this basis, we obtain an estimate of the true correlation to be 0.63. The value should be 1.00 if both methods measured the same process.

The difference between 0.63 and 1.00 could be attributed entirely to differences in technique only on condition that it were known that no other source of difference exists. As a matter of fact, it is very likely that the difference in geographical location from which the *S*s were recruited might have an influence. There is also reason to believe that even a few years in time might make an appreciable difference, particularly in the catch words in general use. For example, the present results were taken just after the use of beer was legalized, and one of the syllables frequently called up the name of a popular brand of beer. On the other hand, it seems likely that a substantial portion of the apparent difference may be due to differences in technique. The matter of spelling the syllables by *E*, employed by Glaze, as contrasted with the more usual method of having *S* pronounce the syllables, employed in the present study, is noteworthy in this connection.

In order to secure a little indirect light on this latter question, speech records were made of a naïve *S*'s spelling and pronouncing a selected list of 13 syllables from those here studied.[3] It was found that the spelling on the average required nearly 3 times as long as pronunciation. Moreover, with 11 of the 13 spelling reactions there was at least one interruption of vocalization, and in 7 cases there were 2 interruptions. This indicates that spelled reactions at the beginning of learning are generally chains of 2 or 3 objectively distinct reactions, whereas the typical nonsense syllable composed of a vowel between two consonants ordinarily presents a single motor unit. These facts, together with those presented above, would seem to indicate the desirability of having separate determinations of meaningfulness and memorization difficulty for each major difference in the experimental technique employed.

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[3] The writer is indebted to Harold C. Taylor for this determination.

APPARATUS

THE GLARELESS LIGHTING OF A DARK ROOM

By C. E. Ferree and G. Rand, Johns Hopkins University

Among the conditions needed in lighting a dark room the following may be noted: (1) a total absence of light for dark adaptation and other work that requires the complete exclusion of light; (2) a low general illumination giving just the degree of visibility that is needed for the convenient use of certain instruments employed for the examination of the eye, for other work in which complete exclusion of light is not required, and in general for finding one's way about without the use of more than a minimum amount of light; (3) a moderately high illumination with a downward component for use when arranging and setting up apparatus, when cleaning the room, and for other work of various kinds; and (4) the complete absence of glare. This latter has ever been the difficult feature to obtain. To provide without glare a sufficient amount of light for all needs which arise, in a room whose walls, ceilings, and floor are especially surfaced for the purpose of the complete exclusion of light, has been one of the outstanding problems in special lighting. In this connection, it will be remembered that the sharp contrast with the black surroundings gives an intolerable glare to surfaces which might otherwise not be considered of objectionable brightness.

The problem under discussion came up rather acutely in the lighting of the four dark rooms in our laboratory. These rooms required to be lighted in a way that would be suitable for any type of clinical examination of the eye and for all kinds of experimental work requiring low illumination or the complete absence of light. For purpose of description one room will be chosen as an example. This room is 19.5 ft. long, 15 ft. wide, and 12 ft. high. The walls, floor, and ceiling are surfaced with an especially prepared, flat black paint. The dark rooms are all inside rooms and the doors are carefully light-proofed. To provide for ventilation each door is fitted with light-proofed louvers. Each room is also provided with a duct connected with a fan-ventilating system which supplies air from the outside and completely changes the air in the room in a very short time. The arrangement is such that the air from this system can be turned on and off in each room separately. Also especial care has been taken to sound-proof the ventilating system.

The lighting system is a combination of direct and indirect lighting graduated to give the correct proportion of distribution of light and brightness in the upper and lower halves of the room. In the center of the room is a fixture of the totally indirect type supplied with a 60-watt lamp and at each corner of a circumscribed rectangle, 6.5 x 9 ft., is a unit of the direct type (X-ray reflector no. 575) also supplied with a 60-watt lamp. The direct units are installed on the ceiling. The installation of the indirect unit is such that the top of the reflector is 30 in. from the ceiling. The two types of unit are placed on separate

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* From the Research Laboratory of Physiological Optics, Wilmer Institute, Johns Hopkins Medical School.

circuits. The indirect unit is intended to give a low general illumination and the direct units the higher light that is needed at times on objects in the plane of work. Each of the direct units is provided with a diffusing plate and a glare baffle across the bottom. The diffusing plate which is positioned immediately above the glare baffle is of imported Belgian flashed opal glass. This glass has a high coefficient both of diffusion and transmission. It is smooth on both surfaces which adds to the ease of cleaning. The attachment of the diffusing plate and glare baffle is such that they can be easily removed for replacement of the lamp and for cleaning. As a further protection against glare we added to the indirect unit a downwardly extending canopy or glare shield which encloses all the bright parts of the socket and the neck of the lamp. This canopy, the means of suspension of the indirect unit, the outer surfaces of both types of unit, and all auxiliary parts and fittings were painted a flat black.

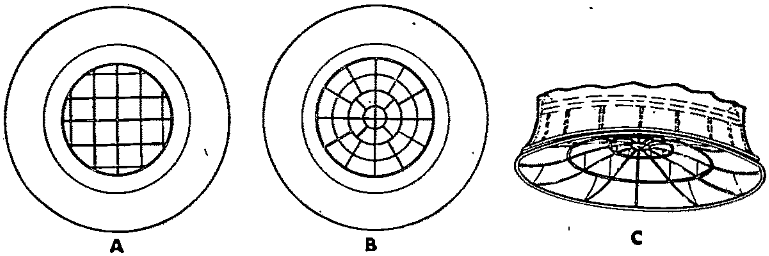

FIG. 1. SHOWING THREE TYPES OF GLARE BAFFLE

Three types of glare baffle have been used. One of these is shown in Fig. 1A. It has a cellular construction similar in appearance to an egg-crate. In fact egg-crates were used at first as baffles and are still in use in the room which is being described. The baffle consists substantially of two series of vertical partitions extending across the opening of the enclosure at right angles to each other. The partitions extending in each direction are parallel. They are in the present model spread apart by about 2 in. and are approximately 2 in. wide. Through the downwardly extending passages formed by these partitions the greater part of the light is allowed to pass to the plane of work, only such rays being intercepted as will produce glare in the eyes of those working or located in various probable positions in the room. To one directly beneath the unit, or nearly so, glare is experienced only when the eyes are directed upwards. In all other positions in the room the eye is protected in all directions. In order that the protection shall be complete, both surfaces of the partitions are painted flat black. When the unit is installed in the proper position in the room and at the correct distance from the ceiling, the opening as seen from the various positions in the room is either not luminous at all or has a dull luster or a soft, silvery sheen of such low brightness as not to cause annoyance or discomfort to the most sensitive eye.

In Fig. 1B the baffles are arranged in the form of concentric circles. Partitions circularly disposed, however, shield the eye completely only when the gaze is directed along the common radius of the circles. In all other directions glare is experienced. In order to shield the eye in all directions radial segments are inserted between the circles at suitable places. While good results are obtained with this form of baffle, we have not found it to be so entirely satisfactory as the egg-crate type shown at A.

In Fig. 1C the partitions forming the baffle are both circular and radial. The radial partitions are curved so that the appearance is that of a bottom slightly convexed upward. Intersecting the radial partitions at suitable vertical levels are circular partitions. Either illuminated or dark, this baffle gives a very pleasing effect. Both it and the type of baffle shown at B, however, obstruct the downward passage of the light more than the cellular or egg-crate construction shown at A.

Besides serving a useful laboratory purpose, this experiment in type of installation teaches a very important lesson in the principles of lighting. It demonstrates more clearly than we have ever seen before, the incorrectness of principle in both direct and indirect lighting. A dark room with its dark walls, ceiling and floor which absorb almost entirely the lesser amounts of light throws into sharp contrast the parts of the room which receive excessive amounts of light and cause the distribution of brightness in the field of view to be out of balance. That is, when the indirect light is turned on alone the ceiling and upper part of the room are light and the lower part of the room is dark; and when the direct light is turned on alone the lower part of the room is light and the ceiling and upper part of the room are pitch black. When both types of unit are turned on together, however, we have the impression of entire comfort and restfulness and of correctly balanced light and brightness in the field of view. Arranged so that the three sets of conditions can be produced in quick succession, this installation gives a striking experimental analysis of what is wrong in direct and indirect lighting, and shows what is needed to remedy the defect; namely a combination of both principles in the same unit with a completely adequate glare baffle to eliminate annoying and harmful brightness in that member of the unit which provides the downward component of light.

A brief review and statement of principles may not be out of place at this point. Thus far ceiling fixtures have been of two types: (a) open bowls, opaque or translucent, installed pendant or inverted; and (b) closed bowls, or other enclosing housings of translucent material.

With an opaque reflector turned up (totally indirect lighting) the upper part of the room illuminated is too bright, especially when ceiling fixtures are used and high intensities of light are wanted on the working plane. With an opaque reflector turned down (direct lighting) the upper part of the room is too dark and the lower part disproportionately bright, and the glare from the opening of the reflector is fatiguing and harmful to the eye. With a translucent reflector turned up (semi-indirect lighting) harmful glare results from the unit itself when the reflector is of too low density and from the ceiling when it is of high density. With a translucent reflector turned down (direct light with a translucent shade) either the opening or the sides of the reflector or both are of harmful brilliance, the ratio of brilliance of the side and the opening depending upon the density of the reflector. With both the opaque and the translucent reflector turned down there is usually also a harmful glare from the work because of the directed beam of light. If an enclosing globe or housing of diffusing glass is used, a better distribution of light and surface brightness on walls and ceiling is obtained, but the glare from the source is cut down only in proportion to the absorption of light by the enclosing glass. Because of this limitation such units invariably have much too high a surface brilliance.

In lighting interiors three practices have been followed, singly or in varying degrees of combination, for reducing glare from the source: reflection, absorption and an increase of the area of surface acting as the source. Clearly the first two are effective only in proportion as they are wasteful of light. The third has reached its highest development in flood lighting and in the lighting of interiors with indirect lighting from floor and table stands, in which case the ceiling or some other large surface acts as a secondary source for illuminating the plane of work. It has never been made satisfactorily effective, however, with the use of any type of ceiling fixture.

It is obvious that in the lighting of interiors with ceiling fixtures, some type of glare baffle is needed that will protect the eye from glare without harmful waste of light. With the demand for higher intensities of light this need has become imperative. The use of higher intensities of light has also caused an increase of glare from the work. With a given character of working surface this type of glare reaches its maximum with a directed beam of light and its minimum with completely diffused light. In order to protect the eye from glare from the work, every lighting unit should therefore be provided with a diffusing member or some means of diffusing the light.

To give satisfactory results a ceiling fixture should combine the following features. (1) It should provide a means for illuminating the ceiling and the plane of work with light in the ratio of intensities needed to give a comfortable distribution of brightness in the field of view. This can be obtained by a suitable combination of the features of direct and indirect lighting. (2) Means should be provided for shielding the eye from glare from that part of the fixture which directs the light to the working plane. (3) The beam directed to the working plane should be diffused.

All the foregoing features have been combined by us in a unit, the idea for which came from observing the effects produced by the combination of units described above for lighting a dark room. The direct incentive for devising the unit was a request from the Director of one of our leading hospitals for a glareless unit for the lighting of hospital wards. The unit is not only well fitted for the lighting of hospital wards and is giving excellent satisfaction for that purpose but is also admirably adapted to the lighting of dark rooms. For that reason we think that it should be noted here.

Drawings of the unit are shown in Fig. 2A and B. The construction will not be described in detail. In A of this figure it may be seen that the upper part of the reflecting member is curved upward and outward to direct a portion of the light to the ceiling. The lower part of the reflector directs a part of the light from the lower hemisphere of the lamp to the opening in the bottom of the unit. The remainder of the light from the lower hemisphere of the lamp passes directly from the lamp to this opening. The direct and reflected light is received by a plate of glassware which extends across the unit at a suitable distance beneath the lamp. This plate which has a high coefficient of both transmission and diffusion serves to diffuse the light with a minimum loss by absorption. In the opening in the bottom of the unit, directly beneath the diffusing plate, is inserted the glare baffle. The type of baffle used here is that shown in Fig. 1A.

In order further to protect the eye from glare a downward canopy or glare shield is provided which encloses all the bright parts of the socket and the neck of the lamp, as was the case in the indirect unit described earlier in the paper. In

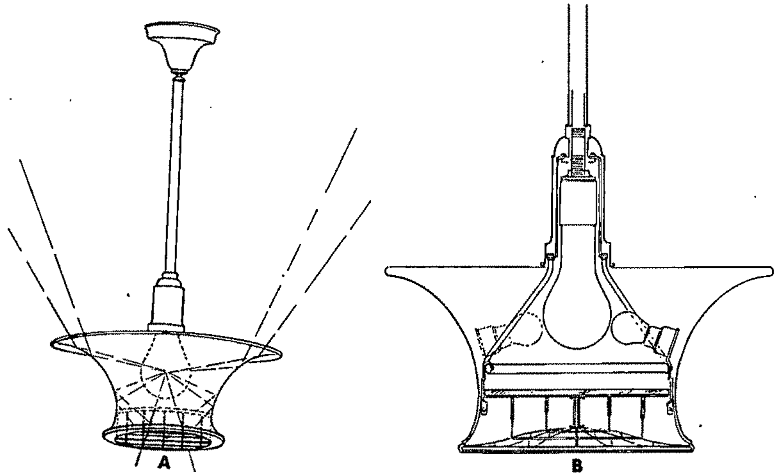

FIG. 2. SHOWING TWO TYPES OF UNIT
A shows unit without night-light attachment and with the glare baffle shown at A, Fig. 1. B shows unit with night-light attachment and with the glare baffle shown at C, Fig. 1.

the greater number of even the better lighting units in use at the present time, these have a high brightness and are objectionable and disturbing to the eye. This canopy which is attached to the lamp socket is shown in the illustration given in Fig. 2B. It may be painted in the same color as the outer surface of the reflector, in the present unit light ivory, so that it will harmonize with the reflector when the light is not turned on; or it may be painted a darker shade to harmonize with the reflector when the light is turned on. In the latter case a much better protection is afforded the eye. In units as uniformly glareless as these it is surprising to note how small a difference in brightness is noticeable and even distractingly annoying to a sensitive eye. For use in a dark room this canopy, the outer surface of the reflector, the means of suspension and all auxiliary parts and fittings should be painted a flat black.

The unit has also a provision for a low illumination, which in case of the hospital unit, is called a night-light attachment. The attachment supplies enough light for the inspection of the patient and the objects in the room. The light is in part reflected to the ceiling and from there diffused to the working plane, and in part passes through the diffusing member and glare baffles across the bottom of the fixture, directly to the working plane. When the light is turned on, no surface or object in the room has a brightness high enough to be sensed through the closed lids. The room is softly and uniformly illuminated with well diffused light of an intensity that is not high enough to waken a sleeping patient. The two sources of light, general and twilight, are on separate circuits and both can be operated from the corridor by an attendant or by the nurse in charge. This night-light attachment is of value in the use of the unit in the dark room to give the faint light that is needed for the types of work that require low illumination and in general to give enough light to see without greatly disturbing the state of adaptation of the eye.

The attachment is shown in Fig. 2B. It is mounted inside the upwardly extending reflecting member, a short distance above the diffusing plate. It consists of a circular brass tube on which at diametrically opposite points are mounted two small frosted lamps (2-candle power, 120-volt, Form I, S-8, candelabra base). The filaments of these lamps are at approximately the same vertical level as the filament of the lamp that provides the general illumination. The feed wires for the lamp are threaded through the circular tube. The tube is held in position by suitable mounts depending from the main socket of the fixture. In this figure is shown the type of glare baffle represented in Fig. 1C.

Two lamps were used for the following reasons. If only one were used, the lamp employed for general illumination with its auxiliary fittings, which remain in position when the night light is turned on, would cast troublesome shadows. Two lamps in the location described give a shadowless, well distributed illumination of optimum or very nearly optimum intensity. More can be used, however, if for any reason more light is wanted.

While the unit in question was devised for a hospital unit, the same features of glareless, well balanced distribution of light and brightness in the field of view give it merit for other lighting situations. Without the night-light attachment the unit serves excellently for commercial and home lighting. In combination with the night-light attachment it has additional uses in the lighting of bedrooms and other rooms and of halls and corridors in which a low as well as a high illumination may be needed.

The unit combines the features of diffused and glareless direct lighting with indirect lighting. The former provides for a strong downward component of diffused light without glare on desks, tables, instruments and other objects in the lower half of the room, and the latter gives a pleasing illumination to the upper half of the room and in addition an overlay of highly diffused and well distributed reflected light on objects in the lower part of the room. The purpose has been to secure in one unit the best features of both direct and indirect lighting in supplementary relationship. Neither type of lighting is complete in itself.

A means of illumination of this type for the dark room not only removes many of the annoyances and inconveniences ordinarily associated with dark room work, but it opens up new possibilities for the utilization of a standardized low illumination for many types of laboratory and testing work, for the use of certain instruments in the study of pathologic cases and for other purposes for which a low illumination is required.

A DEMONSTRATIONAL APPARATUS FOR THE PATELLAR TENDON REFLEX

By Robert G. Ruhl and Harrison Musgrave
University of Southern California

The following simplified apparatus for demonstrating the patellar-reflex measures the strength of stimulus, the extent of motor response, and the latent time in 1/120-sec.

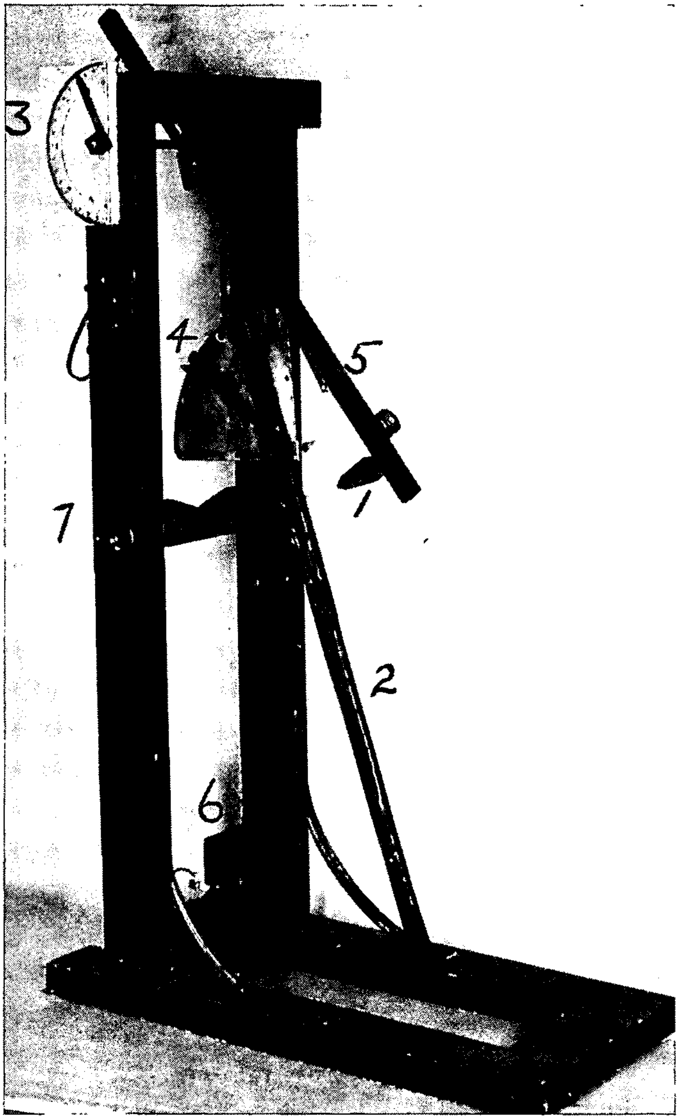

Fig. 1. A Demonstrational Apparatus for the Patellar Tendon Reflex

The frame of the apparatus is made of 2 in. x 4 in. lumber braced with metal right-angle brackets. The bar (7) supporting the *S*'s leg (see Fig. 1) is hollowed on its upper side to hold the leg in a fixed position. A metal shaft inside the bar extends through vertical slots and is fitted with a wing-nut at either end to adjust the height of the leg. The hammer-head (1) is a wedge-shaped piece with a striking edge 3 inches long. The hammer-arm is slotted at the upper end to vary its length. It is fixed to a block with a wing-nut. This block is mounted solidly to a shaft which extends through, and which is free to turn in, the frame. One end of this shaft is fitted with a pointer (3) which moves over a scale. Thus a measure of the stimulus is indicated by the number of degrees through which the hammer drops. The extent of the leg-movement is measured by a lever (2) fixed below to the foot. The pivot around which this lever turns, with its angular scale, is adjustable up and down so that it may be centered opposite the knee-joint. The pointer (4) carried by the lever over its scale remains at the lowest point when the lever moves down and back. The pointer must be reset to zero for each trial. A wooden shoe, fastened with rubber bands to *S*'s foot, has a projection on one side which serves to move the lever.

The latent time is measured with a Klopsteg impulse counter.[1] The simplest arrangement is to mount a make-switch on the stimulus hammer-arm (5) and a break-switch behind *S*'s heel (6). Then the circuit through the counter is closed from the time of the stimulus-impact to the beginning of the leg-movement. A projection from the back of the wooden shoe holds this break-switch closed until movement begins. A more satisfactory method is to use two break-switches. The two-break circuit may be connected to the psychodometer, or to a relay similar to the one used with that instrument.[2]

A CONDENSER-CHARGE TIMER

By WM. D. MOEDER and JOHN G. JENKINS, Cornell University

In an earlier note, reference was made to a condenser-charge timer which was being used to control tachistoscopic exposures in the Cornell Laboratories.[3] Inquiries from other universities have prompted us to present at this time the details of this device without waiting for more extensive checks on its accuracy. Meanwhile it may be noted that extensive determinations here and in two other psychological laboratories seem to indicate that its average error is of the order of one or two milliseconds (ms.) in 500. The timer is accurate, easy of construction, and inexpensive—a trio of virtues which may appeal to psychologists in general at the moment.

As employed by the authors, the two models in use at Cornell are laid out on small panels which are situated within easy reach of the experimenters. Reference to the schematic wiring diagram (Fig. 1) will reveal the necessary parts, which, it will be obvious, are few in number and inexpensive. The 0.1 mfd. condenser is critical in value and should be a mica condenser of the best grade obtainable. The relay we employ is of the ordinary telephone make-and-make type. This operates well on the output of a 171-A tube with 90 volts on the plate, and its contacts are capable of carrying the 110 volt current used for both sets of lamps in our tachistoscopes. The blocking resistor (R_2) is employed only as a protective device and may be omitted without affecting the operation of the circuit. The key is easily constructed by adding an off-normal contact to any available key, thus permitting the discharge of the condenser after each exposure. Since filament voltages are critical, a voltmeter should be inserted in the filament lead. Needless to say, old batteries with high internal resistance cannot be expected to produce accurate results.

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[1] P. E. Klopsteg, An impluse counter, tallying device, and electric stop watch: A useful laboratory instrument, *J. Opt. Soc. Amer. & Rev. Scient. Instr.*, 19, 1929, 345-349.

[2] M. Metfessel and H. Musgrave, *Instructor's Guide for Demonstrations of Psychological Experiments,* 1933, 3-4.

[3] J. G. Jenkins, A simple tachistoscope of many uses, *supra,* 150.

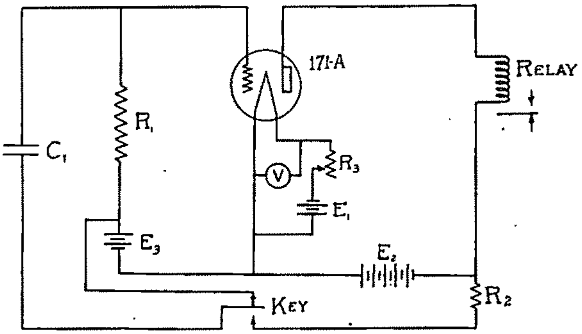

FIG. 1. WIRING DIAGRAM OF CIRCUIT OF CONDENSER-CHARGE TIMER

C_1 = 0.1 mfd. condenser
R_1 = critical resistor (½ to 20 meg.)
R_2 = blocking resistor (½ meg.)
E_1 = A-battery (6 volts)
E_2 = B-battery (90 volts)
E_3 = C-battery (16 volts)
V = voltmeter

Variations in time are obtained by employing as R_1 resistors of various values. Since the times obtained are a function of the characteristics of the individual tube employed as well as the resistors, we shall not attempt to give a table of time-equivalents. Although the device must be standardized for a particular tube, it may be of some value to know that the tube we are using at present yields almost a straight-line function from ca. 60 ms. for a 1 meg. resistor to ca. 300 ms. for a 10 meg. resistor.

The principle of operation may be briefly described as follows. Before the key is closed, the grid and negative C-battery terminal (E_3) are at the same potential; this is sufficiently negative so that the plate current will not actuate the relay. The experimenter now closes the key and holds it closed until the click of the relay informs him that the exposure is complete. While the key is closed, a charging current flows to the condenser through the resistor (R_1) causing the grid to become instantaneously positive with respect to its steady condition, and causing a plate current large enough to actuate the relay.

If the grid is brought to a positive potential at which a grid current will flow, the grid impedance will act as a shunt resistor to the resistance (R_1) and will hasten the charging rate of the condenser until the contact potential of the tube is reached. At this point the grid resistance may be assumed to become infinite, the only resistance affecting the charging rate of the condenser from this point to the point at which the relay will open being the resistor (R_1). Since our tube is operated at such values of E_1 and E_2 that the relay operates at a grid voltage less than the contact potential of the tube, and since R_2 is so chosen that the grid never actually becomes positive, the entire cycle tends to occur with the resistor (R_1) as the only controlling element. This reduces the chances of inaccuracies due to changes in tube characteristics. Since the charging current through R_1 decreases logarithmically with time, the grid potential becomes increasingly negative and a point is reached at which the plate current is insufficient to hold the relay closed, and it returns to its original position.

As stated before, the release of the key permits the discharge of the condenser by way of the off-normal contact, and the device is ready for the next exposure. The length of the exposure is in no sense a function of the time for which the key is held down, except that it must be kept depressed until the relay has released. A feature of the apparatus is its flexibility and wide range of possible application. It may be used to make and break a single circuit, or it may be employed, as it is here, to secure reciprocal make-and-break actions in two circuits. By changing the values of condenser and resistance appropriately, almost any range of times may be secured, so that the timer may actually be used as a delay-circuit for times even several minutes in length. At the very least, it can replace various laborious methods of securing accurate exposure times of extremely short duration.

NOTES AND DISCUSSIONS

Revision of a Biography

In 1923 I published a biography of Sigmund Freud,[1] which appeared in English translation in 1924, and which, because of the nature of the book, received wide attention in scientific circles. It is to the present day cited by psychologists in their lectures and publications, and I have reason to fear that a number of psychologists, rather remote from the field of psychoanalysis, derive a major part of their knowledge of this subject from my book. This has been a source of constant embarrassment to me for I have, in the ten years that have elapsed since the appearance of the book, changed my opinion considerably concerning psychoanalysis and its founder, and therefore can no longer stand sponsor for the errors and misrepresentations which I have come to recognize as such.

I have been repeatedly urged by my American friends to publish a revised edition of the book, and this seemed at first to be the simplest way out. As I went over the book, however, for this purpose, I realized that it would be a sheer impossibility for me so to transform the work that it would express adequately my present position in regard to Freud and his school, to which I belong. The tone of the book throughout is a striking example of ambivalence and it is beyond my power to entirely eradicate it. Passages in which I pay high tribute to Freud alternate with others which hurt—not only his work, a brunt which every scholar must bear, but his personality, passages which, I was later to discover, partly through my own observations, were based on false assumptions. The book is so written that it is perforce welcome to all who seek rationalization for their own ambivalent attitude toward psychoanalysis.

I have since given expression in various books and articles to my change of view, yet none of these works have been able to obliterate the impression that I created upon scientific circles by my publication of 1923. I had hoped and with some ground for the hope that the publication would gradually fall into oblivion. For this reason I kept silent, thinking it best to return no more to what I regarded as a sort of youthful indiscretion. While, however, the purely scientific portion of my book, with its truth and errors, scarcely needs further comment, it is precisely the personal content of the book which to my chagrin continually reappears.

I say in my book that Freud has an overweeningly good opinion of himself, the Jehovah complex; that he is a despot who will not tolerate deviations from his system; that he brings his disciples into hypnotic dependence upon him; that he repels his friends and especially if they are men of importance themselves; that he rarely abandons errors which are obviously such, is not free from cryptomnesia, and in his psychoanalytic policy pursues ways not always above reproach. These words of disapproval—condensed here to a few lines—are spread throughout my book, over 287 pages, and are again and again interspersed with frank hero worship. However, this does not serve to mitigate them; quite to the contrary. The reader inevitably arrives at the conclusion: Behold one who so honors him, and yet . . .

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[1] F. Wittels, *Sigmund Freud: His Personality, His teachings and His School,* 1924, 1-287.

The letter which Freud wrote me after the German edition had appeared, and with which in an authorized abstract I prefaced the English translation, shows that he, astonishingly unmoved, scarcely noticed the defamatory passages and approached this biography, of which before its appearance, he knew nothing, with apparently complete objectivity, as though it had nothing to do with his person but only with a matter which was, in general, science, in particular, psychoanalysis. Yet he did more than that. I had been pursuing my way apart from his pupils for a number of years—five years of war had lengthened the time. After the appearance of my book, which certainly was anything but a good introduction, he drew me back to his school and gave me the opportunity to relearn and to see for myself whether my opinion of his personality and his teaching was correct or not. I may say that never have I met a worker who so completely and effortlessly disappeared behind his work.

(1) Jehovah. Before the appearance of my book there could be found, with the exception of scattered remarks in his works *The Interpretation of Dreams,* and *The Psychopathology of Everyday Life,* only Freud's *On the History of the Psychoanalytic Movement,*[2] which contained personal statements concerning the author. Later there appeared his autobiography which gave in terse, reserved manner, certain facts of his life.[3] It reads like a protocol that leaves it to the reader to decide for himself whether it is a great man who speaks or not, and how great he perchance may be. Among all psychoanalysts, myself included, he is perhaps the only one who is not intoxicated with the method and its results. It has always remained questionable to him whether psychoanalysis—a matter of which we, his pupils, are all firmly convinced—would change the spiritual face of the world. He has always considered himself a "foreign body in German intellectual life," has allowed himself to be convinced of the opposite by Thomas Mann only out of courtesy, and has proved himself right, as the most recent events in Germany show—temporarily right, we hope.

If one speaks of his world renown, he answers not only with the wisdom of Solomon that this is vanity, but he reminds us that many an "immortal" together with his theories, has been forgotten in an incredibly short time. Therefore, albeit I then wrote that he looked upon himself as Jehovah, it now seems to me that it is his disciples who bestow upon him a rank against which he does not defend himself because it is a matter of indifference to him. I had moreover at that time no other foundation for my assertion than some mistakes (Freudian "parapraxis") and interpretation of dreams which Freud had imparted from didactic motives.[4]

(2) The Analysis of Freud. I employ in my book psychoanalytic methods with which to interpret certain of Freud's actions. It is, however, an unwarrantable act to strip bare the living or even departed contemporaries whose relatives are still alive, by unveiling their unconscious without permission to do so. I did not, as Freud himself stated to me in his letter, carry this indiscretion very far. Yet, as a matter of principle, it is not right to do this, just as it is not right to operate upon a person against his will. I set here a bad precedent,[5] which has led others to follow

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[2] S. Freud, *Collected Papers,* 1, 1924, 1-359.

[3] Freud, *Selbstdarstellung,* 1930, 1-25; *Gesammelte Schriften,* 10, 1925, 1-447; and *The Problem of Lay Analysis,* 1927, 1-316.

[4] See pp. 16 and 28 of my book.

[5] *Op. cit.,* 36, lines 17–19; 72, lines 18–26; 100 f.; 103 ff.; and 233, lines 2–20.

me in a rather malicious manner. Herein, there lies a temptation which—aside from ethical objections—readily leads to ways of error, for without the free ideas and associations of the victim the psychoanalytic truth cannot be reliably discovered. If, moreover, a polemic purpose enters into the undertaking, precious little of scientific glory is left.

(3) Orthodoxy. Except for a few ill-fated attempts, no university has yet dedicated a chair to the newly created science called psychoanalysis by its creator. Hence, any charlatan can with impunity call himself a psychoanalyst. It was the good fortune of Freud's theory that its founder possessed the power of organization to assure his disciples places of work. His power of organization maintained sufficient strength to surround the civilized globe when scientific interest in psychoanalysis awoke in all cultural lands. Freud's theory must be preserved from disintegration and dilution. The International Psychoanalytic Association attends to that with its numerous branches, its publishing house and periodicals. It must be left to Freud and his disciples, I do indeed believe, to decide what psychoanalysis is and how it can be learned. Freud said even as far back as 1914, of his former pupils: The earth has room for all, if only they did not disturb him and his work. These former disciples, who have changed their scientific points of view widely enough to separate from the psychoanalytic path of research, like Jung, Adler, Rank, themselves no longer maintain that they are psychoanalysts. Why, therefore, differentiate Freud's psychoanalysis as "orthodox," as if it were not a matter of scientific conflict and defense, but one of faith and bigotry? Here, I have, I am afraid, indulged in a thoughtless mockery of patient scientific labor carried on for almost forty years under great sacrifices—even monetary sacrifices—in the face of a hostile environment. All who do not recognize this work as scientific have a right to say so. It should, however, be at least understood that Freud's school itself continues to believe that it is engaged in a scientific pursuit and that it moves as honorably and freely in the service of the truth as any other school. On May 10, of this year, Freud's works, among others, were openly burned in Germany. In this action one sees plainly on which side orthodoxy lies. Many others would consign Freud's works to the bonfire were they not ashamed to do so. The particular sort of resistance to the teachings of psychoanalysis, which "disturb the world's slumber," has been so often set forth by us that I need not revert to it here.

I state in my book that Freud holds his consultations behind closed doors. Naturally, he does so. What purpose could it serve after so many years of specialized work to admit people who are the more vociferous the less they understand? But when, because of this, and because he, allegedly, as I state in my book, "tolerates no deviation from his theory," I called him a despot, I wronged him. On reading our periodicals one may see how violent and active is the scientific dispute within the borders of psychoanalysis. Every doctrine adheres to certain fundamental principles, otherwise it would not be a doctrine. We psychoanalysts have united upon certain basic facts, like repression, resistance, transference, the Oedipus complex and its derivatives, the repetition compulsion and others, and we feel that we waste our time when we strive with those who have inadequate knowledge of our theories over the validity of discoveries long tried and tested. Every petty chief of a medical or psychological institute that is conducted according to his ideas is more of a despot than is Freud. I can only hold myself up again as an example of this, when

Freud, after the appearance of my, to put it mildly, imprudent book, drew me back to his fold.

(4) Cryptomnesia. I furthermore wish to retract my accusation of cryptomnesia.[6] Under the influence of ambivalence, I charged one of the most original thinkers of our time with plagiarism, albeit unconscious plagiarism. To be sure, we find ideas of Freud in Nietzsche and Plato, but such thoughts are in the very air that we breathe. No student is shut up in a vacuum; he founds his ideas always upon the concepts of others. Alfred Adler, in his own fashion, described the aggressive instinct and pursued the psychology of the ego before Freud had definitely comprehended and elaborated his own theories on the subject. Freud, however, grasped this problem so much more profoundly when he did recognize it, and was able, moreover, to subject it so completely to his own theory, that one can hardly say that he was influenced by Adler. Still less was he influenced by C. G. Jung when he wrought the "higher nature of man" into the magnificent conception of the superego.

(5) Psychoanalytic policy. I should like to add to what I have said in (3) above, an example of Freud's psychoanalytic policy as given in by book,[7] and present it in a new light. I refer to Freud's attempt in 1910 to put the leadership of the psychoanalytic movement into the hands of the Swiss. I quote from my book what he said at that time to his pupils in Vienna: "Most of you are Jews, and therefore you are incompetent to win friends for the new teaching. Jews must be content with the modest rôle of preparing the ground. It is absolutely essential that I should form ties in the world of general science. I am getting on in years, and am weary of being perpetually attacked. We are all in danger." Seizing his coat by the lapels, he said: "They won't even leave me a coat to my back. The Swiss will save us—will save me, and all of you as well." I no longer know precisely why I cited just these words of Freud's. Perhaps I desired to show that the great man under certain circumstances was faint-hearted. Today I am glad that I have rescued this episode from oblivion, for every one is aware at the present time, since the latest events in Germany, that twenty-three years ago, Freud had truly prophetic vision and that his fear was not for his own life but for the life of his teachings; he recognized a danger to its dissemination which at that time he alone realized. It is, however, one thing to recognize a danger and another to flee it. Freud could not escape it; he had to fulfill his destiny.

(6) Freud's disciples. That Freud's disciples, that is, the members of the International Psychoanalytic Association, are better and above all, different from the description of my book, may be apparent, perhaps, from the fact that today I count it an honor to number many of them among my friends. One must not, on reading my biography of Freud, fail to observe that the author at that time was unawaredly unfolding the psychology of Lucifer. The five years of war service had also made me somewhat bitter perhaps. Chiefly, however, I stood when I wrote this book under the influence of Wilhelm Stekel, who had analyzed me a short time before. I, together with most analysts with a sense of responsibility, who know the power of "transference" in analysis, would urge their analysands to desist from writing a book in which their analyst plays a chief rôle. Stekel did not do this. In some pas-

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[6] *Ibid.*, 195, line 28; 201, line 6.

[7] *Ibid.*, 139 f.

sages of the book can be found doubt of my objectivity, and expression of my own feeling that I was unduly influenced by Stekel. Thus on page 217: "I agree that I may be prejudiced in his favor, and that I am not in a position to give an unbiased opinion in this affair of Stekel versus Freud." Nevertheless, Stekel is glorified in my book. Today, I would no longer write in this vein. I am not in a position to say anything concerning Stekel. It is not meet to attack one's former friends, it justifiably creates a bad impression. However, I may say that I certainly no longer regard him as the "most successful and most distinguished disciple of Freud and his logical inheritor."[8]

(7) Suicide of analysts. I think that I should make further explanation of my statements concerning the suicide of analysts. I spoke of Otto Weininger, the author of the famous book entitled *Geschlecht und Charakter,* who shot himself. He died in 1903, hence before it was in any way possible for him to have been influenced by psychoanalysis as a therapeutic means, nor did I state that he had been.

Of the three other analysts whom I mentioned as having taken their own lives, not one of them had been analyzed, which was the chief reason for my having brought up the matter. An analyst who has not himself been analyzed is in danger, be it suicide or otherwise. One can refer to this in my book.[9] A disagreeable feeling was aroused among my colleagues by the following passage: "Three among the distinguished psychoanalysts I have personally known . . . ended their own days. . . . . Others may follow in their steps." The last sentence was interpreted as a more or less veiled challenge for some analysts to go hang themselves. I do not actually know whether in those bygone days, the will to a malicious challenge was not present rather on the side of my colleagues, but I may say with gratification that today such a sentiment is no longer attributed to me. Nevertheless, I expressly state that it was my desire to emphasize and over-emphasize how important it is for analysts to become aware of their own complexes before they call forth those of other people and permit these to react upon themselves.

In conclusion, I merely wish to state that psychoanalysis has since 1923, in which year my biography was written, made advances which I may compile in the near future and publish separately. It would not be fair to compress this important material into the frame of a short report which is chiefly designed to revise and rectify a previous publication.

I do not believe that I shall have recommended my book through this revision, but such, after all, is not my purpose. The book is too much an outburst of temperament to be scientifically above reproach.

New York City FRITZ WITTELS

THE DEPENDENCE UPON CHANCE FACTORS IN EQUATING GROUPS

In a recent analysis of experimental studies of learning and memory the writer was impressed with the frequency with which students in two sections of the same college course were used as control and experimental groups in psychological investigations.[1] The validity of the assumption that two groups of 25 or 30 students each represent comparable average abilities has been questioned before. The following statistical manipulations were undertaken to present empirical evidence bearing on this point.

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[8] *Ibid.,* 17, 225.

[9] *Ibid.,* 67 f.

[1] S. M. Corey, A summary of certain factors in current investigations on learning and memory, this JOURNAL, 44, 1932, 190-192.

One thousand cards were marked with I.Q. in such a manner as to represent a normal distribution of intelligence. All of these cards, shuffled thoroughly, were then dealt four times, (1) into 40 groups of 25 cards each, (2) into 20 groups of 50 each, (3) into 13 groups of 75 each, and (4) into 10 groups of 100 cards each.

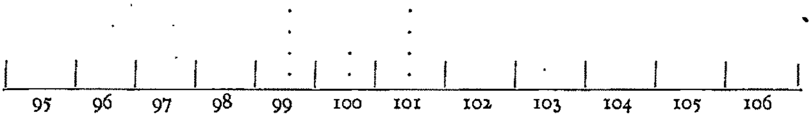
The purpose of these chance divisions of the large symmetrical group into smaller groups was to determine whether the various smaller groups of any given size would be comparable with respect to the mean I.Q. If they were, then it would seem reasonable to believe that other groups of the same size, selected by chance from a large symmetrical group of approximately 1000 would represent comparable average abilities. In other words, the use of two college classes as control and experimental groups, provided the classes were selected by chance from a much larger symmetrical group, might be justifiable.

The mean I.Q. for the 1000-card group was, of course, 100. The most probable expectation for the mean of each of the smaller groups would be the same. The degree in which this expectation was actually fulfilled is as follows:

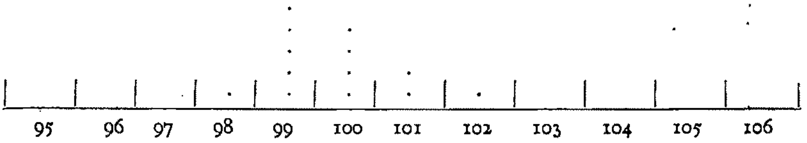
Size of group	Number of groups	Mean I.Q. for all such groups	S.D. of means for such groups
25	40	100	2.34
50	20	100	1.20
75	13	100	1.08
100	10	100	0.75

Here are shown the results of averaging the means for the 40 groups of 25, the 20 groups of 50, etc., as well as the standard deviations for each distribution of means. The departure from theoretical expectations was very slight, even in the case of the smallest groups. As the groups increase in size they become more comparable, *i.e.* the S.D. of the means decrease. The S.D.$_{m}$ for the ten groups of 100 is less than one-third as large as that for the 40 groups of 25.

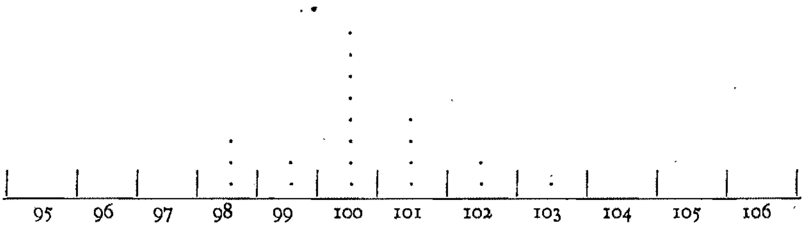
The following figure gives the distribution of the mean I.Q. for each of the size-groups.



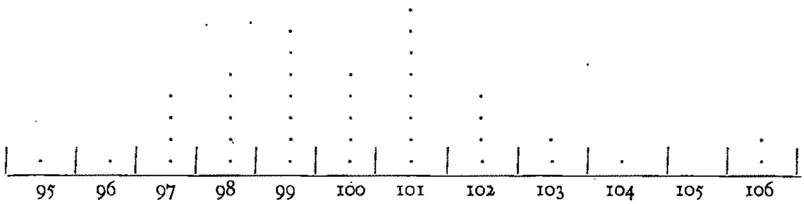
Distribution for 10 groups of 100 each (S.D.$_{m}$=0.75)



Distribution for 13 groups of 75 each (S.D.$_{m}$=1.08)



Distribution for 20 groups of 50 each ($S.D._m = 1.20$)



Distribution for 40 groups of 25 each ($S.D._m = 2.34$)

The difference between the two extreme means of the group containing 25 cards is 11 ± 3.3. These two extreme groups, although selected by chance, are obviously not comparable. But in any chance selection it might be very improbable that two such extremes would be paired for experimental purposes. In order to determine further whether groups of 25 selected under the circumstances of this study are comparable, the means for all such groups were placed on cards and put in a box. Two of these means were then selected at a time until 94 such selections had been made. The two means for each of the 94 pairs were then compared to see if significant differences existed between them. A difference was considered to be significant when it was three times its probable error. The result of these comparisons is given below in terms of the number of chances in 100 favoring the conclusion that a significant difference existed between the two means selected at any one time.

CHANCES IN 100 THAT THE DIFFERENCE BETWEEN THE MEANS OF ANY PAIR IS SIGNIFICANT

	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85–89	90–94	95
No. of such pairings	9	12	14	9	7	13	6	7	8	9

Although none of the differences was three times its standard error, there was a large number of cases (43) wherein the odds were anywhere from 3 to 1 to 10 to 1 in favor of a significant difference between the means compared.

It would appear from the above demonstration that the practice of using various sections of the same course in psychology for experimental and control groups is unsound. Admitting the convenience of this procedure, the probabilities are that fairly significant differences might have existed between the groups with respect to the variables studied prior to any experimentation. This is particularly true when the classes used include no more than 25 individuals. The magnitude of these differences between actual sections would be greater than those apparent in any manipulation of a normal distribution of 1000 cards. This is true for a number of reasons. First, the sections of a University course-group are rarely recruited from a group as large as 1000, and the smaller the original group the more likely will smaller groups selected from it show asymmetry. Secondly, even though attempts are sometimes made to bring it about, the allocation of students to the several sections is seldom a matter of chance. The instructor, the hour of the class-meeting, and the other persons to be in the class are all selective factors. Finally, no matter how large the original group may be, it will probably not represent a symmetrical distribution of abilities, a fact which would of course affect the symmetry of the smaller groups too.

University of Nebraska STEPHEN M. COREY

THE EIGHTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association held its eighth annual meeting on Thursday, Friday, and Saturday, May 18, 19, and 20, at the Iowa State College, Ames, Iowa. All of the sessions took place in the Memorial Union Building except the public address of Professor C. Spearman and the Symposium on Industrial Psychology which were held in the Physics Auditorium. About 350 psychologists were registered for these meetings and they came from a widely distributed geographical area. Professor Horace B. English of Ohio State University officiated as president. Altogether there were six sectional meetings with two sessions running simultaneously throughout Friday and on Saturday morning during which 38 papers were read. These had been selected by the program committee from more than twice that number submitted. In addition during the usual Saturday afternoon session devoted to reports from laboratories, representatives from ten institutions gave a synoptic account of the research work under way.

The meetings opened with an address by Professor Harvey A. Carr (Chicago) on "The quest for constants" in which he summarized various attitudes customarily taken in learning experiments and made a comparative study of results obtained. He concluded with the concept of numerous specific learning curves in place of one stereotyped form of the curve. It is hazardous to assume that data obtained in connection with experiments on learning and forgetting could be treated independently of the specific conditions surrounding each experiment. Over 150 persons attended the lecture which was given in the Great Hall of the Memorial Union Building. On Friday evening the annual dinner was held with Dean Carl E. Seashore (Iowa) serving as toastmaster. President R. M. Hughes (Iowa State College) gave the address of welcome and Dean Charles E. Friley (Iowa State College) read a paper on "Psychology in a technical institution." The address of the President, Dr. Horace B. English (Ohio State), followed. He dealt extensively with the dualistic notion of the mind-body relationship and proposed a type of psychology in which the activities of the entire organism would hold a major place. At the close of the address the Association was pleasantly entertained by Mr. Charles Cooney, a professional magician, of Ames. This staged performance was camouflaged on the program under the title, "Practical demonstration of visual perception with attentional misdirection" and its real nature was known only to the committee. The annual business meeting was held on Saturday afternoon during which reports from the officers and from the Executive Committee were read and approved. The invitation of Purdue University to hold the next meeting of the Association at Lafayette, Indiana, was accepted. Two informal symposia were organized, one on learning, under the chairmanship of Professor H. B. English (Ohio State), attended by 30 persons, and the other on emotions, under the chairmanship of Professor Christian A. Ruckmick (Iowa) with over 50 persons in attendance. In the symposium on learning the chairman raised the methodological question of proceeding from relatively gross forms of learning response and by experimental means breaking these down into component parts in contrast with the effort to begin with the supposed neurological basis of learning and working up to discover their function in the learning process. During the discussion it was of interest to note several concrete instances of retention in which there was apparently no evidence of forgetting taking place. The symposium on emotions centered on matters of definition of emotion and the possibility of distinguishing one form from another in terms of electrical manifestations from the body. The chairman pointed out that the more primitive forms genetically considered gave a more definite response. Discussion also revealed the possibility of using moving stimuli, such as the motion pictures, to produce effective emotions. The group was interested mostly in matters of technique and procedure and in the description of results thus far obtained.

About 250 persons listened to an elaborate symposium on industrial psychology which was held on Saturday evening under the chairmanship of Professor J. E. Evans (Iowa State). The following four papers were read: "The analytical method of job-grading," F. A. Kingsbury (Chicago); "Measuring the ability to operate automotive vehicles," A. R. Lauer (Iowa State); "Psychology and occupational adjustments," D. G. Paterson (Minnesota); and "The riding qualities of an automobile," A. Swope (Purdue). Two principal techniques of job-grading commonly employed in programs of standardization of salaries were compared by Dr. Kingsbury. The possibilities and limitations of the analytical technique were discussed in terms of a recent classification of some 500 office positions based on three aspects referring primarily to duties and responsibilities and on four items representing minimal essential qualifications of the position analyzed. Dr. Lauer presented the results obtained by the use of specialized spectrophotometric methods in testing drivers of automotive vehicles. Normal subjects over-estimate the short wave-lengths and under-estimate the long wave-lengths when the stimuli are of equal intensity. Color-blind persons match colors independently of intensity with almost the accuracy of normal individuals. The psychological ratings of 3700 individuals over a period of one and a quarter years were discussed by Dr. Paterson. He found that the general traits, including education, ability, and interests, averaged lower in the unemployed than in the employed classes and pointed out that psychologists will play an increasing rôle in employment agencies in the future. Professor Swope outlined five studies which he had completed on the mental and bodily effects of the riding qualities of automobiles. The mental effects come largely from the elimination of noise, increased comfort of sitting, and the increased field of visibility. He devised a "steadiness meter" to test individual differences in driving and found that adaptation to a definite type of car produced lower readings.

Preceding this symposium Dr. Spearman in a public address outlined his program of testing one thousand school children with a battery of 95 measures under the title, "The plan now in operation for an extensive investigation of unitary traits in ability." The actual testing in accordance with this program is actually under way now at the Mooseheart Laboratory. Dr. Spearman briefly explained his three principal factors of *g*, *o*, and *p* which are used to designate inherent common modes of response. A special exhibit of lantern slides, motion pictures, apparatus, and demonstrational charts was arranged for in connection with the meetings.

By way of diversion the Psi Chi national fraternity held a luncheon in the tea room of the Home Economics Building for its members and guests on Friday noon. In the afternoon of that day the wives of the faculty of the Department of Psychology and friends served tea to members of the Association and guests in the Modernistic Room of the Memorial Union Building. A special trip to the Ledges State Park was also provided for on Saturday afternoon by the wives of the faculty in the Department of Psychology.

Section A under the chairmanship of H. A. Carr (Chicago) opened with a paper by H. Gulliksen (Mooseheart) in which he developed a new rational equation of the learning curve which synthesizes three differential equations that have previously expressed Thorndike's law of effect. It makes possible an analytical distinction between initial attainment and learning ability in a given task and also analyzes the effect of the amount of initial training upon the degree of transfer. N. Reiter (Wisconsin) followed with a paper on "The relation of gross bodily movement to problem-solving activity." She showed the combined results from different subjects who were seated in a chair while they were solving as rapidly as possible certain standardized tests involving arithmetical, algebraic, and logical problems. The movements which occurred while the correct answer was given and also those which occurred during the prescribed period when the correct answer was not forthcoming were traced. "Similarity in form of original and interpolated learning and retroactive inhibition" was the title of the next paper by Z. Peel (Arkansas). Paired associates and serial learning were used as techniques. Both experiments showed that retroactive inhibition was definitely greater when the original and the interpolated learning were of the same general form. H. N. Peters (Missouri) discussed seven series of experiments on mediate association. The conditions were controlled by instructional inhibition of the common item, by its prior meaningful connection, and by the Ebbinghaus technique of testing for remote association. In no case was positive evidence for mediate association shown. This was followed by a study on "The retention of interrupted maze performance" presented by F. McKinney (Missouri). With 50 *Ss* learning the mazes while interrupted during their performance and 50 learning them without interruption, his results show that there was no significant increase in retention due to the interrupted procedure. R. N. Menzies (Wisconsin) investigated the memory for pleasant, unpleasant, and indifferent events in recent past experiences. Changes in affective tone were correlated with the amount of forgetting. The last paper in this session established age and sex norms as bases for determining the relative performances of siblings in tests of mechanical aptitude. Extreme differences among individuals were discovered.

Running concurrently with Section A, Section B under the chairmanship of F. C. Dockeray (Ohio State) opened with a paper on pigmentation and visual sensitivity by J. P. Guilford (Nebraska). After determining the mimimal thresholds for light in 82 white subjects and 25 negroes he discovered that consistent differences were found both in white and negro subjects. The greater the pigmentation of the iris the greater, as a rule, was the sensitivity. L. Kardos (Mooseheart) followed with a study of the development of visual perception. The problem centered around the "whole" character of 23 outline figures as over against the piecemeal character in the way that they were described or drawn. Eighty-six children of both sexes between the ages of 6 and 15 served as subjects. D. M. Purdy (Kansas) then presented a paper on "A case of chromatic agnosia and its modification with time." The subject when first examined could not match colors but could distinguish chromatic stimuli from achromatic stimuli. He lacked any concept of hue. Two years later he was able to match colors with considerable precision even though he still lacked the concept of hue. R. N. Gault (Northwestern) asked to have his paper on "Eliminating hearing in experiments on the tactual interpretation of speech and music" read by one of his students, L. D. Goodfellow. The effectiveness of sound-eliminating equipment was measured and was found to produce an effect equivalent to a hearing loss of 100 db. This was checked with 16 *Os* in 3200 trials. Later 14 naïve *Os* were entirely unable to identify five vowel qualities with this equipment. The extra-auditory sensations in the ear arising from stimulation by excessively great sound-pressures were discussed by S. N. Reger (Iowa). Tickling, pressure, and pain arising from sound pressures of about 1000 dynes per sq. cm. were experienced by 27 *Os*, who were completely deaf, at the same values that 46 normal *Os* did; but the threshold of these sensations was raised 25 db. or more in 33 ears lacking tympanic membranes. H. Woodrow (Illinois) presented data in connection with errors in weight discrimination. He made a comparison between two procedures, both variations of the method of right and wrong cases, but in the first fixed standards were used, while in the second the standards varied irregularly from trial to trial. The resulting time-order errors differed radically with the two procedures.

On Friday afternoon Section C, R. M. Elliott (Minnesota) presiding, opened with a paper by L. K. Henry (Iowa) on the rôle of insight in plane geometry. With a series of stimulus-cards, which gradually developed theorems and original propositions, no insightful behavior in geometrical problems was found in the 32 *Os* used. H. H. Remmers (Purdue) then discussed the validity, reliability, and halo-effect of human judgments in defined situations. A high degree of reliability obtained through 20 samplings of r for a population of 60 high school teachers and 60 college teachers and a high measure of validity, measured by the absence of the halo-effect, resulted. R. W. Husband (Wisconsin) obtained negative results in his study of the value of the photograph on the application blank when judged by large groups of college men and women and of business men. One hundred individuals were rated on 18 traits. The only value of the photograph which remains is its use as a check on race, facial disfiguration, and other matters of social importance. A technique for the measurement of popularity in pre-school children and some related factors was discussed by H. L. Koch (Chicago). By the method of paired comparisons 17 4-yr.-old nursery-children were tested for the rôle of the position-error, sex-preference, ennui, intelligence, and popularity. Various forms of tests were used in the analysis of these traits. H. B. Carlson (Chicago) used Thurstone's attitude-scales in correlating intelligence and student attitudes in 215 seniors. Women were found to be more favorable to prohibition and to have a stronger belief in the reality of God than men. Jewish students were the most liberal, Protestant students intermediate, and Catholic students least liberal. Intelligence was found to be positively correlated with communism, birth control, pacifism, and atheism. The last paper in this session on the effect of practice on growth-curves was read by H. B. Reed (Kansas State) in the absence of the writer, E. B. Greene (Michigan). It was given only in abstract form. A number of test procedures were applied to 300 individuals ranging from 5 to 25 years of age. Some of the conclusions showed less improvement on the simple than on the complex tasks, higher correlations between subsequent trials with practice, and between adjacent trials on the easier tasks than on the harder ones.

Section D ran concurrently with Section C under the chairmanship of H. H. Remmers (Purdue). A. G. Bills (Chicago) read the first paper on "Stuttering and mental fatigue." On comparing the continuous perceptual-motor reactions of 25 stutterers with those of 25 normal subjects in the naming of colors, he found that stutterers tended to block more than twice as often and for longer periods than did normal individuals. Their reaction-times were irregular, they fatigued quickly, and their normal records closely resembled those of normal individuals when the latter were extremely fatigued. L. E. Travis (Iowa) followed with a study on the dissociation of homologous muscle function in stuttering. Action-currents were recorded simultaneously from the two masseter muscles during stuttering and during the free speech of both the stutterer and the normal speaker. The results indicated that the two sides of the speech-mechanism probably receive similar innervation during normal speech while they receive dissimilar innervation during stuttering. S. R. Hathaway (Minnesota) took parallel records of action-potentials from four muscles during simple voluntary movements and found that antagonistic muscles of a single pair contracted simultaneously. Single motor units often discharged periodically while the subject was relatively relaxed. The next paper was by L. E. Misbach (Northwestern) on "Modification of the knee-jerk by a concomitant voluntary reaction." His subjects were required to respond either with a right- or a left-hand finger movement in accordance with the brightness of a stimulating light. At the same time knee-jerks were produced during all stages of the voluntary reaction. Photographic records revealed the fact that the magnitude and form of the knee-jerk varied according to the stage of development reached by the voluntary reaction at the time that the knee-jerk occurred. A. Silverman and L. Baker (Wisconsin) next presented a paper which attempted to condition various responses to a subliminal electrical shock. A relationship was discovered between this stimulus and the increase in the amount of breathing, in the frequency of the eyelid response, in the muscular tension of the right leg, and in the amount of gross bodily movement, all of which responses were produced voluntarily by O. A. Jenness (Nebraska) closed this section with a discussion of the facilitation of sleeping hypnosis by previous motor response in the waking state. His results seemed to indicate that with easily hypnotized subjects a response to the suggestion of sleep facilitated the response to the suggestion of postural changes.

The first session on Saturday morning, Section E, under the chairmanship of R. H. Wheeler (Kansas), opened with a paper by G. Finch, E. A. Culler, and E. S. Girden (Illinois) on the "Relation of the Wever-Bray effect to auditory acuity in dogs." When the round window of the cochlea is closed with a soft plug, sensitivity to auditory stimuli is definitely decreased about 10 db., and the electrical pick-up from the homolateral nerve is reduced simultaneously by the same amount. W. C. Halstead and F. Fearing (Northwestern) next presented a paper on the incidence of central degeneration following labyrinthectomy with reference to the variability of behavior-symptoms. The non-acoustic labyrinth was excised and a post mortem histological examination was performed. There was no evidence of central degeneration. The marked variability and temporal pattern in post-operative behavior can, therefore, not be explained by progressive degeneration of the central nervous system. By means of charts and slides H. F. Harlow (Wisconsin) described the capacity of monkeys to solve crossed-string problems. The monkeys showed levels of response equal to the human infant two or three years of age. Following the motion-picture film shown last year W. N. and L. A. Kellogg (Indiana) presented another film depicting the behavior of an ape and a child brought up under approximately the same conditions and of about the same age. The development of walking, affectionate behavior, socialized play, reactions to water and behavior in a baby-buggy were all shown and described in a very interesting fashion. D. R. Disher (Ohio State) then read the results of an experimental study of the reactions of new-born infants to olfactory stimuli. Thus far no racial differences have been discovered in sensitivity to a number of different smell stimuli. But there are large individual differences from one infant to another and from day to day in the same infant. The next paper on the differential responses of new-born infants to different degrees of intensity of light and sound was given by L. A. Weiss (Iowa). She studied 90 new-born infants under different intensities of light stimuli and 42 under different intensities of sound. Positive results were obtained for the most part regarding the effect of these stimuli on the amount of activity and on the movements throughout the stimulus period. The last paper in this section was contributed by W. L. Valentine and I. F. Wagner (Ohio State) concerning the relative motility of the arms of the new-born infant. Studying 55 infants over a period of one to ten days from birth they found no differences as related to color, sex, body-weight, head position, strength of grasp, or ancestral handedness. There is a slight preference for the right arm and the vertical component of the movement predominates in the right arm while the lateral component is most prominent in the left.

H. Woodrow (Illinois) presided over Section F which ran simultaneously. M. A. Tinker (Minnesota) read the first paper in this session on the "Use and limitations of eye-movement measures of reading." While he agreed that the photographic technique is the most satisfactory method of measuring eye-movements and that these measures are valuable, he is convinced that perception-time and the fixation-frequency are important in most quantitative comparisons and that ineffective habits in making eye-movements are symptomatic rather than causal in reading disability. The same subject was continued by R. Y. Walker (Iowa). He discussed the norms of seven measures of eye-movements with 50 students who ranked in the top decile of reading and other tests. Varying difficulties of reading material and requirements of comprehension gave significant differences in eye-movements for duration of fixation, size of fixation, and rate of reading. Mode of duration and extent of forward shift remained fairly constant. The next paper was read by G. A. Kelly (Kansas State) in connection with the relation of cerebral dominance to the perception of symbols. He came to the conclusion that this neural disintegration is a functional disorder of the nature of transient aphasia. This is further established by a study of difference in the orientations of the mnemonic images governed by the two cerebral hemispheres. D. A. Worcester and B. Silkett (Nebraska) followed with a study of the distance from eye to work among school children engaged at various tasks. After examining a great many children in kindergarten through the sixth grade children while normally engaged in various tasks, the investigators conclude that it appears to be futile for teachers to attempt to establish a standard distance at which the children's eyes should be from their work. By well-standardized checking techniques K. Hevner (Minnesota) was able to establish differential mood effects for the major and minor modes respectively in music. With 205 listeners in connection with 10 compositions representing a variety of musical forms, each one of which was transformed from its own key to the other key, the descriptive terms which the *O*s used confirmed the usually assumed characteristics of the two modes. D. Lewis (Iowa) read the last paper describing the tone integrator, an apparatus for the study of timbre. This ingenious device, working through a photo-electric cell, makes it possible to synthesize a number of independent wave-forms in such a way that differences in timbre may easily be detected through a loud-speaker.

Under the chairmanship of C. A. Ruckmick (Iowa), who replaced S. Renshaw (Ohio State) in the latter's absence, a synoptic review of the laboratory work done at various institutions was obtained. Reports were made by the following persons: J. P. Guilford (Nebraska), C. E. Seashore (Iowa), A. G. Bills (Chicago), J. P. Porter (Ohio), R. Stagner (Wisconsin), H. H. Remmers (Purdue), H. B. Reed (Kansas State), D. P. Boder (Lewis Institute), H. Woodrow (Illinois), J. C. Peterson (Kansas State). The order of reports was determined by chance. Some interesting pieces of apparatus were explained, ingeniously improved techniques were described and a significant development of test-material was noted.

University of Iowa CHRISTIAN A. RUCKMICK

THE CURVE OF FORGETTING

Dr. S. K. Chou[1] states that Ladd and Woodworth were the first to express graphically the results obtained by Ebbinghaus in his fundamental experiments on retention and forgetting as a function of time. This is an error. It is true that the 'curve of forgetting' is not found in the work entitled *Über das Gedächtnis,* but it appears in the same author's *Grundzüge der Psychologie* on p. 646 of the first edition, 1902, where it serves to express the falling off of associations, in other words, forgetting. The term adopted by Ladd and Woodworth, by Thorndike, and by others after them, is consequently entirely justified.

J. LARGUIER DES BANCELS

University of Lausanne, Switzerland

A CORRECTION

A regrettable and curious error occurred in a book review in the July issue of this JOURNAL (p. 574). The manuscript of the review of Burnham's *The Wholesome Personality* is correct. Somehow a substitution of 'Wholesale' for 'Wholesome,' made in the galley proof, escaped detection by the editors. The compositor who set the type must have been so much set for 'sale' as to take this lead-off from 'whole' despite the objective stimulus before him; or shall we leave the interpretation to the psychoanalyst? The author can, we hope, find consolation in the fact that the person who made the substitution had not read the book.

JOSEPH PETERSON

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[1] S. K. Chou, What is the curve of forgetting? this JOURNAL, 45, 1933, 349.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The House that Freud Built. By JOSEPH JASTROW. New York, Greenberg, 1932. Pp. x, 293.

Under a sustained figure of architectural construction, Dr. Jastrow describes the design and elaboration of the Freudian doctrines and practices; plans and specifications, building materials, superstructure, and uses. This fills the first part of the book. The second part is critical. It sets psychoanalysis against science; it analyzes the Freudian argument; it dissects the technique of practice; it sums up in judicial temper the entire doctrine; and it anticipates future spread and influence. Notwithstanding the prodigious amount of past and current exposition, of comment, of devastating criticism, and of defense of Freud and Freudism, the present volume has succeeded in making a notable and useful addition in all these directions. If Freud had anywhere written as straightforward and coherent an account of his beliefs regarding the soul of man as Jastrow has in his expository sections, a great amount of time, much printer's ink, and many weak souls might have been saved.

Men have usually written of Freud either as disciple and panegyrist or else as annihilating critic. Jastrow, whose view of the distempered mind has itself been strongly influenced by Freud, displays "a partial and reserved approval of principle, a profound distrust of execution." Jastrow does not "commend the greatness of a great delusion, nor yet condemn the delusion utterly, when he considers the merit of the venture that runs through its intention and formulation" (p. 142). The reader who rests with the author's statement that psychoanalysis "is an amazing conglomerate of unsupported conclusions and unnatural assumptions carried out with a speculative abandon close to irresponsibility—all of which makes it an outlaw in science" (p. 175) may leave the book with a quite wrong view of Jastrow. In such deliverances he is denouncing the Freudian unconscious and the Freudian emphasis upon sex; and in other contexts he is just as relentless in his pursuit of the logical absurdities involved in dream-symbolism and its interpretation, in transference, and in some other major psychoanalytical vagaries. But Jastrow also writes that "both for its promises and its enlightenments the motif of psychoanalysis will endure, and the work inaugurated by Freud marks a turning point in the history of psychology" (p. 260). There is here no real inconsistency. The informed psychologist will remember that one of Jastrow's fundamental concepts is the *Subconscious,* about which this keen critic of Freud and of fraud long ago wrote a book. The critic is therefore friendly to a 'depth psychology' of a sort and is himself accustomed (although he denounces the *Unconscious*) to appeal to the twilight regions of the mind for 'human motivation' and for a fabulous explanation of many facts in human experience. While many psychologists *von Fach* may find Jastrow's subconscious as dangerous to description and to sound understanding as the macrogonadic Unconscious of Freud and other lusty mystics, it is doubtless important for the tolerant and apappreciative tone of the book under review that this sympathetic bond exists. It helps to make Jastrow fair to, and at times sympathetic with, a man and a movement whose logic he finds intolerable. The cultish adherent of psychoanalysis, whose demand for factual evidence, for historical sense, and for authentic methods of observation, is no greater than that of the Viennese Master himself, is not likely to be greatly moved or enlightened by a leisurely tour through *The House that Freud Built.* Whether in religion, in morals, in fine art, or in medicine, the devotee always observes best and most happily with closed eyes, bowed head, and open mouth. But many younger men and women, who know something about the method of the sciences and something about the history of superstition and magic, will be usefully informed and critically balanced by Jastrow's candid and competent book.

Cornell University M. B.

The Jungle of the Mind. By EDGAR JAMES SWIFT. New York, Charles Scribner's Sons, 1931. Pp. ix, 340.

This, Swift's last book, revolves about a familiar thesis: thinking is not a natural and spontaneous process; the mind is prone to linger on the primitive level, setting mysticism and emotion before logic. The revolutionary discoveries of science in the last decades and the forced revision of scientific laws instead of emancipating have but confirmed mankind in their uncritical ways, and credulity now poses as open-mindedness. We still live largely on the perceptual level, accepting sense-experience —often deceptive and distorted—at its face value, or interpreting it in the light of prejudice and wishful expectation. Tagging phenomena with emotionally charged names continues to take precedence of the slower processes of analysis and experiment.

The sign-posts raised to point the way out of the jungle are, to be sure, neither new nor startling. Resistance to the thrill of mystic phrases, recognition of the subtle pull of personal bias, suspension of judgment till the supplementary facts are in, application of the touchstones of conformity with organized knowledge, of a reasoned economy in explanation—these all have a familiar ring to the psychologist and educator. Such is the author's wealth, however, of carefully collated and dissected cases, his picturesque diction, and sly humor, that shopworn topics take on a new slant and freshness. With its seasoned wisdom and sound scientific outlook, the volume gives promise of as great usefulness on the supplementary reading list as the earlier "Psychology and the Day's Work," or "Youth and the Race."

For many years, it appears, Swift had collected and followed up instances of superstition and emotionalized opinion in civilized man: belief in prophetic dreams, in materializations of the dead, in almanac prophecies of weather, in faith-healing, in panaceas for defective memory, in telepathy and the unconscious mind. Reference of such phenomena to occult or supernatural sources he combats, sometimes by citing experiments in which the effect is paralleled by purely natural means; sometimes by an appeal to common sense—the frequency of coincidence and the neglect of negative cases; sometimes by a gentle irony quite his own. Memoirs and confessions of hoaxes by magicians and men of science serve also to expose the absurdity of the lapse into "jungle ways."

To illustrate, the deceptive character of sense-experience is illumined by Patrick's experimentally demonstrated exclusion of flavors from the class of tastes. Instances of phenomenal memory in day-laborers, savants and hypnotized subjects are explained in terms of laboratory science, laws derived from which are offered to replace and better the panaceas put out by quacks. The fallacies of 'telepathy' are dispelled by citing Beard's and Cumberland's early analyses, Downey's and Coover's experiments, and Pfungst's careful work on Clever Hans. The use of cues from eye and ear as well as muscle by palmist and clairvoyant is exposed. Moving out over a wider field, the scientific and adaptive treatment of the psychiatrist is set over the rule-of-thumb methods of the faith-healer. The phenomenon of 'suggestion,' and the psychically induced paralyses and anaesthesias of nervous disorders such as 'hysteria' are briefly expounded, as the simpler explanation of apparent miracle.

In Chapters VIII-XII, under such captions as "Have Human Beings Minds?" and "The Power Behind the Throne," the abuses of science as well as superstition come in for comment. The myopic logic of the behaviorist is handled with sly humor. Even the "twelve apostles" of the cult might read this section with profit if not with pleasure. Under another heading, "Ironclad Manacles," the limits of 'conditioning' as a device for race betterment are effectively exposed—if with something less than the brilliance of an Aldous Huxley.

As for Swift's appreciative critique of psychoanalysis, all but the more fanatic followers of Freud will concede its justice. The functional dynamic approach to neuroses is commended; while such biased postulates as exaggerated emphasis on sex, on repression into the unconscious, on a symbolism as "standardized as the parts of a Ford car" are rejected on the basis of later studies of war neuroses, 'shell-shock,' and the dreams of the normal. (Incidentally, in the opinion of the reviewer, Swift has misconceived the dream theory of Havelock Ellis.) There is shown to be no chasm between waking and dreaming consciousness, between ordinary and creative thinking. Throughout the book, indeed, the resolution of the mystic or aberrant into terms of the natural or psychological is a cardinal point. The emotional exaltation induced by foggy phrases and the thrill of posing as a member of a unique and 'progressive' sect are listed, on the other hand, as potent lures in winning followers for the cults and fads, to the detriment of clear thinking.

In spite of the attractive makeup of the volume, its popularized captions and wealth of anecdote and illustration, it strikes the reviewer as a peg above the level of the 'average reader,' at whom it apparently was aimed. Those of us who are not faddists in psychology, however, can commend it whole-heartedly to our students, not yet weaned from jungle ways by radio lessons and the introductory course; with the parting admonition that psychology "is not a patented remedy for making us healthy, wealthy and wise, as psychic fakers would have us believe;" yet it may "show the weak spots in the armor of those who care to fight their way out of the jungle of our primitive ancestors—and who seek for intellectual alertness, poise, and a robust common sense in a muddled world of muddled thinking."

Cornell University ELSIE MURRAY

Habits, Their Making and Unmaking. By KNIGHT DUNLAP. New York, Liveright Inc., 1932. Pp. x, 326.

Beginning with a preface in which the author claims to have flouted (sic!) the accepted principles of learning, this recent attempt to clarify the problems germane to habit formation and destruction, stresses two hitherto unemphasized learning fundamentals. These are, first, that we frequently learn to perform a particular activity by practicing something quite different, and secondly, that under many circumstances the best way to break a bad habit is to practice it consciously—"negative practice." A lack of experimental evidence supporting these and other alleged principles is none too satisfactorily explained in terms of a wish stated in the preface "to spare the reader the technicalities which might not only weary him, but might distract him from the main points" (p. ix).

The first two chapters are devoted largely to a definition and formulation of the problems of habit and learning, and to a discussion of first principles. Considerable importance is attached to the allegation that thought processes are of fundamental importance in learning. While neglecting to state exactly what he means by "thinking," Dunlap seems almost convinced that without it learning is an impossibility. "It is not evident, though it *may possibly be true,* that we can learn without thinking" (p. 29, italics mine). There are also the usual criticisms of certain Thorndikian contentions, chiefly the "law of exercise." It is made logically evident that we do not learn to do by doing. "The responses which are employed in learning are not the responses to be learned" (p. 33).

Dunlap then develops in an interesting fashion his own criticism as well as a summary of the accepted criticisms which have been leveled at the "brain path" and "conditioned reflex" theories of learning. His arguments are well put, but their cogency is not enhanced by insinuations as to the lack of help given psychologists by researchers whose work has generally been considered important. This is particularly true of Pavlov. Concerning the psychological theories of learning Dunlap concludes that, "for the present, the only available theory of brain action in learning is that the brain does act in learning; but that the nature of its action is unknown" (p. 58). While the reviewer was a bit confused at calling the fact of the brain's activity an "available theory of learning," the conservatism of the statement was appreciated.

In the chapter on "The Process in Learning" a considerable amount of symbolism is employed to illustrate the nature of our learning reactions. The two principles referred to in the first paragraph of this review are elaborated in detail. Dunlap frequently dignifies his subject matter in no uncertain terms by descriptions of the complexity of learning and statements of the strenuous requirements which should be met by practicing psychologists.

In his discussion of "The Conditions of Efficient Learning" attention is called to an error which text book writers in the field of educational psychology have consistently committed; namely, unjustified generalizing from insufficient or partially digested facts. Particularly is this true in connection with whole-part learning, the distribution of practice periods, transfer of training, punishment or reward, etc. Educational psychologists have tried valiantly to make a little bit go a long way. There is a great volume of research materials, but when these are studied conscientiously there seems to have been a disheartening elaboration of the obvious or inconsequential. Dunlap believes that a number of the really fundamental problems are practically unrecognized, much less studied.

Certain of the later chapters strike the reviewer as being interesting but rather irrelevant. The treatment of personal and social adjustment involves too much discussion of the nature of behavior aberrations with a consequent neglect of their genesis or treatment. This is also true of the chapter on "Habits of Emotional Response." The concluding chapter, devoted to a discussion of "Learning Ability and

Intelligence," is largely a critique of current testing practices coupled with statements as to the nature of intelligence.

Chapter X, "The Breaking of Specific Bad Habits," is in many respects the most interesting, particularly for the lay reader. Great emphasis is placed upon the importance of initial negative practice for the removal of speech defects, tics, and irregular sexual habits. For one so obviously wedded to the scientific method, Dunlap makes some extreme statements concerning homosexuality, viz., "Every homosexual is a point of social contagion, a real vampire, whose 'bite' makes a vampire of his or her victim." "The practice of males impersonating women in college dramatics, for example, where it does not make the play ridiculous, has a direct connection with homosexual problems." "It should be noted that every man who shows a tendency to caress or needlessly handle boys, every woman who 'courts' young girls, should be watched" (pp. 222 f.).

It is to be regretted that the publishers, and at times the author, of this book imply that it was written with the non-professional reader's interests in mind. Any understanding of most of the theoretical materials involves a rather thorough familiarity with psychological principles. This is particularly true of Chapter V, the "Process in Learning." This seeming inability to decide upon the particular type of reader addressed, frequently gives the impression of an attempt to carry water upon both shoulders. It is true that the author does say in the preface that some readers "may find the reading easier if they begin with the seventh chapter and read the earlier chapters after reading the later ones."

Brief notes and a classified bibliography, arranged for each chapter, cover 53 pages at the end of the book. In certain fields it is far from complete.

University of Nebraska STEPHEN M. COREY

Ethical Relativity. By EDWARD WESTERMARCK. New York, Harcourt, Brace & Co., 1932. Pp. xviii, 301.

This book, like most works on ethics, is more concerned with word distinctions and logical relationships than with facts and inductive generalizations; it is more philosophical than psychological. Yet it makes some references to facts upon which the author is so well informed, and gives good accounts of moral emotions and their origin and relationships. Westermarck holds that moral judgments must be based upon facts of consciousness and are determined chiefly by emotional reactions and are, therefore, relative and cannot be validated objectively. He admits that we have a tendency to objectify our feelings and therefore to think of things that please us as good, and those that displease as bad; and that emotions are influenced by special circumstances and results, and by customs, concepts and beliefs; yet holds that the chief determinants of moral judgments are emotions. There are two moral emotions—approval and disapproval or indignation, both belonging to retributive emotions. Disapproval has had the larger part in the development of moral judgments. Offenders are punished not only on account of resentment but to make them change or reform, and to deter others from commiting the disapproved act.

It will be profitable to contrast Westermarck's philosophical treatment with the more scientific approach to the problem. If ethics is to be developed as an inductive science worthy of ranking with other inductive sciences, it is necessary to refrain from seeking absolute truth, and from dependence upon logic as an instrument of research.

From the standpoint of psychology a science of ethics might be developed by making a purely objective study of the behavior of men in relation to each other with a view to determining the generality of various acts classed as moral. The result would be a pure science of ethics somewhat resembling the science of plant life and growth. In applying such a science means would be sought for promoting the most prevalent of the fundamental forms of behavior, or those best adapted to bring about certain desired results such as increase of number and intensity of life activities. This would result in an applied science of ethics similar to that of hygiene, but broader.

A second method is open to those who do not take the extreme behaviorists view; that of a study of conscious ideas, emotions and volitions involved in acts generally regarded as moral, and checking the facts and introspection as completely as possible by means of the objective concomitants. Few men have a better preparation for making such a research than Westermarck. If he would abandon his philosophical and logical premise that a proposition is either true or false, in favor of the modern scientific efforts to determine by exact observation, measurement and calculation, what degree of truth is expressed by any given generalization, he could render great aid in developing a science of ethics. Such a product would be in accordance with our author's view of determinism as against chance or fate, but he would probably then be less sure of his claim that moral judgments must apply to persons and that intent must be considered by all enlightened people rather than the act alone.

It is well to distinguish between the moral acts of a person and acts judged as good or bad regardless of who commits them. To decrease disease, accidents, stealing, killing, etc., is the aim of intelligent men of today. It may well be that the enlightened man of tomorrow will expend most of his disapproval on acts themselves, and be indignant, not toward the individual committing them so much as toward a society which permits or produces the conditions under which they are likely to occur.

The reviewer regrets that our author has not increased his contribution to human knowledge by maintaining throughout his researches and writings the viewpoint of the inductive sciences, making as reliable generalizations as possible, instead of merely resting content with showing that subjective moral judgments cannot have universal objective validity.

Columbia University E. A. KIRKPATRICK

Industrial Psychology. By MORRIS S. VITELES. New York, W. W. Norton & Co., 1932. Pp. xviii, 652.

The book falls into three sections, the first on the foundations of industrial psychology, the second on fitting the worker to the job, and the third on maintaining fitness at work.

The section on foundations of industrial psychology goes into its background with a historical slant from the economic, the social, and the psychological standpoints. Individual differences are stressed in this section and illustrated with reference to industrial production.

The second section on fitting the worker to the job states the problem as involving the proficiency, competency, temperament, character, and interest of the applicant or employee. It is pointed out that the usefulness of intelligence tests is mainly at the lower borderline in clerical work, and at the professional level, and that therefore tests for more specialized traits constitute the larger problem. The various methods of job analysis are reviewed with the emphasis primarily upon the psychological items in such analyses. The limitations of the interview and experimental demonstrations of its frequent shortcomings are then presented and are followed by some suggestions for improving the interview. Then follow application blanks, application letters, and the use of photographs, leading up to the unsatisfactory procedures of character analysis.

The third section, on maintaining fitness at work, begins with a consideration of accidents. After stating the seriousness of the problem, many studies are cited to make the point that individuals different in accident-proneness. The author feels that merely dismissing accidents as due to carelessness is scientifically unsound and that it is necessary to study the individual worker. We have efforts at testing susceptibility to accidents and also data on the effect of age, physical condition, nationality, fatigue, rate of production, atmospheric conditions, and illumination. A wide range of studies is summarized in this connection. Accidents in the transportation industry are considered separately with considerable emphasis on clinical study of the individual driver and investigation of the nature of accidents.

Pertaining to the matter of skill and training, learning curves are discussed in somewhat conventional fashion. The writer lays a wholesome stress upon reward rather than punishment for industrial learning in that the former not merely strengthens the tendency toward correct performance but tends to build up favorable feelings as well. The literature is reviewed on the question of whole versus part methods with special reference to motor skill. The author finally concludes that it is desirable very early in the operation to give the worker some idea of the process as a whole, although he may nevertheless practice considerably on the different parts. Similarly, on the speed versus accuracy question he concludes, after reviewing the literature, that the initial stress should be upon doing the performance correctly but as soon as possible some emphasis should be placed upon speed.

Two chapters are devoted to fatigue. Then follows a chapter on motives. Financial incentives are shown to be inadequate and the reader is left with the impression that motivation is an extremely complicated thing in industry due to a multiplicity of causes. Supervision looms rather large in the discussion. Then we have the maladjusted worker with some clinical material on typical cases. Finally, we have a brief treatment of supervision with emphasis on leadership.

The book has several outstanding features. In the first place, it is comprehensive and it at least touches upon practically every large problem in industrial psychology. In the second place, it has much more contact with the European literature in the field than any book thus far published in this country. The reader will probably have more respect for the German industrial psychologists than he had prior to reading the book. In the third place, the treatment is, on the whole, thorough. However, in many cases it is not clear whether the author is reviewing critically all the literature on a particular topic or merely striving to illustrate his point. He will frequently give six examples where two would suffice to make the point which he is stressing. This tendency possibly makes a bit for heaviness in the work.

The author rather deliberately avoids anything smacking of statistics and merely refers the reader to other works on the subject. For the technical reader this would suffice because he would immediately look up the methods elsewhere. But for the person without such background it might have been well to lay a little more stress upon statistical methods merely to show him what the industrial psychologist is up against and to disillusion the reader as to any short-cuts to success in this field. However, the stress on the painstaking clinical methods which the author makes would tend to break up any such illusions. The reviewer would be inclined to include salesmen in the other groups when training was being discussed. Inasmuch as selling is such a tremendously important part of the whole program of psychology, the training of salesmen might be legitimately included in the present discussion—not the psychology of selling, but the mere training of the sales force. Then the question of morale, while mentioned specifically in a few places and discussed by implication in a good many others, might even to advantage have been lifted out and made a separate topic.

Ohio State University HAROLD E. BURTT

The Scientific Basis of Social Work: A Study in Family Case Work. By M. J. KARPF. New York, Columbia University Press, 1931. Pp. 424.

It is inevitable that a contribution which attempts to sketch the scientific basis of social work should reveal what psychology has made and should make available to aid in the social work processes. A glance through the index, clearly reveals the dependence of adequate social service on sound psychological procedures. As one turns page after page of this book one finds recognition of the contributions psychology has made to influence progress and scientific methodology in this field. Just as obviously does one find a running statement of our limitations, a catalogue of the missing or inadequately developed sections of our psychological systems and rather constant stimulation and prodding to undertake or to continue research in directions likely to yield usable data in the adjustment of human beings to their environment. The author is no less constructively critical regarding the use which the social worker now makes of the accumulated materials of psychology as well as of other social sciences.

The work of such a summary must have been extremely difficult, not alone because of the fragmentary nature of many of the contributions but because of the huge territories which had to be canvassed to locate material which had utilitarian value. Under the circumstances, the task is very well done. The field of biology, however, appears least adequately treated. It is intimated that "the biological point of view is likely to have a somewhat limiting if not discouraging and depressing effect on the social worker," and the major portion of the discussion of the contributions of biology is devoted to the pros and cons of eugenic and euthenic programs.

The contributions of psychology might have included greater amplification of the recent gains from studies of conditioned behavior, the psychology of suggestion, the psychology of testimony and report, and the significance of imitation, sympathy and a variety of other phenomena from the feeling and emotional fields. Karpf finds that very little has been made available by psychology for the immediate applications of the social worker to her tasks. If this be true, it merely emphasizes the importance of the task and the grievousness of her mission.

The second part of this book is devoted to a review of what knowledge social workers actually use, based on an analysis of family case work records. This is the first substantial effort to determine objectively what applications are really made in this field of the accumulated facts of the social sciences.

The third part of the book is concerned with an analysis of what knowledge social workers receive. It is instructive to note the extent to which schools of social work attempt to transmit, to the social worker in training, the practical applications of psychology. On the one hand, the meagerness of what psychology has to offer is contrasted by the large amount required to orient and to empower this type of practitioner adequately. This exposition should serve as a stimulus to organize more satisfactorily what we have accumulated and to apply ourselves to a number of pressing problems, the solution of which will make for more adequate service to the social work field. The need for this is clearly indicated in the final section of the book.

The Scientific Basis of Social Work is a challenge to the field of applied psychology. More thinking, more exploring, more adventuring on the part of the psychotechnologist is needed. If anyone has developed symptoms of self-complacency or self-admiration regarding the contributions which psychology has thus far made for a better regimentation of our lives, and wishes to be helpfully irritated and disturbed, he should read this book.

Graduate School of Jewish Social Work S. C. KOHS

Criminology. By ROBERT H. GAULT. New York, D. C. Heath & Co., 1932. Pp. ix, 461.

At last we have a criminology written by a psychologist! Though the author shows himself thoroughly familiar with the materials of the sociologist, the psychiatrist, the jurist, and the welfare worker and proclaims crime an inherently social phenomenon, he still directs his analyses to the offending individual called the criminal or potential criminal. The kind of mental organization and the potentialities which make it possible for human beings to get out of harmony with the social order are the major elaborations of the text, such questions as the social machinery for dealing with criminals and the techniques of detecting crime receiving only a secondary emphasis. Age, race, sex, physique, heredity, attitude, and mental abnormalities are classes of variables whose relationship to crime are very carefully evaluated, the usual errors in the interpretation of these relationships as a result of failure to detect faulty sampling procedures, etc., being admirably dragged to the light and avoided. In fact, the author in his caution seems at times almost to devitalize certain factors and set up a straw man to knock down. The general statement on page 227, which follows a lengthy discussion of the factor of age will illustrate such caution. However one may feel about the extent of certain dissections, it is a great relief, in view of the dogma that is rampant, to have available so exceedingly trustworthy a text.

In the discussions of the fundamental dynamic phases of human behavior one finds no mention in the work of those mechanisms stressed by the Freudians but rather an exposition of the concept of the prepotent reflex and the concept of a generalized drive to activity. The former the author seems to feel the more respectable, the latter serving as a sort of catch-all for the dynamic patterns which defy classification under the category of prepotent reflex. It does seem regrettable to the reviewer that more recognition was not given to such considerations—to use a type —as the universality of the direction (relative to the stimulus) of the varied adjustments to persistent pain. Perhaps Gault's unwillingness to focus upon the dynamics of mental organization with that slant is responsible in part for the rather ineffective support which his chapter on the drives of the organism lends to later developments in the text. In the section on attitudes, however, though one might choose to wrangle over the definition presented, an approach does seem to be made to the parameter we have in mind.

Viewed as a text or reference book for college students, the volume is excellent. It is well captioned, clear cut in its organization, interestingly written, rich in well-chosen case material, concentrated in its content, and provided with an up-to-date bibliography on the topic of each of its chapters. Such questions, however, as the practical details of remedial procedures are touched rather lightly. Avenues of approach are pointed out, but detailed road maps are not given.

A tendency on the part of the author to give some attention to the history of the major issues, the points of view, and the institutions which he discusses has the wholesome effect of forcing the present status to appear in its proper proportions. Though led to feel encouraged over progress thus far made, one does not lose sight of the fact that as yet criminology boasts of no final solutions or perfect tools.

University of Chicago HELEN L. KOCH

Human Heredity. By ERWIN BAUR, EUGEN FISCHER, and FRITZ LENZ. 3rd Ed. Trans. by Eden and Cedar Paul. New York, The Macmillan Co., 1931. Pp. 734.

Although the science of human heredity has made great progress and a vast literature has been published since the first edition of *Human Heredity* in 1923, comparatively little has been added to the general theory of heredity during that period. Those who are familiar with the two earlier editions of this work will not be surprised to find little revision in the theoretical section, Section I, written by Baur. The author believes that even in the field of human heredity a process of stabilization is in progress, which will make unnecessary any radical revisions of future editions of the book. This is perhaps too optimistic an outlook in view of the present unsettled theories regarding the relation of genes to the inheritance of specific character traits and certain unsettled problems of sex determination.

Section II, written by Fischer, deals with racial differences in mankind. In this section additions have been made concerning various forms of bodily structures or types of constitution. The book was not written for anthropological specialists, but rather "to provide the reader with such elementary knowledge as is requisite for the understanding of the racial-hygienic inferences which can be drawn from a knowledge of the laws of inheritance." Of particular interest to the psychologist are conclusion that the cranial index is probably a Mendelian trait; that on the average high intellectual capacity is associated with heavier brains than the average of their national stock; and that there is not information to show that particular arrangements of convolutions are hereditary, a matter seemingly quite as important as brain weight.

Section III, on Morbific Hereditary Factors, by Lenz gives much recent data to prove the hereditary character of all forms of disease. Numerous family histories point definitely to the conclusion that myopia and other errors of refraction run in families as is also the case with albinism, nystagmous strabismus, and cataract. Cerebellar ataxia, tremor, Huntington's disease, deaf-mutism, stuttering and other disorders of speech are described as recessive Mendelian traits. Insanities and psychopathies including imbecility and idiocy, schizophrenia, epilepsy, manic-depressive insanity, hysterical temperament, neurasthenia, psychasthenia, and homosexuality are also shown to run in families according to the law of recessive inheritance.

Chapter 10 is devoted to the origination of hereditary morbid predispositions. Such morbid predispositions imply anatomical disorders which themselves are hereditary. But since the anatomical basis of hereditary factors is of a physico-chemical nature, it must be subject to alteration by physico-chemical influences. The general term for the causative process effecting such alterations in the hereditary equipment is idiokinesis. Numerous experiments are cited to show the idiokinetic damage produced on offspring by alcohol. Not all the results are conclusive, but many experiments on fowls, guinea pigs, mice, and other animals as well as data collected from alcoholic intoxication in the human population give supporting evidence of various states of enfeeblement and various malformations in the offspring. "When such degenerate animals were paired, the morbid taint appeared in the second, and even in the third and fourth generation." Other idiokinetic causes are also considered, as lead, mercury, phosphorus, nicotin, carbon disulphide, benzol, anilin, and kindred substances. A number of substances used in medicine must also be included, as quinine, iodine, mercury, and arsenic. Idiokinetic effects have been demonstrated in the case of Roentgen rays, and syphilis. No support is given to the widely prevalent belief that malnutrition is a cause of morbific hereditary. Its influence is probably only parakinetic rather than idiokinetic.

Section IV, also by Lenz, is concerned with the methods for the study of Human Heredity; and Section V deals with the inheritance of intellectual gifts.

Family histories are presented to prove that musical talent is inherited. Statistical evidence is adduced from the writings of Galton, Wood, and Terman to show that genius is undoubtedly hereditary.

The last chapter is on psychological differences between the races of men. Here it is asserted that the differences between the various racial factors are in general less extensive than those between normal and morbid factors. The racial characteristics of the so-called Nordic, Mediterranean, and Jewish races are described.

There is a bibliography of 65 titles.

University of Montana FRANKLIN O. SMITH

Life and Beauty: A Spiritual Autobiography. By P. W. ROBERTSON. London, Edward Arnold & Co., 1931. Pp. 174.

"All theories of art are wrong. From the idea of beauty particular to any age each man of genius has differed, precisely in accordance with his genius. Beauty springs from life, is the enduring expression of its most passionate moments." Thus the author, who is a chemist as well as a connoisseur of art, opens his introduction, or 'Justification.' This information, he indicates, is obtained from the poets—and from his own experiences—rather than from the philosophers who "would twist and torture the words into a vain semblance of other reality." Robertson is apparently unacquainted with experimental psychology, and depends on theoretical and on Freudian methods and views for his criteria of the beautiful. Indeed he regards Freud "as one of the greatest of modern thinkers," and attributes to him the shattering of the illusion "that man is a perfectly rational being" (p. 159). Robertson even (when convenient) uses the methods of the Freudians against opponents, as when he says that "the professional moralists are wrathful [at Freud's views] because their own inherent immorality is divulged" (*ibid.*). As used in the book before us, "life implies the unfolding story of human endeavor, the pulse and sway of the seasons, the heart throbbing in hatred or in love, the inestimable spirit of man, his inclination to the stars" (p. 11). Even this is vague enough, but from the autobiographical pictures, rather sparsely presented, one gets the following notion of the author's view: Every man in his life repeats the history of his race, whose fundamental passions—love and combat—are in fact not chaotic but simple and direct. From "the age of three to about nine" the child "recapitulates the mental history of his ancestors" from the primates to the modern civilized man. "After that age he begins to live on individual experience" (p. 17). The art-life, or rather the beauty in one's life, is thus early set in his hereditary background of passionate struggles, and, even to a larger degree, in the incessant conflicts of his early life. Thus are fixed fundamental emotional patterns which are repeated and elaborated in later experiences. The intellectual impulses in man (aroused by external stimuli) to explore, create, and to communicate his knowledge are constantly checked and guided by early scenes of love recurring in different concrete emotional settings in later stages of life, and are given shape and motivation by them. Thus interest in ships was found by the author to be expressions of impulses in the "Unconscious" to get back to a childhood love-scene. Anger seems to play a decidedly minor rôle. The artist's "desire to communicate his experience is probably very deeply rooted in the early social history of man and is derived possibly from the herd instinct" (p. 134).

The spectator of works of art is able to understand and feel the conflicts and satisfactions of the creator of the art in so far as they are able to arouse his own individual patterns formed in childhood with their accompanying emotions. Thus a scene may be beautiful to some and not to others, and beautiful to a man one day and not on another day. It is impossible to determine where aesthetic values end and other values begin. While art, according to the author's views, has no moral end or purpose, there is, however, an incessant conflict between intellectual and emotional adjustments. "The artist remodulates this disharmony in his Unconscious into a new order of life which is harmonious, thereby obtaining a sense of release and achieving a kind of freedom" (p. 133). Admitting that Croce's work has been stimulating to him, Robertson informs us that the Freudian view of the Unconscious led him toward "a more satisfying solution" of his problem and that he can now "look back upon the philosophy of Croce [identifying intuition with expression] with the respect that may still be held for an idol that has fallen by the wayside" (p. 129).

There is no inquiry into the possibility of similarity of inborn structures in individuals (hereditary or not) by means of which early emotional experiences may be largely similar in pattern in different individuals; nor is the question faced as to what extent the general interpretation given can be at all satisfactorily applied to numerous detailed phenomena in the several arts. There is no index. While the book is well worth a careful reading, it is not very suggestive as to experimental procedures on the problem. This for decades has, of course, been a weakness in 'studies' of art.

J. P.

Psychophysiology: III. Cerebration and Action. By LEONARD T. TROLAND. New York, D. Van Nostrand Co., 1932. Pp. xxiv, 446.

This book is the third in Troland's four volume work. It represents the author's attempt to correlate experience with events in the central nervous system and to give an account of the manner in which impulses are organized and released in the efferent paths.

A close reading of the book leaves one with the general impression that the author has done an extremely difficult task exceedingly well. The available fa regarding the nature of the specific operations in the nervous system which a correlated with specific types of experience are limited, and sometimes indefinite i meaning. Accordingly, Troland indicates at the outset that it will be necessary to make liberal use of the *method of hypothesis* in order to predict, at points where facts are lacking or equivocal in meaning, the most probable nature of cerebral function underlying the item of experience in question. Although the over-cautious scholar may criticize the work with a considerable degree of justification, on the ground that the author indulges too freely at many points in speculative reasoning, nevertheless it may be submitted in turn that a too cautious attitude may not contribute so much to the rate of progress with which the unrevealed facts at these difficult and complicated points are to be acquired as Troland has contributed. He surveys and interprets the known facts with the view of predicting the most probable direction which progress will take presently. The work may also be criticized by some on the ground that in certain instances the contributions of men which a decade ago received much attention are here assigned a less conspicuous place. However, the author is obviously carried by the recent swing to a strong dynamic emphasis.

As the sub-title indicates, the work falls into two parts, Cerebration and Action. Although the results of recent investigations have led certain writers to believe that consciousness is determined by general cerebration, or activity in all parts of the cerebral cortex, the corpus striatum and the thalamus and that the cerebral cortex serves as a "general dynamic battery for energizing processes whose specificity depends on lower nerve centers" (p. 68), Troland advances anatomical and pathological evidence in support of the opposite view, specificity of function in the cortex. He states that conscious experience is apparently determined by a process of *focal synergy,* consisting in a "detailed, dynamically compounded activity" (p. 128) in a limited part of the cerebral cortex, chiefly in the frontal area. Nerve currents converge upon the determinative area to form an "electrically unified synergy" (p. 67). Cerebration includes the establishment of *neurograms,* or afferent recording, and the conditions by which they are activated subsequently in reproduction, and in the linkage of these with *incitograms* which underlie the organization of efferent currents. Affection is intimately related to the establishment or destruction of neurograms. Pleasantness is correlated with increase in conductance in the cortex, the degree varying directly with affective intensity; unpleasantness, with decrease in conductance. With regard to the location of the center for feeling, the author does not agree with Head that this is controlled by the thalamus alone. He cites evidence to sustain the view that the thalamus and the cerebral cortex act coöperatively in determining feeling, and finds support in the work of Sherrington for inferring that the degree of cortical coöperation is greater in pleasantness than in unpleasantness. Imagination and thought are said to represent forms of cerebration which result from intra-cortical excitation of neurographic units or systems.

Having located hypothetically the cerebral foundations of conscious experience chiefly in the frontal regions of the cortex, Troland turns to the problem of action. It is clearly shown that all forms of experience are related to motor processes. The rôle which action plays in learning and the bearing of motivation upon action are thoroughly treated.

All forms of action are founded on *incitograms,* or incitation systems, which underlie the organization of efferent impulses. These are structurally separate from impression systems or neurograms, although necessarily linked with them. In voluntary action the incitation system is very complicated. Such systems are not limited to the cortex but include lower motor centers, especially in the cerebellum. The most unique part of the author's treatment of action, perhaps, is his view regarding the basis of an act of will. The process of "will" has proven so elusive as to fail to appear in introspective descriptions of voluntary acts. Still, Troland reckons with this process, giving an explanation of its cerebral foundation. In this undertaking, he relies chiefly on the writings of James, Ach, and Michotte. According to James, the characteristic phenomenon of will consists in "holding the idea of action before the mind" (p. 355). Ach would add that the idea of movement must be "identified with the self" (p. 356). Accordingly Troland relates the self to the kinesthetic system or "inner experience," and suggests that an act of will "consists in the process of bringing the kinesthetic image into proper relationship . . . or in register . . . with the total kinesthetic complex" (p. 357).

This work is a contribution of the greatest importance to the worker in physiological psychology. It is probably too theoretical, too controversial and too conjectural in nature to find application at the lower level of instruction in psychology, but the graduate student and professional man in psychology will find it indispensable. The author is accurate in his statements and clear in his reasoning. The work is notably free of typographical errors, the few exceptions which caught the attention of the reviewer being—"phyllogenetic" for phylogenetic (p. 311, 318), "prioceptive" for proprioceptive (p. 371), "desiderâtium" for desideratum (p. 402) and in the index (p. 445) the first initial for Swift should be E instead of D.

Fortunately for psychology this series of volumes on psychophysiology was completed before Dr. Troland's accidental death. However, in his death American psychology has sustained the loss of one of her most productive and thoughtful scholars. (See this JOURNAL, 44, 1932, 817.)

Wellesley College MICHAEL J. ZIGLER

Growth and Development of the Child, Part IV. Appraisement of the Child: I. Mental Status. II. Physical Status. Report of the Committee on Growth and Development, KENNETH D. BLACKFAN, Chairman. White House Conference on Child Health and Protection. New York, Century Co., 1932. Pp. xix, 344.

The fly-leaf of this book lists thirty-five authors, sixteen of whom contributed to the portion of the volume devoted to Mental Status. The importance of the book is derived both from its content, the distinction of the authors, and the auspices under which the volume was prepared. The views expressed in it are likely to dominate endowed research for the next ten years.

Since the volume was prepared on the committee plan, it is impossible to credit any one author with the writing of any particular section or chapter. The General Introduction to the volume perhaps expresses the viewpoint of the authorship more fully than any other chapter. If we may judge from this Introduction, the research of the next decade will study with equal vigor both the normal and the abnormal; especial attention will be given the pre-natal, the neo-natal, and the adolescent periods; animal experimentation will be continued; the fruits of clinical observation will not remain unnoticed; and exact statistical method will continue to be employed, though with greater recognition of its limitations and dangers. The Committee favors strongly "the type of study which follows an individual over a long period of time, making systematic tests and measurements at regular intervals, and gathering all kinds of relevant information about him." For some reason, the promise of human experimentation—clearly feasible in the study of immunity, nutrition, and parental education—remains unmentioned.

To the present reviewer, the General Introduction seems by far the most significant portion of the volume; the remaining chapters devote themselves largely to a survey of the data of child psychology. The first chapter, on "Mental Growth in Infant and Child" (apparently written by Arnold Gesell), stresses the importance of clinical judgment for the interpretation of mental measurements in infancy; and urges that "programs of infant training cannot be well undertaken apart from the safeguards, the technique, and the traditions of medicine." Clinical psychologists should have something to say about this! The next chapter—an excellent survey of "Intelligence Tests"—appears to deny the importance of clinical judgment, in its emphasis on the value of tests "taken by plan . . . , and interpreted by standards which have been freed from personal bias." This chapter rather scouts the importance of separate race- and sex-norms (a point of view which is duly contradicted in a later chapter (p. 186). The assertion, on p. 36, that it is at present "impossible to measure intelligence in terms of any absolute scale whose units are of equal value" seems to the reviewer to dismiss Thorndike's and Thurstone's extensive work rather briefly and curtly.

The next two chapters are devoted to "Mentally Superior Children," and "Inferior Mental Development," respectively. The first of these two chapters is practically identical, word for word, with Terman's chapter on "The Gifted Child" in *A Handbook of Child Psychology.* The brief chapter on "Inferior Mental Development" raises the question whether children who are ultimately inferior, do not "develop during infancy at a rate which much more closely approximates the normal curve of mental growth than would be expected from the ultimate mental inferiority." Such a phenomenon could be explained (although the author does not do so) on the supposition that early mental tests are less purely intellectual than later.

The chapters on the "Development of Motor Skills in the Preschool Years," and the "Development of Motor Skills in the Early School Years," will be appreciated by those who like every fact to be accompanied by an interesting problem yet to be investigated. The next chapter, "Motor Skills in Later Youth," appears perhaps as the most up-to-the-minute section in the book. Distinction is made between fine motor skills, gross motor activities, and so-called "basic motor capacities." The evidence seems quite clearly to favor the theory of specificity of skills.

"The Development of Language" is a brief, competent discussion of language development up to about school age; disorders of speech are also discussed, including their relation to lateral dominance.

The brief chapter on "Parental Education for the Training of Children" emphasizes the responsibility of the family physician in parental education, but neglects entirely to consider the rôle of visiting teacher or parent-teacher organizations. The same medical bias is evident in the chapter entitled "Medical Attitudes toward the Sexual Education of Children." The chapter on "Training in Habits" wisely cautions parents both against over-training the child into a robot, and under-training him into erratic irresponsibility. Many a mother will wonder at the implications of the statement, in the chapter on "Behavior in Normal Children and Adolescents," that "much of the mental invalidism of the world is simply persistent childhood behavior . . . childhood psychology is fundamentally related to psychiatry and mental hygiene."

The difficult problem of heredity vs. environment is reviewed in the chapter on "Education and Mental Development," with a conclusion which emphasizes educational and environmental possibilities.

The chapter on "The Relationship between Socio-Economic Status and Mental Traits" discusses methods of measuring socio-economic status, and its relation to intelligence, language development, and personality traits (particularly honesty). Personality is further considered in the chapter on "Personality Tests." Various conduct-tests and self-rating scales are listed, and relationships with age, sex, intelligence, and the home, are discussed. Measures of reputation are but briefly mentioned.

There are four additional chapters in the portion of the volume devoted to "Mental Status." One of the few cases of disproportion in treatment occurs in the chapter on "The Influence of Prematurity on Mental Growth;" two cases studied in the Yale Psycho-Clinic receive almost as much space as Mohr and Bartelme's extensive statistical study based on objective mental-test criteria! The conclusion is that prematurely born children develop normally in the onset of walking, the beginning of talking, in bladder control, and in intelligence, provided that allowance is made for the period of prematurity when computing the child's chronological age. The chapter on "The Relation of Mental and Physical Development" reports negative findings in general, but seems to consider that suitable composite measures of physical development, if treated by suitable statistical technique, might reveal a relationship. The chapter, "Nutrition as a Factor Influencing Mental Development" is largely inconclusive. The chapter on "Structural Disturbances of the Central Nervous System—very brief—observes that real progress in the knowledge of birth injury requires adequate post-mortem data, which "will not be collected with any eagerness until the present tendency to regard any suggestion of birth injury as an indictment of obstetrical method is abandoned." The chapter emphasizes the great importance of neurological analysis of disabilities, rather than mere description in terms of "syndromes."

It unfortunately lies beyond the scope of the present reviewer's knowledge to discuss the second section of this volume, on Physical Status. Quite striking, however, is the insistence on the value, both clinically and scientifically, of repeated measurements of the same individual.

On the whole, the volume is, in content, exceptionally accurate and authoritative. As a compendium of present information, the book will be most useful to both the general psychologist and the layman; as an indication of current trends, the book will be found invaluable by every serious student of child development.

Unfortunately the proof reading has been poorly done.

University of California HERBERT S. CONRAD

Change of Interest With Age. By EDWARD K. STRONG. Foreword by Walter R. Miles. Stanford University Press, 1931. Pp. xix, 235.

The author has given a meticulous clerical description of the responses of 2,340 men (384 engineers, 257 lawyers, 260 insurance men, 216 ministers, 411 physicians, 245 school men, 167 writers, and 400 Y.M.C.A. secretaries) in four age-groups (20-29 yr., 604; 30-39 yr., 759; 40-49 yr., 581; 50-59 yr., 396) to the 420 items on his "Vocational Interest Blank." That the results he describes have any causal relationship to age is in no sense demonstrated though Strong assumes that such is the case (pp. 20, 162). That self ratings of interests are not markedly different between men of 25 and 55 yr. within the same occupational group (p. 162) may mean: (1) that age is not a causal factor; (2) that different environmental backgrounds have obscured the influence of age (surely one cannot assume that environment has been the same or even remotely comparable for the men in any professional group born in 1872 and 1902); and (3) that self-ratings may describe the traditional pose of a group and may not indicate what the individual is actually like aside from the pose. Strong's treatment throughout assumes that the individual marking the "Interest Blank" not only knows what his attitudes are but is 100% honest in disclosing them—certainly an hazardous assumption. "The proverbial cautiousness of a banker is accompanied by less interest than the average in all forms of recklessness, whether physical, mental or moral" (p. ix). Banking procedures of the past decade or so should make psychologists skeptical of this self-evaluation.

That Strong is somewhat aware of the inadequacy of "control" in his investigation is shown by his statement: "The ideal procedure to follow in an age-interest study would be to determine the interests of a man at regular intervals throughout his life and base the results upon such records" (p. 15 and reiterated p. 163). This would, of course, be acceptable experimentation if one could control the environmental factors or determine their influence. Investigation without such control is comparable to estimating the amount of gas in a container without a knowledge of both temperature and pressure. Any determination of the influence of age upon mental processes awaits more careful experimentation, possible, than psychology is capable of at present. Medicine has found it necessary to revise many conclusions relative to the influence of age upon such objective matters as bone composition and blood pressure. These forced retractions should make psychology particularly wary.

Psychologists will have many minor criticisms of Strong's work: "As one does not long continue to like what one cannot do, it is only to be expected that a measurement of one's interests is approximately a measurement of what one can do" (p. 9). "Liking candy and disliking quinine are certainly innate qualities" (p. 9). "A decrease . . . amounting to 1.84 times the standard deviation of the difference is not what would be called a significant change" (p. 31). And so on.

Reports of investigations such as this illustrate the hazards of pioneering and emphasize the need of coöperatively projected long-time psychological studies. The continuity of the author's analysis of "Interest Blank" data, dating from the early post war period, illustrates one aspect of these much-needed investigations.

Quenemo, Kansas MILTON B. JENSEN

The Problem of Musical Expression. By ERICH SORANTIN. Nashville, Marshall & Bruce Co., 1932. Pp. vi, 123.

Perhaps the most important problem in musical aesthetics is the question of whether or not musical configurations actually represent or "express" specific feelings. The matter has been vigorously debated in musical and philosophical circles, with none of the protagonists showing a disposition to approach the problem empirically. Sorantin's study is the first systematic analysis of an extensive range of musical compositions, with a view to determining whether or not general types of musical patterns or figures are used to "express" specific feelings.

It was discovered that the musical structure of compositions designed to express general feeling and "volitional" states actually manifested "a very striking similarity." In general, two types of configurations are distinguished; namely the melodic and the rhythmic, according as the major psychological import of the figure is based upon the melodic or the rhythmic aspects of the tonal configuration. Rhythmic patterns, generally, were found to portray rhythmic movement rather "directly," on account of the rhythmic motor activity induced or suggested by the pattern of tones. The periodic repetition of a two-tone motif or figure representative of pendular motion (swinging of bells) is an example. More symbolic is the "descending melodic line with diminuendo dynamics and ritardando tempo" which has been used by several composers to represent the act of dying.

The melodic figures or patterns are perhaps of greater interest psychologically. A rather striking difference was noted between figures expressive of pleasant and of unpleasant feelings. The latter (sadness, grief, despair) were symbolized by "figures of lamentation" in which the chief musical characteristics were "ascending intervals of iambic, rhythmical character, connected by iterative structure with the following descending minor second mainly in the form of a suspension or appoggiatura." Figures symbolic of pleasant feelings were characterized by the "interval of the fourth in connection with the distinct appearance of the purposive melodic line, that is, the intentional striving towards a certain note as goal, which is apparently significant of volitional efficacy." Major tonality, forte dynamics, staccato phrasing and accelerated tempo, frequent recurrence of figures of circular movement, and the trill were other aspects of the figures portraying this general class of feelings. In addition to these rather definite and simple feelings of grief and joy, more complex states such as longing, love, etc. were found to have characteristic types of figures.

All of these analyses were based upon careful study of diverse musical composers ranging from the admittedly "expressive" music of Wagner to the presumably "formalistic" music of Bach. Some 240 examples are cited and many of them are reproduced in the text. No definite information is given as to the instances in which the figures symbolizing a given feeling or emotion (as indicated by the text, for example) may not have conformed to the typical figures. One gains the impression from the number, range and diversity of examples given that Sorantin's thesis is definitely established, although the statistically-minded individual might inquire about the bases of the selections and the extent (say in percentages) of the conformity of a random sampling of selections to his generalizations.

In general, the study may be regarded as a most important piece of pioneering research. Sorantin, with his unusual combination of musical and philosophical and psychological training, has been able to accomplish a type of empirical investigation of the first order. The work should be of great interest and significance to psychologists because it will suggest problems and supply materials for experimental investigations of the affective aspects of musical experience, in contrast to the somewhat limited and often superficial work on special aspects of "sensitivity" done thus far.

Vanderbilt University LYLE H. LANIER

The Laboratory Mouse: Its Origin, Heredity, and Culture. By CLYDE E. KEELER. Cambridge, Harvard University Press, 1931. Pp. viii, 81.

This is a collection of scientific data on the house mouse, *Mus musculus.* Its origin is traced to Central Asia in Eocene times, from which place and time it has become widely distributed, perhaps as a stowaway in early human migrations, and later in ships among merchandise. Different varieties are described and their respective geographical distributions indicated. Folklore and early historical accounts show that the mouse was well known to the ancients from the earliest times, and that both rats and mice abounded where grain was stored. The mouse, as well as the rat, has been associated with plagues, detested, regarded as the handiwork of devils, and feared, while the cat, as the natural enemy of the mouse has been deified, as in Egypt. But these respective rôles have not been universal: mice have also been worshipped, *e.g.* in Asia Minor as early as 1200 B.C., and the mouse-worship cult was popular in the time of Alexander the Great. White mice were cultured in temples. "Early Greek and Roman physicians employed mice in their medical formulae" (p. 14), mouse-blood before Hippocrates' day having been used as a cure for warts. Galen in the second century, A.D., advocated for the cure of cataract equal parts of mouse-blood, cock's gall, and women's milk mixed and dried.

But in the Orient the mouse was generally in better favor, and both the Chinese and the Japanese showed interest in collecting and breeding different varieties. The waltzing variety has been known in China since 80 B.C. Over a hundred years ago fancy varieties of the house mouse were taken to Europe by British traders, and "only a few decades ago did muriculture spread to America."

The author devotes a chapter to the description of the various unit-characters (gene mutations) as they have been developed in the light of Mendel's law of inheritance, giving the fanciers descriptive terms, the scientific terms, and genetic formula in a full-page table. A well-illustrated and clearly written chapter is given over to normal inheritance of the mouse as now known, including a diagram of chromosomes with genes for unit-characters distributed arbitrarily among them to illustrate either genetic independence or linkage of the unit-characters. Certain abnormal features of inheritance, such as dominant lethals (or types which cannot live), and hereditary sterility, are also described and discussed. The final chapter covers in rather thorough manner the practical matters associated with the breeding of mice in laboratories. There are 21 very good half-tone pictures of different varieties of mice, and a selected bibliography of 184 titles. The omission of a good index is unfortunate, but is partly compensated for by a rather complete table of illustrations. The book is valuable to comparative psychologists.

J. P.

Acquisition of Motor Skill in Young Children. By JAMES ALLEN HICKS. University of Iowa Studies: Studies in Child Welfare, Vol. 4, No. 5. Iowa City, The University Bureau of Publ., 1931. Pp. 80.

This monograph reports the results of an investigation made during the author's incumbency as a National Research Council Fellow during the years 1927-1929.

Sixty children, aged 3-6 yr., were divided into experimental and control groups of 30 each. The learning task was to hit a moving target—a 6-in. bull's eye in a 4-ft. circular—with a standard squash ball. The angular velocity of the moving target was 0.3 ft. per sec. The target was 5 ft. from the thrower.

The practice group had an initial test of 10 throws, then one practice session of 10 throws weekly for 8 weeks, and two final test sessions—a total of 110 throws for each child. The control children threw 30 times, 10 at the beginning and 20 at the end of the 10 weeks of experimentation. Thus the controls had about one-fourth the total number of throws of the practice group.

In addition to the target a strength of arm and shoulder test, a perforation test (punching holes in a sheet of paper), and a test of tracing between two irregular lines were given.

In the target experiment 17 of the 30 children of the practice group gained and 13 lost in score from the initial to the final tests. In the control group 16 of the 30 children gained, 13 lost, and 1 showed no change in score.

The author and the writer of the foreword (G. D. Stoddard), conclude that "there were gains due to maturation in three of the four motor tests used, and specific practice could account for but little of the improvement." Stoddard believes that this work is "another link in the growing chain of evidence as to the primary rôle of physical maturation (as exemplified by time elapsed under normal environmental conditions) in the early development of complex motor skills."

The reviewer holds that such conclusions are not justified by the data printed in this monograph. The children were under experimental observation for less than 90 days; the range of ages studied was not broad enough to furnish convincing data on any question of growth, differentiation or de-differentiation; maturation, as contrasted with 'learning' is a highly questionable biological concept, and the author's implied distinction is nowhere defined or made clear; inferences drawn from the comparison of an experimental and a so-called control group, particularly under such limited training, are of doubtful validity; and finally the actual results show essentially no difference in the relative accomplishments of the two groups. The author cites this fact (pp. 38-39) by his comparisons of the initial and final critical ratios.

The target apparatus is well designed and should prove useful for still more extended studies on the problem. There is a bibliography of 41 titles.

Ohio State University SAMUEL S. RENSHAW

Accidents, Neuroses and Compensation. By JAMES H. HUDDLESON. Baltimore, Williams & Wilkins Co., 1932. Pp. ix, 256.

The author intended his book primarily for the medical profession. It evidences an imposing review of the literature in the field and some acquaintance with psychology. The stifling quotation of "authority" will scarcely appeal to psychologists. Its array of psychological theory is of marked service to psychology, first from the standpoint of compilation, and second, as an example of how the theory of a science is often accepted as fact by those not intimately associated with that science. The complete acceptance of the "functional" explanation of disorder comes at a time when psychologists themselves (many of whom proselyted the doctrine) are looking askance at what now seems too simple an explanation. Science in general and psychology in particular may well be hesitant of so complete an avowal as that of Huddleson: "A *traumatic neurosis* may be defined most simply as a psychogenic or a nonstructural nervous disorder, shortly following a physical injury, and complicated or not by structural change in the central nervous system or elsewhere" (p. 1). Such a definition implies a disregard of the limitations of diagnostic technique. The fact that present methods have found no structural basis for the so-called "functional" disorders is scant justification for concluding that this will always be the case. Psychologists will find fault with the author's categorical treatment of behavior, clearly shown to be gradient instead. Psychosis and neurosis are so separated by definition (p. 3) and by treatment throughout the book.

The abundance and accuracy of citation are highly commendable. Whether the materials presented justify so many pages may be questioned and why the material is organized around twelve rather than fewer chapters is somewhat bewildering. That Chapter III on "Etiologies" merits the 38 pages accorded it seems particularly questionable in view of etiological discussion running through practically the entire 216 pages of the book proper. Insistence upon a common sense treatment of neuroses is an outstanding feature of the entire treatise. Perhaps the major contribution of the book is the finality with which it shows how little is actually known of neuroses, either as to etiology, diagnosis, prophylaxis, or treatment.

Quenemo, Kansas MILTON B. JENSEN

The Genesis and Constancy of Ascendance and Submission as Personality Traits. By Sister MARY AQUINAS MCLAUGHLIN. University of Iowa Studies in Education, N.S., No. 216. Iowa City, University of Iowa Press, 1931. Pp. 95.

From a population of 444 students to whom the A-S Reaction Study had been given, the investigator selected certain cases whose scores deviated approximately 2 sigma from the mean. With the aid of this and an additional criterion of ratings a group of 12 markedly ascendant subjects and 13 markedly submissive subjects was selected for intensive study. The investigator then undertook a process of reëducation to determine to what extent the traits could be modified. "Frequent personal contacts afforded by weekly interviews gave opportunity for detecting erroneous concepts and substituting others more adequate for social adjustment."

At the close of the period of reëducation it was found by the criterion of ratings that 12 of the 13 submissive subjects had changed in the desired direction (*i.e.* had become more ascendant), whereas only 5 of the 12 ascendant cases had become more submissive. "The greater tension produced in submissive subjects because of unfavorable adjustment made them more susceptible to reëducation." The author lists certain causes of the traits in question and offers representative case-studies. She believes that a modification of the traits was brought about more rapidly "when the environmental factors operative in building up either ascendance or submission, were ascertainable."

The author assumes that an extreme degree of either trait is *ipso facto* undesirable.

Had she reported more fully upon the tension and disturbance which these traits caused in the individual subjects it would perhaps be possible to hazard an explanation for her interesting discovery that ascendance is less modifiable than submission. At present it is not clear whether ascendant subjects are merely more satisfied with themselves, or whether in an interview they are unable to yield to the counsellor and to accept her suggestions, or whether ascendance is the more "normal" phase of personality and when once developed resists modification.

Harvard University GORDON W. ALLPORT

Reflex Activity of the Spinal Cord. By R. S. CREED, D. DENNY-BROWN, J. C. ECCLES, E. G. T. LIDDELL, and C. S. SHERRINGTON. Oxford, The Clarendon Press, 1932. Pp. viii, 183.

Using the individual "motor unit" as the functional element of reflex response, the authors have been able to obtain accurate descriptions of reflex "time relations" and "reflex quantities of effect" as expressed in both mechanical and electrical changes. This motor unit consists of the axone of a nerve cell together with all the muscle-fibers which its many branches innervate. It makes possible a quantitative statement of the changes occurring in such conditions as fractionation, convergence, and occlusion which have proved extremely useful in accounting for such characteristics as facilitation and inhibition.

The book is not an account of the numerous investigations on reflexes of its authors. Rather it represents a complete summary, with a critical discussion, of all the important experimental work of the past thirty years. As indicated in the title, the book deals only with reflex responses manifest in preparations in which the intact central nervous system does not extend further headward than the corpora quadrigemina. The general object is to give a concise account of the mechanism of reflex behavior as found in the mammalian cord. Discussion of such topics as the "biological significance" of reflexes or reflexes as "items in animal behavior or conduct" have been purposely omitted. Such discussion would have materially increased the value of the book for psychologists. However, those espousing the principle of the conditioned response, or supporting the view that the complex acts in behavior are traceable to a synthesis of more elementary reflex units, will find the book extremely helpful in unraveling the physiological processes involved in supposedly simple reflex activity.

The first two chapters present the characteristics of the reflex arc and the properties of the spinal grey matter. In chapter three, the flexor reflex is used in describing such phenomena as the latent period, reflex tetanus, fractionation, convergence, occlusion, summation, refractory period, synaptic delay and after discharge. The next chapter deals with the stretch reflex and includes studies on the tension curve, the tendon jerk, action currents, lengthening and shortening reactions, and the action of red and white muscles. In a discussion of the crossed and ipselateral extension reflexes such factors as de-afferentation, blending, rebound, tetanic and single shock stimulation and latent successive induction are considered. Chapter six gives a very complete account of the phenomena observed in central inhibition. It contains accurate descriptions of the changes occurring in the central excitatory state and the central inhibitory state. A discussion of reflex coördination is given in the last chapter. It includes such topics as concealed reflexes, types of rebound, quantitative adjustment of reflex contraction, reciprocal innervation, speed of movement, etc. In concluding remarks the authors present a general principle which seems to emerge from their analysis of simple reflexes. It is that "the quantity of contraction presents itself as the resultant commonly of two interacting antagonistic central processes, excitation and inhibition." There are two appendices, a very complete bibliography of 209 references, and an index.

University of California C. W. BROWN

A Field Study of the Chimpanzee: Observations of Chimpanzee Behavior and Environment in Western French Guinea. By HENRY W. NISSEN. Foreword by Robert M. Yerkes. Comparative Psychology Monographs, 8, 1931, (no. 1). Pp. vi, 122, 41 pl.

This account of chimpanzees in their natural habitat, the first study of its kind to be made by a psychobiologist, should prove almost as valuable to psychologists generally as to those interested primarily in animal behavior. Of necessity descriptive in emphasis, and admittedly an informational survey, the monograph nevertheless challenges many existing interpretations of anthropoid learning and social organization. It would appear, for example, that adaptation to the *Umwege* situation is not as detached from previous experience as is assumed by those who postulate the concept of insight.

The author, happily for his readers, has presented quotations from original notes and summaries of observations under the following broad but logically determined categories: social organization and physical appearance; nomadism, locomotion, and activity distribution; nests and nesting; food and feeding; and social behavior. Under the last mentioned category are included discussions of group leadership, play responses, contacts with other animals, reactions of chimpanzees to human behavior, and discussions of sound production and communication. Supplementing these observations and summaries, the reader will find interpretations which carry weight because they give résumés of the facts available and because they enjoy freedom from dogmatic assertion and anthropomorphic bias.

Since the investigator has recognized frankly the limitations of his study, he qualifies as the best critic. Undoubtedly future field studies will profit from this pioneer research and will in turn modify certain of its hypotheses. Be that as it may, for the present Nissen has presented to his colleagues an excellent account of the chimpanzee in non-laboratory situations.

University of Minnesota CHARLES BIRD

Psychiatry and Mental Health. By JOHN RATHBONE OLIVER. New York, Charles Scribner's Sons, 1932. Pp. 330.

The substance of this book was presented in the Hale Lectures in 1932 at the Western Theological Seminary with the purpose of orienting religious and social workers to the nature and peculiarities of abnormal people. The author has succeeded in producing a readable book, which may prove valuable to workers lacking psychiatric preparation. The appeal of the book as a whole is limited by the religious presuppositions of the author.

The cases appended to each chapter are clearly illustrative and not typical. The bibliography is surprisingly inadequate so far as psychiatry is concerned.

Trinity College ROBT. B. W. HUTT

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By J. P. GUILFORD, University of Nebraska

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